On Developing the Implicit Relational Assessment Procedure (IRAP) as a Tool for Quantifying Tobacco Addiction

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Table of Contents

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Abstract

The present research aimed to develop the implicit relational assessment procedure (IRAP) as a means of quantifying tobacco addiction in terms of implicit (i.e. automatic) evaluative processes. Chapter 1 begins by highlighting the fundamental importance of such processes to the losses of personal autonomy which characterise tobacco addiction. Accordingly, thereafter, Chapters 1-3 involve critically reviewing all major measures of addiction-related implicit processes in terms of their respective abilities to distinguish between implicit evaluating of one topic as distinct from another. The review culminates by recommending the IRAP, and its behaviour analytic rationale, as a tool for both functional and cognitive theorizing about tobacco addiction. Chapter 4 reports a study showing that the IRAP compares favourably with the most popular implicit measurement tool, the implicit association test (IAT), in terms of its ability to validate against multiple defining features of tobacco addiction. Indeed, this was despite the fact that both implicit measures were focused on evaluative topics specifically designed to favour the IAT rather than the IRAP; and also despite the fact that as a result the relevant IAT convincingly outperformed all of its predecessors in the literature. In response, Chapter 5 describes research that explored the potential of the IRAP to target complex, mood-conditional aspects of smokers' implicit reasons for smoking that were unavailable to the IAT, or any other implicit measure. Crucially, by revealing motivational distinctions that were simply not available using other existing measures of implicit cognition, the IRAP's experimental precision allowed us to identify aspects of implicit evaluating with unprecedented levels of criterion validity in relation to tobacco addiction. For example, these findings suggested a preliminary functional model wherein tobacco addiction is motivated by complex, coordinated and mood-dependent networks of implicit evaluative processes which collectively insist that one should regulate one's ongoing emotional experiences – particularly negative craving-related affect – by smoking. Extending this model, the three experimental studies reported in Chapters 6 and 7 confirmed that smokers' most popular, and also least successful, method of managing their (implicit) tobacco cravings is ultimately self-defeating insofar as it involves experientially avoidant tactics like thought suppression. Accordingly, Chapter 8 concludes by recommending the IRAP, with certain important qualifications, as a useful means of quantifying tobacco addiction in such a way as to begin clarifying the motivational problem(s) that smoking-cessation treatments must ultimately tackle.

CHAPTER 1: A Review of Foundational Rationales and Early Approaches for Examining Tobacco Addiction in terms of Implicit Cognition

1.1. Defining the Problem of Tobacco Addiction

Tobacco smoking is one of the single most self-destructive and yet persistently popular pastimes of our era. As a testament to this troubling contradiction, most regular smokers not only recognize that smoking undermines the length and quality of their lives, but many also recognize that it harms even non-smokers (i.e. CDC, 2002, 2008; Eurobarometer, 2006, p. 53, 2012, p.101; Koh, Alpert, Judge, Caughey, Elqura, Connolly, & Warren, 2011; Stuber, Galea, & Link, 2008; Wilson, Creswell, Sayette, & Fiez, 2013; World Health Organization, 2011). Of the roughly one quarter to one third of regular smokers that populate developed countries most report that they would like to quit smoking (i.e. if they felt able to; see CDC, 2002, 2008; Eurobarometer, 2006, p. 52, 2012, p. 99; Fong, Hammond, Laux, Zanna, Cummings, Borland, & Ross, 2004; Peterson, Vander, & Jaén, 2011, p. 4; U.S. Department of Health and Human Services, 2012; West, 2004; Wilson et al., 2013; World Health Organization, 2011; Zatoński, Przewoźniak, Sulkowska, West, & Wojtyła, 2012); and yet, those one in three or four people continue to smoke.[1]

Irrespective of the many longstanding theoretical controversies concerning the minutiae of what defines tobacco addiction, this is in essence the core puzzle at its heart: that so many smokers appear unable to stop themselves from regularly smoking even though they are very clearly often desperate to do so (see Chassin, Presson, Rose & Sherman, 2007; Conklin, Clayton, Tiffany & Shiffman, 2004; DiFranza, 2010; DiFranza, Savageau, Fletcher, Ockene, Rigotti, McNeill, Coleman, & Wood, 2002; DiFranza, Ursprung, Lauzon, Bancej, Wellman, Ziedonis et al., 2010; DiFranza, Wellman, Mermelstein, Pbert, Klein, Sargent, 2011; Donovan & Marlatt, 2005, pp. 1-49; Edwards, 2012; Heyman, 2009, 2011, 2013; Hughes, Keely, & Naud, 2004; Köpetz, Lejuez, Wiers, & Kruglanski, 2013; Piasecki, Piper, & Baker, 2010; Scragg, Wellman, Laugesen, & DiFranza, 2008; Ursprung & DiFranza, 2010; Wellman, DiFranza, Pbert, Fletcher, Flint, Young, & Draker, 2006, p. 494; West, 2001, 2006). For example, among the most popular ways of defining tobacco addiction is in terms either of nicotine providing irresistible

[1] For brevity any mention of smoking or smokers shall henceforth refer exclusively to tobacco smoking unless otherwise specified.

emotional relief (e.g. Baker, Piper, McCarthy, Majeskie, & Fiore, 2004; McCallion & Zvolensky, 2015; Tiffany, Conklin, Shiffman, & Clayton, 2004), providing irresistible reward (e.g. Everitt & Robins, 2005), or indeed in terms of hijacking motivational brain systems that make its use irresistible regardless of hedonia (e.g. Robinson & Berridge, 1993, 2003). Thus, it could be argued that when the term *tobacco addiction* is stripped back to its raison d'etre it is fundamentally conveying a problem of diminished personal autonomy; and one which ultimately constitutes the single greatest premature cause of death among humans to date (see CDC, 2002, 2008; U.S. Department of Health and Human Services, 2012; World Health Organization, 2011). Exploring sources for this pivotal loss of personal autonomy is the focus of the current thesis.

The general approach will involve developing and testing measures of automatic cognitive biases, usually termed *implicit evaluations or cognitions*, that relate to smoking (De Houwer, 2006, 2011; De Houwer, Barnes-Holmes & Moors, 2013; De Houwer & Moors, 2012; Gawronski & Creighton, 2013). In particular, we[2] will consider how a relatively new measure of implicit cognition, the Implicit Relational Assessment Procedure (IRAP; Barnes-Holmes, Barnes-Holmes, Power, Hayden, Milne & Stewart, 2006) may be used to build upon the capabilities of its predecessors in terms of measuring smoking-related implicit cognition. By experimenting with smoking-related IRAP scores the following research thus seeks to clarify two major issues about tobacco addiction: why smokers continue to smoke even when they dislike doing so; and also to explore how smoking-related implicit cognition relates to classic formulations of self-control involving deliberate suppression of tobacco cravings. In so doing, we hope to provide some theoretical clarification as to what extent tobacco addiction manifests in implicit evaluating in terms of emotional relief versus reward.

1.2. Why Research on Implicit Cognition is required to solve the Problem of Diminished Autonomy at the Heart of Tobacco Addiction

In keeping with western peoples' strong tendency to embrace the age-old libertarian idea of 'free-will' (see Acton, 1922/2007; Augustinos, Walker, & Donaghue, 2006; Baer, Kaufman, & Baumeister, 2008; Balaguer, 2012; Dunning, 2013; Nichols, 2011; Malle, 2004; Nichols & Knobe, 2007; Sarkissian, Chetterjee, De Brigard, Knobe, Nichols, &

[2] For purely stylistic reasons the collective pronoun "we" will henceforth be used instead of the personal pronoun "I" even though the work presented herein was conducted solely by the present doctoral candidate.

Schlinger, 2005; Schwartz, 2004; Shariff, Greene, Karremans, Luguri, Clark, Schooler, Baumeister, & Vohs, 2014; Viney, 1969), *psychological* theories of (tobacco) addiction are traditionally built upon the assumption that humans make choices using only rational and introspectively accessible mental processes (see Abrams, Niaura, Brown, Emmons, Goldstein, & Monti, 2003; Ajzen, 1988; Ajzen & Fishbein, 2000; Bargh & Ferguson, 2000; Becker, 1974; Brandon, Herzog, Irvin, & Gwaltney, 2004; Conklin et al., 2004; DiFranza, 2010; DiFranza et al., 2010; Donovan & Marlatt, 2005; Fishbein & Ajzen, 1975; Heyman, 2009, 2011, 2013; Köpetz et al., 2013; Peterson et al., 2011; Rogers, 1983; Rooke, Hine, & Thorsteinsson, 2008; Shadel, Shiffman, Niaura, Nichter, & Abrams, 2000; Tiffany, Conklin, Shiffman, & Clayton, 2004; West, 2001, 2006; Wiers & de Jong, 2007; Wiers et al., 2010). From this *rationalistic* point of view human behaviour is assumed, a priori, to be mediated exclusively by mental calculations that involve deliberately and systematically "working out" whether the benefits of some behaviour or action are likely to outweigh its costs.

At first glance this mainstream, rationalistic way of understanding human thinking may seem very plausible in that it certainly comports well with the ongoing personal experience of observing oneself thinking things through to come to (seemingly) reasonable judgements and decisions about the things that matter to us. Indeed, it is an empirical fact that humans with the ability to notice and describe their thinking normally strive to resolve any inconsistencies that they happen to notice within that thinking and/or their behaviour more broadly (see Barnes-Holmes, Hayes, & Dymond, 2001; Bem, 1967, 1972; Bordieri, Kellum, & Wilson, 2013; Cooper & Carlsmith, 2001; Crisp & Turner, 2010, pp. 114; Fazio, Zanna, & Cooper, 1977; Festinger, 1957; Gawronski, 2012; Harmon-Jones, 2012; Heintzelman, Trent, & King, 2013; Nickerson, 1998; Skinner, 1989; Wray, 2011; Wray, Dougher, Hamilton, & Guinther, 2012). Crucially, however, it is also a well-established empirical fact that human behaviour is frequently influenced in ways that are not exclusively governed by rational and/or introspectively accessible processes (e.g. Chiesa, 1994; Greenspoon, 1955; James, 1890, 1892; Kahneman, 2011; Kihlstrom, 2008; Moore, 2003, 2010, 2011; Proctor & Capaldi, 2012; Skinner, 1945; 1953; 1974; Stacy & Wiers, 2010, pp.554-555; Verplanck, 1955). Specifically, psychologists have long noted that what people claim to know about their own psychology can vary in its accuracy in many contradictory ways depending upon context.

Firstly, people often fail to correctly self-report the source of contrasting experimental effects upon their behaviour (see Dunning, 2013; Jones & Harris, 1967; Jones & Nisbett, 1971; Kahneman, 2011; Nisbett & Wilson, 1977; Roediger, 2003; Schiffrin & Schneider, 1977; Uziel, 2010; Wilson, 2009). Secondly, self-reports are themselves susceptible to many common confounding contextual effects (e.g. demand characteristics, and/or more broadly, to the context-specific cuing of irrelevant but highly rehearsed ways of thinking and acting; see Bilalić, McLeod & Gobet, 2008; Cronbach, 1990; Dane, 2011; Dougherty & Johnston, 1996; Everitt & Robbins, 2005; Haas & Hayes, 2006; Hayes, 1989; James, 1890, 1892; Kahneman, 2011; Kendrick & Olson, 2012; Lally & Gardner, 2013; Lelkes, Krosnick, Marx, Judd, & Park, 2012; Nichols & Maner, 2008; Nickerson, 1998; Nisbett & Wilson, 1977; Roediger, 2003; Rugg & D'Agnese, 2013; Schiffrin & Schneider, 1977; Schwarz, 2008; Verplanck, 1955; Tiffany, 1990; Wilson & Schooler, 1991; Wilson 2009; Wood & Neal, 2007; Wulfert, Greenway, Farkas, Hayes, & Dougher, 1994). Thirdly, self-reports are prone to a large variety of trait-like cognitive biases that mislead human reasoning in general (i.e. philosophers call these biases *logical fallacies*; for popular reviews see Ariely, 2008; Kahneman, 2011).[3] Fourthly, even if we assume that people can sometimes manage to introspect accurately, the relevant self-reports would still be prone to distortions of social desirability wherein people seek to conform what they say with what they perceive to be the prevailing social norms of the relevant audience (e.g. Bem, 1967, 1972; Fazio, Zanna, & Cooper, 1977; Festinger, 1957; Gawronski, 2012; Holtgraves, 2004; Paulhus, 1989; Uziel, 2010).[4] The cumulative upshot of these various distortions to which self-reports are prone is that many aspects of behaviour do not operate in full accordance with the subjective rationality of self-reports. Rationalistic theories, derived as they are from questionnaire-based self-report methodologies, have therefore been largely unsuccessful at predicting irrational behaviours even cross-sectionally (e.g. Abraham, Connor, Jones, & O'Conner, 2008, pp.148-155; Ajzen & Fishbein, 2000, pp. 16-26; Armitage & Connor, 2001; Connor & Sparks, 2002; Dunning, 2013; Orbell & Sheeran,

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[3] In fact, some of these popular logical fallacies specifically hide inconsistencies from introspection thus directly bolstering rationalistic illusions even further (e.g. confirmation, einstellung and attribution biases; see Crisp & Turner, 2010, pp. 42-120; Dane, 2011; Jones & Harris, 1967; Jones & Nisbett, 1971; Malle, 2004, 2006, 2011; Malle, Knobe, & Nelson, 2007; Mezulis, Abramson, Hyde, & Hankin, 2004; Nickerson, 1998; Proctor & Capaldi, 2012; Rugg & D'Agnese, 2013; Stanovich, West, & Toplak, 2013).

[4] To complicate matters even further peoples' self-reports sometimes polarize against social norms that they find threatening or otherwise unpalatable (see Hayes, Wilson, Gifford, Follette, & Strosahl, 1996; Legault, Gutsell, & Inzlicht, 2011; Lewandowsky, Ecker, Seifert, Schwarz, & Cook, 2012; Nestler & Egloff, 2010; Rhodes, Roskos-Ewoldsen, Edison, & Bradford, 2008; Wood, 2000).

1998; Schwarz, 1999, 2008; Schwarz & Oyserman, 2001; Sheeran, 2002; Stacy, Bentler, & Flay, 1994; Topa & Moriano, 2010; Verplanken & Aarts, 1999; Webb & Sheeran, 2006; Wiers et al., 2010, pp. 464-465).

Crucially, the tobacco addict's thinking about smoking is a classic microcosm of the foregoing duality between rationality versus irrationality. On the one hand, even the most addicted smokers retain some degree of rationalistic insight and discretion about their smoking: whether they postpone their cigarette for a short time in the service of waiting for their coffee to brew, or whether they postpone it much longer in the service of taking a plane journey to somewhere they would like to visit (see DiFranza et al., 2007; DiFranza et al., 2012; Wilson et al., 2013). And yet at other times, the addicted smoker's thinking is characteristically conflicted, fluctuating irrationally between wanting to smoke and wanting to quit smoking, and such that even those smokers who report disliking smoking do not know how to stop themselves from continuing to do so (e.g. Chassin et al., 2007; DiFranza, Ursprung, & Biller, 2012; DiFranza, Ursprung & Carson, 2010; Lipkus, Green, Feaganes, & Sedikides, 2001; Sayette, Loewenstein, Griffin, & Black, 2008; Shadel et al., 2000; Webb & Sheeran, 2006, p. 254; Wilson et al., 2013). Thus, despite the ability of all tobacco addicts to at least temporarily maintain rational consistency in their thinking about smoking, when considered in its defining contexts tobacco addiction is a psychological condition that appears to be inherently irrational[5] and also elusive to introspection.

Bearing out the mismatch between smoking-related explicit self-reports and the irrational often stigmatized nature of tobacco addiction, changes in self-reported beliefs, smoking-related attitudes and/or intentions are generally poor predictors of changes in smoking behaviour. If anything, changes in self-reported attitudes and intentions about smoking tend to follow changes in smoking-related behaviour, and even then the resulting post hoc self-reports are usually only weakly related to smoking behaviours (see Baldwin, Rothman, Hertel, Linde, Jeffrey, Finch & Lando, 2006; Brandon, Juliano, & Copeland, 1999; de Leeuw, Engels, Vermulst, & Scholte, 2008; Hendricks, Prochaska, Humfleet, & Hall, 2008; DiFranza, 2010; Donny & Dierker, 2007; Gifford & Humphreys, 2007; Gwaltney, Metrik, Kahler & Shiffman, 2009; Hertel, Finch, Kelly, King, Lando, Linde et al., 2008; Hughes, 2007a; Lipkus et al., 2001; Palfai, 2002, p. 318; Piper, McCarthy, &

[5] That is, irrational in the sense of the cognitive (in)consistency of its constituent processes, rather than irrational in any moral sense, or indeed in any sense that it cannot in principle be scientifically understood in terms of experimental variables.

Baker, 2006; Stacy et al., 1994; Tiffany et al., 2004; Topa & Moriano, 2010; Webb & Sheeran, 2006, p. 254).[6]

Nonetheless, without viable alternatives, psychological treatments of smoking-cessation traditionally resort to self-reports for empirical guidance. This has contributed to a largely hit-or-miss approach to treatment discovery wherein even the best smoking-cessation interventions stagnate at disappointingly low 10-30% six-month smoking-abstinence rates. In particular, one of the most frequently cited reasons for this clinical stagnation is the lack of treatment-specific psychological measurement effects to target for systematic development (see Abrams et al., 2003, pp. 21-23; Borland, Partos & Cummings, 2012, 2013; Chapman & McKenzie, 2010, 2013; Dawkins, 2013; Fiore, Jaén, Baker, Bailey, Benowitz, Curry et al., 2008; Kapson, Leddy, & Haaga, 2012; Kazdin, 2007; Gifford & Humphreys, 2007; Hughes, 2008; Hughes, Rennard, Fingar, et al., 2011; Lancaster & Stead, 2005; Levin, Hildebrandt, Lillis, & Hayes, 2011; Messer & Pierce, 2013; Murphy, Cooper, Hollon, & Fairburn, 2009; Peterson et al., 2011, pp. 49-57; Piasecki et al., 2010; Roefs, Huijding, Smulders, MacLeod, de Jong, Wiers, & Jansen, 2011; Rooke et al., 2008, p. 1324; Rosen & Davison, 2003; Tønnesen, 2009; Walsh, 2008; Webb & Sheeran, 2006, p. 254).

At this point, therefore, we can safely conclude that a sole reliance on traditional questionnaire-based self-report measures is not sufficient to provide a complete picture of the psychological causes of tobacco addiction, and worse still, such practices thus limit smoking-cessation treatments (see Abrams et al., 2003, pp. xiii-xiv; Croyle & Backinger, 2008; DiFranza, 2010; Donovan & Marlatt, 2005, pp. 114-115; Gifford & Humphreys, 2007, pp. 356-358; Tiffany, 2008; Tiffany, et al., 2004; Tønnesen, 2009, p. S22; West, 2001, 2004, 2006; Wiers & de Jong, 2007; Wiers et al., 2010; Wray et al., 2013). The lost autonomy which defines tobacco addiction is instead an inherently irrational process that defies self-report; and so the only effective way of understanding tobacco addiction is to employ measures that capture irrational cognitive processes per se. In principle, measures of implicit cognition are uniquely suited to the task because by definition irrational

[6] Although self-reported tobacco cravings sometimes co-occur with the onset of heavier patterns of smoking and/or moderately predict relapse during abstinence, they are generally poor predictors of changes in future patterns of smoking (DiFranza, 2009, 2010; DiFranza et al., 2012; DiFranza et al., 2011; Erblich & Montgomery, 2012; Hughes, 2007a, 2007b; MacKillop, Brown, Stojek, Murphy, Sweet, & Niaura, 2012; Piasecki et al., 2010; Sayette et al., 2008; Shiffman, West, & Gilbert, 2004; Tiffany et al., 2004; Tiffany, 2008; Wray, Gass, & Tiffany, 2013).

cognition involves inadvertent, and thus automatic, contradictory changes in cognition (see De Houwer, Teige-Mocigemba, Spruyt, & Moors, 2009a, 2009b; Roefs et al., 2011; Wiers & de Jong, 2007; Wiers et al., 2010). For example, implicit measures might be used to quantify the propensity of pro-smoking evaluating to *automatically* interrupt and thus contradict corresponding anti-smoking evaluating (or vice versa). Therefore, what follows in the next section, and over the course of the following two chapters are a series of systematic reviews critiquing each of the most popular measures of implicit cognition relative to our intended task of improving experimental understanding of tobacco addiction processes. In providing this review, we ultimately hope to make it clear why the IRAP is uniquely equipped to solve a range of critical impasses in the literature on smoking-related implicit cognition.

1.3. The First Wave of Research on Smoking-related Implicit Cognition: A Comprehensive Review of Attention- and Cue-based Measures

At first, and for the longest time, people could only detect irrational processes in terms of the inadvertent occurrence of contradictions among self-reports. Among the earliest measures of addiction to specifically target implicit styles of evaluation per se (i.e. tasks that orchestrate fast, efficient, introspectively-inaccessible and/or unintentional evaluative responses; see De Houwer & Moors, 2012; De Houwer et al., 2009a, 2009b), were those that attempted to capture automatic attention and/or distraction processes (see Bruce & Jones, 2006; Payne & Gawronski, 2010; Tiffany, 1990, pp. 159-161).

Parsing Tobacco Addiction in Terms of Implicit Evaluating: A Systematic Review of Attention-based Measures

There are many different varieties of attention measure, but in broad methodological terms they all measure cued cognitive consumption by gauging the extent to which an individual does or does not attend to particular addiction-related versus control stimuli (for comprehensive reviews and meta-analyses of addiction-related attention research see Bruce & Jones, 2006; Field, 2010; Field & Cox, 2008; Field, Mogg, & Bradley, 2006; Field, Munafó, & Franken, 2009; Rooke et al., 2008). Attention-based measures have perhaps been most useful in highlighting how addiction-related stimuli and contexts can automatically monopolise cognition to varying degrees depending upon various background motivational factors (e.g. such as how self-reported goals, emotions or expectations can moderate which aspects of the ongoing context capture cognition; see Becker, Folk, & Remington, 2013; Drew, Võ, & & Wolfe, 2013; Eitam, Yeshurun, & Hassan, 2013; Harmon-Jones, Gable, & Price, 2013; Huntsinger, 2013; Rensink, O'Regan, & Clark, 1997; Vogt, De Houwer, Crombez, & Van Damme, 2013; Zhao, Al-Aidroos, & Turk-Browne, 2013).

Nevertheless, attention measures are severely limited in one important way; although they can indicate that a stimulus is (or is not) attended to, they are silent with respect to whether that stimulus was evaluated positively, negatively, or indeed ambivalently (see Bruce & Jones, 2006; Fox, Russo, Bowles, & Dutton, 2001; Field & Cox, 2008; Field, Munafó, & Franken, 2009; Shomstein & Johnson, 2013). Thus, a researcher using attention-based measures could conclude that a smoker attends to a picture of a lit cigarette to a greater extent than a non-smoker, but the reason behind the difference in attention would remain unclear. Intuitively, one might assume that smokers will attend more to such pictures because they like smoking, but attention-based measures provide nothing to support or contradict this assumption – they simply reveal greater or lesser levels of attention (see Field & Cox, 2008; Field et al., 2009).

To illustrate, consider the *addiction Stroop* which is by far the most frequently used measure of addiction-related attentional bias. It is a modified version of the classic Stroop paradigm (Stroop, 1935) that requires participants to name the varying colours of neutral versus addiction-related words as quickly and as accurately as possible (see Cox, Fadardi, & Pothos, 2006). The basic rationale here is that when addiction-related words capture a participant's attention it will selectively interfere with, and thus delay, the colour naming of addiction-related words relative to words that are assumed to be motivationally neutral. Thus, the longer it takes a participant to name the colour of the addiction-related words relative to the neutral words (i.e. the *addiction Stroop effect*) the more it indicates attentional bias towards the addiction-related stimuli (see Cox, Fadardi, & Pothos, 2006; Field & Cox, 2008; Williams, Mathews, & MacLeod, 1996). However, in and of itself, an addiction Stroop effect tells us nothing about any positive and/or negative evaluations that might have been involved in attending to those addiction-related stimuli (see Algom, Chajut & Lev, 2004; Bradley, Field, Healy & Mogg, 2008; Bruce & Jones, 2006; De Houwer, 2003a; Fox et al., 2001; Frings, Englert, Wentura, & Bermeitinger, 2010, p. 47). As noted above, if smokers showed a Stroop effect for smoking-related words, one might intuitively assume that they did so because they like smoking; and yet, smokers may equally have shown this same attentional bias because they temporarily evaluated smoking negatively in the relevant context (e.g., many addicted smokers sometimes think of smoking as being a life-threatening habit that they cannot control; Baldwin et al., 2006; Bradley et al., 2008; Heyman, 2013; Lipkus et al., 2001).

Another related problem is that measures of smoking-related attention have rather poor internal reliability (i.e. visual probe tasks and modified-Stroops related to substance abuse, respectively, exhibited *N*-weighted average Cronbach's *α*s of just .14 and .46; see Ataya, Adams, Mullings, Cooper, Attwood, & Munafó, 2012a, 2012b; Spiegelhalder, Jähne, Kyle, Beil, Doll, Feige & Riemann, 2011; for even worse findings in other domains see also Cisler, Bacon, & Williams, 2009; LeBel & Paunonen, 2011, p. 572; Schmukle, 2005). Furthermore, they also tend to correlate weakly and/or inconsistently with core tobacco addiction criteria (i.e. subjective craving, experimental levels of deprivation, addiction-status, or rates of overt addictive behaviour). For example, examining the relationship of attention to addiction criteria in general Cox et al. (2006) found an $r = .21$ for the addiction-Stroop; Rooke et al. (2008) found an $r = .28$ for visual probe tasks; and Spiegelhalder et al. (2011), found *r*s = .14 and .07, respectively, for visual probe tasks and addiction-Stroops (for more intricate but concordant reviews of attention effects in tobacco addiction see Bradley et al., 2008; Field & Cox, 2008; Littel & Franken, 2011; Waters & Sayette, 2006, pp. 312-324).[7]

Subjective cravings is the variable most often theoretically related to addiction-related attention in the literature (see Bradley et al., 2004, 2008; Field, Mogg, & Bradley, 2005, 2006; Field & Cox, 2008; Franken, 2003; Larsen, Kong, Becker, Cousijn, Boendermaker, Cavello, Krishnan-Sarin, & Wiers, 2014; Mogg, Bradley, Field, & De Houwer, 2003; Mogg, Field, & Bradley, 2005), and yet a recent meta-analysis examining the correlation between measures of addiction-related attention and subjective cravings found the relationship to be very weak (i.e. across all addictions $r = .19$, and for tobacco addiction specifically $r = .16$; Field et al., 2009, p. 600). Moreover, Field et al.'s (2009) meta-analysis found evidence that addiction-related attention effects are commonly

[7] Indeed, it is worth noting that the poor internal reliability of attention measures may be largely a result of their aforementioned lack of precision with regard to motivational processes (i.e. in principle, attention measures could result haphazardly on each occasion from any of a variety of different confounded cognitive processes depending on context; see Ataya et al., 2012a, 2012b; Cox et al., 2006; Field & Cox, 2008; Field et al., 2009; De Houwer, 2011; Spiegelhalder, et al., 2011).

confounded by many different cognitive processes. Overall, therefore, attention measures clearly do not marshal enough precision to disentangle whatever irrational evaluative processes govern tobacco addiction from situation to situation (see Ataya et al., 2012a, 2012b; Bruce & Jones, 2006; Cisler et al., 2009; Field, 2010; Field & Cox, 2008; Field et al., 2009; Littel & Franken, 2011; Rooke et al., 2008; Spiegelhalder et al., 2011; Stacy & Wiers, 2010; Waters & Sayette, 2006).

Parsing Tobacco Addiction in Terms of Implicit Evaluating: A Systematic Review of Cue-based Measures

The first techniques that were designed to target implicit evaluative processes *per se,* did so by examining the extent to which smoking-related versus control cues could automatically moderate the performance of subsequent evaluative responses (see Barrett, 2006a, 2006b; Carter & Tiffany, 1999a, 1999b; Fazio, Sanbonmatsu, Powell, & Kardes, 1986; Feldman-Barrett, Lindquist, Bliss-Moreau, Duncan, Gendron, Mize, & Brennan, 2007; Ito & Cacioppo, 2007; Roediger, 2003; Roediger, Guynn, & Jones, 1994; Wentura & Degner, 2010; Wray et al., 2013). There are two main versions of this broad, cue-based approach: the *cue-reactivity paradigm* and the *evaluative priming paradigm.* As the simplest and earliest cue-based measure of tobacco addiction we deal with the cue-reactivity paradigm first (Carter & Tiffany, 1999; Drummond, 2000; Drummond, Tiffany, Glautier, & Remington, 1995; Rosenberg, 2009).

The Cue-reactivity Paradigm as a Means of Parsing of Tobacco Addiction

This approach involves presenting individuals with addiction-related versus control stimuli to measure how much doing so moderates self-reported cravings, physiological symptoms of arousal, and/or much less commonly, the intensity of whatever overt behaviours are subject to the relevant addiction (for reviews see Erlblich & Montgomery, 2012; Carter & Tiffany, 1999a, 1999b; Rose, Field, Franken, & Munafo, 2013; Wray et al., 2013). Such work was important primarily because it provided the first evidence that addictive responses are controlled by contextual cues (i.e. including temporary goals or expectations; see Becker et al., 2013; Carter & Tiffany, 1999a, 1999b; Drew et al., 2013; Eitam et al., 2013; Erblich & Montgomery, 2012; Field & Duka, 2001; Harmon-Jones et al., 2013; Hayes & Brownstein, 1986; Huntsinger, 2013; Mucha, Pauli, & Weyers, 2006, pp. 204-209; Rose et al., 2013; Vogt et al., 2013; Wray et al., 2013; Zhao et al., 2013). In particular, cue-reactivity research revealed that addiction-related stimuli tend to provoke many of the classic, intrusive symptoms of addiction whenever they are encountered (Carter & Tiffany, 1999a; Drummond, 2000). For example, Carter and Tiffany's (1999a) meta-analysis found that smoking-related stimuli produced very large experimental cue effects with smokers' self-reported cravings ($\bar{d}$ = 1.18; i.e. $\bar{r}$ ≈ .51)[8], and small to medium experimental cue effects even on smokers' physiological measures like heart rate and sweat-gland reactivity (respectively $\bar{d}$s = .21, .44; i.e. $\bar{r}$s ≈ .10, .22), even if no such effects were found for smokers' skin temperature ($\bar{d}$ = - .07; i.e. $\bar{r}$ ≈ .03; see also Mucha et al., 2006). In addition, although not a meta-analysis of the cue-reactivity paradigm per se, Heckman, Kovacs, Marquinez, Meltzer, Tsambarlis, Drobes, and Brandon (2013) found that negative mood induction moderately increased tobacco cravings ($\bar{g}$ = .47, $\bar{r}$ ≈ .23), even if positive mood inductions had inconsistent effects in this regard ($\bar{g}$ = .05, $\bar{r}$ ≈ .02).

And yet, contemporary cue-reactivity measures are severely limited for our purposes. The central problem is that cue-reactions are measured in an unrestrained fashion. For example, self-reports are the most common evaluative responses examined for addiction-related cue effects: we have already reviewed how such reports are prone to various distortions, and so the same point must apply to addiction-related cue-reactivity research (Carter & Tiffany, 1999a; Drummond, 2000; Drummond et al., 1995; Perkins, 2009; Rosenberg, 2009; Wray et al., 2013). Indeed, this has been confirmed by the finding that cue-reactions based upon self-reported cravings are prone to many extraneous influences (e.g. fleeting goals or expectations, and/or by asymmetric, construal-based order effects that are not resolved by counterbalancing; Dols, van den Hout, Kindt, & Willems, 2002; Erblich & Montgomery, 2012; Sayette et al., 2010).

One might be tempted, at this stage, to conclude that the cue-reactivity paradigm might provide a clear advantage over attention-based measures if psycho-physiological metrics (rather than self-reports) were employed. However, without procedures to ensure that each neural or autonomic response is driven by a particular evaluative process, such cue-reactivity effects are also open to a large variety of different motivational interpretations (see Barrett, 2006a, 2006b; Carter & Tiffany, 1999a, 1999b; Feldman-Barrett et al., 2007; Feldman-Barrett, Lindquist, & Gendron, 2007; Feldman-Barrett,

[8] Throughout the current thesis we used Rosnow, Rosenthal and Rubin's (2000) and Rosenthal and DiMatteo's (2001, p. 71) formulae to convert relevant statistical effects (e.g. *d*, *t* and *F* values coupled with their corresponding degrees of freedom) to *r* values in order to facilitate comparisons among different types of statistical effect sizes where appropriate.

Mesquita, & Gendron, 2011; Field & Duka, 2001; Kelly, Barrett, Pihl & Dagher, 2004; Havermans, Mulkens, Nederkoorn, & Jansen, 2007; Hendricks et al., 2008; Heyman, 2011, 2013; Ito & Cacioppo, 2007, pp. 149-151; Kassam & Mendes, 2013; Miller, 2010; Mucha et al., 2006; Perkins, 2009; Poldrack, 2006; Rose et al., 2013; Rosenberg, 2009; Sayette et al., 2010; Wang & Minor, 2008). For example, if a given addiction-related cue like a cigarette box is observed to heighten a smoker's autonomic arousal in terms of increased heart rate, it could mean that the smoker became more excited about the prospect of smoking, or it could mean that they were begrudgingly experiencing increased urges to smoke, or indeed, it could perhaps otherwise mean that they were anxious about a government sponsored warning about smoking that they noticed on that cigarette box. Thus, cued psycho-physiological measures are unable to make even broad distinctions between positive versus negative evaluating per se (e.g. Ajzen & Fishbein, 2000; Bandura, 1977; Biglan & Hayes, 1996; Eagley & Chaiken, 1993; Hermans, Spruyt, De Houwer & Eelen, 2003, p. 97; Herring, White, Jabeen, Hinojos, Terrazas, Reyes, Taylor, & Crites, 2013, pp. 1-2; James, 1890a; Robinson & Berridge, 2008; Schwarz, 2008).

The key point, therefore, is that the cue-reactivity literature, based as it is upon self-reported cravings and indeterminate psycho-physiological responses, is unable to clearly distinguish how much its cue effects involve positive versus negative evaluating. Accordingly, neither cue-reactivity effects based upon self-reported tobacco cravings nor those based upon psycho-physiological measures have reliably related either to each other, or to any clinically relevant aspects of smoking (e.g. such as number of cigarettes smoked per day, or the risk of relapse during smoking abstinence; see Carter & Tiffany, 1999a, 1999b; Drummond, 2000; Erblich & Montgomery, 2012; Havermans et al., 2007; Perkins, 2009; Rose et al., 2013; Wilson et al., 2013; Wray et al., 2013).[9]

For these reasons, successive reviews of the cue-reactivity literature have emphasized the need to research cue-reactivity effects based directly upon overt and relatively extended aspects of addictive behaviour (e.g. latency to smoke, smoking

[9] For example, Havermans et al. (2007) found an average $r = .04$ between cue-reactivity measures in terms of subjective tobacco cravings versus various aspects of physiological arousal. Likewise, Payne, Smith, Sturges, and Holleran (1996, pp. 147-148) found that of the four non-craving tobacco addiction criteria they assessed none significantly correlated with cue-reactivity based upon subjective tobacco cravings. Likewise, although Payne et al. did observe a correlation between these cue-reactivity measures about subjective cravings and changes in heart rate ($r = .20$), these cue-reactivity measures only correlated modestly with self-reported changes in negative affect ($r = .21$) and positive affect ($r = .16$), and not at all with changes in systolic blood pressure or self-reported physical symptoms.

frequency, etc.; Carter & Tiffany, 1999a; Drummond, 2000; Perkins, 2009; Rose et al., 2013; Rosenberg, 2009; Sayette, Shiffman, Tiffany, Niaura, & Shadel, 2000; Wray et al., 2013). However, such research has been unforthcoming, with just two relevant studies listed in a recent review of the smoking-related cue-reactivity literature (Perkins, 2009, p. 1614; but for a few additional examples of such effects not reviewed, some of which are null, see Greenberg & Altman, 1976; Lochbuehler, Kleinjan, & Engels, 2013; O'Connell, Shiffman, & DeCarlo, 2010; Niaura, Abrams, Pedraza, Monti, & Rohsenow, 1992; Payne, Schare, Levis & Colletti, 1991; Van Gucht, Van Den Bergh, Beckers, & Vansteenwegen, 2010). And in any case, even if such research were more common, it would still suffer from much the same core problem as the psycho-physiological measures described above. That is, to measure only relatively extended addictive behaviours is to ignore the many different positive versus negative varieties of evaluating that may have motivated those behaviours from occasion to occasion (see Barrett, 2006a, 2006b; Chassin et al., 2007; Droungas, Ehrman, Childress, & O'Brien, 1995; Feldman-Barrett et al., 2007; Feldman-Barrett, Lindquist, & Gendron, 2007; Feldman-Barrett et al., 2011; Lochbuehler et al., 2013; Mucha et al., 2006; Niaura et al., 1992; O'Connell et al., 2010; Payne et al., 1991, pp. 477-478; Moore, 2013; Payne, McClernon, & Dobbins, 2007; Perkins, Epstein, Grobe, & Fonte, 1994; Perkins et al, 1997; Rose et al., 2013; Rosenberg, 2009, pp. 526-528; Van Gucht et al., 2010; Wray et al., 2013).

In summary, although cue-reactivity measures are not designed like attention measures to be agnostic about evaluative processes, they are prone to confound such processes. Moreover, even ignoring the indeterminacy of cue-reactivity measures with respect to evaluating, much like the literature on attention there is little in the cue-reactivity literature to verify the implicitness of its measures apart from arguments based upon face validity (see Bruce & Jones, 2006; Carter & Tiffany, 1999a, 1999b; Erblich & Montgomery, 2012; Field, 2010; Field & Cox, 2008; Field, Mogg, & Bradley, 2006; Field, Munafó, & Franken, 2009; Rooke et al., 2008; Rose, Field, Franken, & Munafo, 2013; Wiers & Stacy, 2006a, 2006b; Wray et al., 2013).

The Evaluative Priming Paradigm as a Means of Parsing Tobacco Addiction

One approach that appears, at least at first blush, to address the lack of precision associated with attention and cue-reactivity based measures is *evaluative priming* (for reviews see Cameron, Brown-Iannuzzi, & Payne, 2012; De Houwer et al., 2009a; Fazio &

Olson, 2003; Fazio, Sanbonmatsu, Powell, and Kardes, 1986; Herring et al., 2013, pp. 1064-1065; Klauer & Musch, 2003; Payne & Gawronski, 2010, pp. 8-9; Payne et al., 2007; Wentura & Degner, 2010; Wittenbrink, 2007). Like cue-reactivity methods, evaluative priming is a cue-based approach that presents addiction-related versus control cues in order to measure implicit evaluating. However, unlike the cue-reactivity paradigm, evaluative priming does not involve measuring evaluative responses to its cues per se. Rather, the focus is upon how its cues, usually called *primes*, moderate the *fluency* (i.e. speed, accuracy, or frequency) of positive versus negative evaluating in general.

There are a large variety of different ways to implement evaluative priming measurements (for reviews see De Houwer et al., 2009a; Fazio & Olson, 2003; Herring et al., 2013, pp. 2-3; Klauer & Musch, 2003; Payne & Gawronski, 2010, pp. 8-9; Wentura & Degner, 2010, pp. 96-97; Wittenbrink, 2007). Nevertheless, in broad methodological terms they all compare how fluently participants perform the task of positively versus negatively evaluating *target stimuli* that are respectively designed to be normatively positive versus negative and irrelevant to the researcher's topic of interest. For example, during a priming task a participant might be asked to press one button if a pleasant word is presented and a second button if an unpleasant word is presented. On some trials an addiction-related prime (e.g., a picture containing a packet of cigarettes) is presented briefly before the target word and on other trials a non-addiction-related prime may be presented (e.g., the same picture but without the cigarette packet). Critically, participants are not asked to evaluate the prime in any way. The basic rationale is that if addiction-related stimuli are evaluated positively (at an implicit level) participants should be "primed" to respond more fluently to the pleasant target words when these are preceded by addiction-related stimuli than when they are not. Conversely, if addiction-related stimuli are evaluated negatively (at an implicit level) then the opposite should be the case (e.g., a negative target word should be evaluated as negative more fluently when it is preceded by the addiction-related prime).[10]

[10] The foregoing example describes the most popular variant of evaluative priming, Fazio, Sanbonmatsu, Powell, and Karde's (1986) *affective priming* methodology. Methods like this which task participants with evaluating target stimuli as being positive versus negative contribute approximately 61% of all evaluative priming effects currently in the literature (see Herring et al., 2013; see also the *Affect Misattribution Procedure,* AMP; Payne, Chang, Govorun, & Stewart, 2005). The remaining evaluative priming variants do not task participants with evaluating target stimuli but instead focus upon measuring how the normative evaluative congruity between primes and target stimuli moderates the fluency of participants' performances on non-evaluative categorizing tasks (e.g. such as naming/pronunciation, lexical verification, or non-evaluative semantic categorization tasks which respectively comprise approximately 30%, 5%, and 5% of extant evaluative priming research; see Herring et al., 2013). These so-called semantic variants of evaluative

The important point here is that evaluative priming effects should reflect implicit aspects of evaluation because they generally do not require participants to evaluate the prime stimuli, in a conscious or deliberative manner. In addition, all evaluative priming methods present each trial's prime in close succession with its target stimulus, such that there is very little time for a participant to think about the prime, in a deliberative or highly controlled manner, before evaluating the target.[11] As such, any priming effect that emerges is likely to be due to the automatic or implicit evaluation of the prime, rather than through some slow, deliberative or controlled cognitive processing. Indeed, the evaluative priming literature has gone a long way towards systematically corroborating the implicit character of its measurement scores.

Firstly, evaluative priming effects tend to occur with greater magnitude the shorter the time from prime to target presentation and the shorter the duration of the target presentation (i.e. indicating that the cognitive processes involved were rapid; see De Houwer, 2009; De Houwer & Moors, 2007; Hermans, De Houwer & Eelen, 2001; Moors, Spruyt & De Houwer, 2010; Payne, Cheng, Govorun, & Stewart, 2005; Payne & Gawronski, 2010; Wentura & Degner, 2010; Wittenbrink, 2007). Secondly, even when primes are presented subliminally (i.e., primes are presented so rapidly that participants cannot report accurately what prime was presented), most[12] evaluative priming effects still occur in line with known-groups behavioural preferences (i.e. suggesting that the cognitive processes involved may occur without awareness; see Cameron et al., 2012, pp. 336, 343-344; De Houwer et al., 2009a; Klauer & Musch, 2003; Herring et al., 2013, pp. 20-21; Hermans et al., 2003; Payne & Gawronski, 2010, p. 9; Van den Bussche, Van den Noortgate, & Reynvoet, 2009; Wentura & Degner, 2010). Thirdly, a recent meta-analysis

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priming were specifically designed to settle theoretical controversies about how much Fazio et al.'s (1986) affective priming effects derived from high- versus low-level mental processes (i.e. semantic evaluative memory processes versus evaluative response processes per se; cf. Cameron et al., 2012; Herring et al., 2013; Moors, Spruyt, & De Houwer, 2010; Schmitz & Wentura, 2012; Wentura & Degner, 2010; Werner & Rothermund, 2013).

[11] Usually each priming trial presents a target stimulus within a few hundred milliseconds after the onset of its prime; and yet, for the purposes of examining how various theoretically relevant cognitive effects emerge during the time course of evaluative priming trials, a very small minority of studies have presented individual primes and targets at the same time as each other or even in slightly reversed but overlapping sequences (see Hermans, De Houwer, & Eelen, 2001; Hermans et al., 2003; Herring et al., 2013, pp. 5-7; Klauer, Rossnagel, & Musch, 1997; Schmitz & Wentuura, 2012; Spruyt, Hermans, De Houwer, Vandromme, & Eelen, 2007; Wentura & Degner, 2010, pp. 107-108).

[12] Whereas subliminal evaluative priming does consistently yield evaluative priming effects in line with known-groups preferences when derived from evaluative tasks (i.e. with the exception of one variant, the AMP; see Rohr, Degner & Wentura, 2014), those derived from non-evaluative tasks do not (see Wentura & Degner, 2010, pp. 99-100).

found that evaluative priming effects correlated strongly with various behavioural criteria when those criteria were likely to have been governed mainly by implicit cognition, average $r = .45$, $p < .00001$, $Q = 25.62$. That is priming occurred in situations where there was likely to be lower motivation and/or ability to engage in deliberative, strategic styles of thinking; see Cameron et al., 2012, pp. 332, 338-339, 344), but significantly less, $Z = 3.45$, $p = .001$, and indeed not at all when those behavioural criteria were likely to have been governed by deliberative (explicit) cognitive processes, average $r = .06$, $p = .46$, $Q = 15.68$ (pp. 338-339, 344).[13]

And yet, despite being significantly correlated with various criteria for irrational behaviours, regrettably, it can also be argued that evaluative priming measures suffer from much the same core problem as attention and cue-reactivity measures. Before exploring these arguments, it is important to understand that the relevant issues are very much obscured by the existence of many different varieties of evaluative priming in the relevant literature as compared to attention or cue-reactivity procedures (see Carter & Tiffany, 1999a; Rose et al., 2013; Wentura & Degner, 2010; Wittenbrkink, 2007). With so many variants of evaluative priming attempting to improve upon their respective predecessors each in different ways, the surrounding literature is dense with disjointed controversies about the peculiarities of each variant. This results in an equivocal literature about which it is difficult to establish generalities without the considerable distraction of having to address multiple disjointed and collateral issues along the way. Thus, in general we will refer any such collateral issues to be addressed in Appendix 1 in order to preserve the clarity of each criticism that we wish to make about evaluative priming.

The basic argument is as follows. During evaluative priming tasks, participants must respond to each target stimulus as being either ‘good’ versus ‘bad’, ‘pleasant’ versus ‘unpleasant’, or as more versus less pleasant than average, etc. (i.e. depending on the version of priming being employed). Thus participants are typically permitted to deliberate for as long as they like, and in many evaluatively different but unrecorded ways, before providing the required response on each priming trial (for a discussion of collateral issues

[13] Indeed, even defining the enabling conditions for implicit versus deliberative cognition idiosyncratically according to domain-specific predictions, Cameron et al. still found that evaluative priming effects correlated more with various behavioural criteria under those idiosyncratic conditions predicted to foster implicit styles of cognition, but significantly less, $Z = 7.05$, $p < .00001$, and indeed not at all under contrasting conditions predicted to foster deliberative styles of thinking (i.e. respectively, average $r = .40$, $p < .00001$, $Q = 30.10$, versus, average $r = -.004$, $p = .99$, $Q = 24.63$).

surrounding this topic see Note 1, Appendix 1; otherwise see Hermans et al., 2001, p. 145; Wentura & Degner, 2010). Therefore, regardless of how heavily each trial's recording is influenced by the prime's activation of implicit cognitive processes, such recordings are delivered in a binary fashion that obscures the various different topics of evaluating involved.

To illustrate, consider how many ways a smoker might construe the task of evaluating a target stimulus word 'fabulous' as positive versus negative following a prime word 'cigarette.' A positive response could mean that the smoker interpreted 'cigarette' and 'fabulous' in terms of how smoking a cigarette would make them feel fabulous; or alternatively, it could mean that the relevant smoker was thinking about how fabulous it would be to quit smoking if only they felt able to. As such, a positive response following a smoking-related prime could mean any of a vast range of things not just about how the smoker interpreted their task on the relevant trial, but also about how they interpreted that trial within its wider measurement context. Worse still, there are likewise many different ways a smoker could construe a positive versus negative categorisation task for each combination of smoking versus control primes with positive versus negative target stimuli.[14] Indeed, regardless of instructions to the contrary, there is nothing about evaluative priming procedures that prevents smokers from secretly ignoring target and/or prime stimuli on at least some trials (for a discussion of collateral issues surrounding this topic see Note 2, Appendix 1).

Confirming such possibilities, multiple streams of experimental research have recently emerged showing that evaluative priming effects do not result from the tasks set by evaluative priming procedures per se, but rather, from whatever evaluating is unwittingly induced on those tasks by each participant's wider measurement context (for a meta-analysis of relevant methodological effects see Herring et al., 2013; also see Alexopoulos, Fiedler & Freitag, 2012; Blair, 2002; Cameron et al., 2012, p. 336; Cesario, 2014; Chan, Ybarra, & Schwarz, 2006; De Houwer, 2009, pp. 378-386; De Houwer et al., 2009a, pp. 358-360; Deutsch & Gawronski, 2009; Fiedler, 2003; Gawronski, Cunningham, LeBel & Deutsch, 2010; Gawronski, Deutsch, LeBel & Peters, 2008; Klauer, Rossnagel, & Musch,

[14] Control primes seem particularly likely to prompt a large variety of unrecorded target interpretations when, as is often the case, they are specifically chosen to have neutral, and thus tenuous evaluative connotations with the topic of interest (i.e. otherwise, some researchers choose control primes to fit a conceptual category they speculatively believe will have opposing functions relative to the topic of interest for most people; see Wentura & Degner, 2010, pp. 98, 103, 106-107; Wittenbrink, 2007).

1997; Hermans et al., 2003, pp. 108-110; Loersch & Payne, 2011; Moors, Spruyt & De Houwer, 2010, pp. 26-33; Schmitz & Wentura, 2012; Werner & Rothermund, 2013; Wittenbrink, 2007, pp. 39-45).[15] For example, when participants are suitably motivated and/or instructed they can normally distort evaluative priming effects in a secret and strategic manner (i.e. increasing, reducing, obliterating or even reversing priming effects; for a discussion of collateral issues surrounding this topic see Note 3, Appendix 1; otherwise see Bar-Anan, 2010; Cameron et al., 2012, p. 336; Degner, 2009; De Houwer et al., 2009, pp. 360-362; Klauer & Teige-Mocigemba, 2007; Moors et al., 2010, pp. 30-32; Teige-Mocigemba & Klauer, 2008; Wentura & Degner, 2010, pp. 97-98). Thus, although it is possible to systematically influence evaluative priming effects by experimentally inducing particular types of evaluating, it is important to remember that such influences are extraneous to evaluative priming procedures per se. In other words, even when particular types of evaluating do systematically influence priming effects based on some unknown aspect of the wider measurement context, such effects are not equipped to identify the specific types of evaluating involved.

Worse still, this issue of identifying which particular evaluative processes systematically influence evaluative priming effects is largely moot given that evaluative priming effects are for the most part not very systematic; a fact borne out by their generally very low internal reliability (e.g. average $\alpha \leq .20$ for affective priming; LeBel & Paunonen, 2011, p. 572; see also De Houwer, 2009, pp. 379-384; Gawronski & De Houwer, 2014; Roefs et al., 2011, p. 153; Wentura & Degner, 2010). The key problem here is that participants tend to evaluate each priming trial idiosyncratically in terms of its wider measurement context (i.e. as distinct from other trials and also from the particular topic it was intended to exemplify; see De Houwer, 2009, pp. 374-384; see also Blaison, Imhoff, Huhnel, Hess & Banse, 2012; Deutsch & Gawronski, 2009; Gawronski et al., 2008; Spruyt, Klauer, Gast, Schryver, & De Houwer, 2014; Rydell & Gawronski, 2009). Thus because

[15] Note that variants of evaluative priming which set non-evaluative tasks should, in principle, be even more prone to multiple evaluative interpretations than versions using evaluative tasks. Whereas standard evaluative priming at least instructs participants to evaluate in broad positive versus negative terms, evaluative priming with non-evaluative tasks does nothing to directly encourage participants to evaluate trial stimuli consistently in any particular way across trials. In fact, this is a leading explanation for the dual findings that (a) evaluative priming effects from naming/pronunciation tasks are substantially weaker and less replicable (particularly under subliminal conditions) than priming effects involving evaluative tasks and likewise that (b) more elaborate non-evaluative semantic tasks generally fail even to produce evaluative priming effects (see De Houwer, 2009, pp. 378-386; Herring et al., 2013; Moors, Spruyt & De Houwer, 2010, pp. 28-32; Wentura & Degner, 2010, pp. 98-99; Werner & Rothermund, 2013).

evaluative priming does not succeed in measuring people's evaluative responses to any given topic in some consistent and determined sense, its scores are likely to be severely limited in their ability to correlate with criterion behaviours (for a discussion of relevant collateral issues involving the Affective Misattribution Procedure see Note 4, Appendix 1). Indeed, throughout the 30 or so years since its inception evaluative priming has consistently produced very small priming effects regardless of the extent of known-groups behavioural preferences upon which they were based (weighted average mixed effects model d = .37, i.e. $r \approx .18$, Herring et al., 2013, pp. 1061-1064; see also Bar-Anan & Nosek, 2014). And likewise, Rooke et al.'s (2008) meta-analysis of addiction-related evaluative priming effects yielded poor correlations with behavioural criteria related to addiction (i.e. across 8 individual studies average $r = .23$, $p < .01$). In particular, with the modest exception of research involving two recent variants of evaluative priming, smoking-related evaluative priming scores do not generally correlate with tobacco addiction criteria at all.

The first study to use evaluative priming to measure smoking-related implicit processes (Sherman, Rose, Koch, Presson and Chassin, 2003) yielded a small-to-moderate interaction between heaviness of smoking and nicotine deprivation (i.e. $r = .17$; converted from F to r using conversion formulae from Rosnow et al., 2000). However, those scores did not distinguish smokers from non-smokers, and despite claims to the contrary, nor did they even produce pro-smoking priming effects for smokers (although Study 1 reported a pro-smoking priming effect of just 27.01 ms for smokers, Study 2 reported an average anti-smoking priming effect of 33.62 ms for smokers using the same stimuli).[16] Likewise, when using traditional evaluative priming methods a number of studies have failed to produce pro-smoking priming effects for smokers or to distinguish smokers from non-smokers (Glock, Unz & Kovacs, 2012; Glock, Kovacs & Unz, 2014; O'Connor, Fite, Nowlin, & Colder, 2007; Leventhal, 2008).[17]

[16] Indeed, given that smoking-related evaluative priming effects are based upon interaction effects between smoking-related versus contrast primes and positive versus negative target stimuli, it is therefore not valid to claim that such effects are pro- versus anti-smoking just because they are significantly different from zero. For example, positive scores might arise not because smoking-related primes facilitated correct responses to positive targets but because the contrasting primes interfered with correct responses to negative targets (see Wentura & Degner, 2010, pp. 100-105).

[17] Although Glock et al. (2012) and O'Connor et al. (2007) both claimed to have found evidence for pro-smoking evaluative priming, in fact neither obtained a significant prime by target interaction in favour of smoking. Instead, they mistakenly interpreted simple effects between target stimuli as indicating evidence of evaluative priming even though such effects do not compare the influence of different types of primes at all (for a detailed explanation of the inherently flawed logic behind this common approach see Wentura & Degner, 2010, p. 105).

On balance, Leventhal (2008) did find some evidence for the validity of an improvised evaluative priming method called the subliminal repetition priming task. Specifically, Leventhal found that deprived smokers produced priming effects that were significantly more pro-smoking (and/or less anti-smoking) than those obtained by non-deprived smokers or non-smokers, $F(2, 166) = 4.99$, $p = .01$, $\eta_p^2 = .03$ (i.e. $r \approx .17$). However, these modest findings in favour of smoking-related subliminal repetition priming are not just unprecedented, they appear to participate in publication bias. Namely, Leventhal, Waters, Breitmeyer, Tapia, Miller and Li (2008) published the positive repetition priming effects from Leventhal's (2008) Ph.D. thesis, but they did not mention how these priming effects generally failed to correlate with core aspects of tobacco addiction, and nor did they report Leventhal's (2008) null findings for the equivalent unmodified evaluative priming procedure (see above).

The only remaining evidence in favour of the ability of evaluative priming to measure smoking-related implicit processes comes from just two research publications on the AMP. Payne et al. (2007) were the first to report that AMP scores correlated with core tobacco dependence criteria. In summary, Payne et al. found that AMP scores correlated non-significantly with the number of cigarettes smoked per day, $r = .28$, $p = .11$, correlated significantly with the Shiffman/Jarvik Withdrawal Questionnaire of current nicotine withdrawal, $r = .49$, $p < .01$, and correlated non-significantly with the Wisconsin Inventory of Smoking Dependence Motives (WISDM) a popular 68-item measure of nicotine dependence, $r = .31$, $p < .01$. In addition, Payne et al. found that smokers differed significantly from non-smokers in terms of their AMP scores, $F(1, 58) = 27.01$, $p < .001$, $\eta^2 = .32$ (i.e. $r \approx .57$). Lastly, Payne et al. claimed that smokers with relatively high versus low nicotine withdrawal and/or dependence scores invariably produced pro- versus anti-smoking AMP scores, respectively. However, Payne et al. never tested this assertion directly but instead, rather puzzlingly, based it mainly upon a scatterplot which included many cases to the contrary. Furthermore, even if we put aside the fact that it is not valid to interpret an AMP score of zero as indicating the absence of an evaluative bias (see Wentura & Degner, 2010, p. 105), Payne et al.'s smoking-related AMP scores were clustered about both sides of zero for most of the range of nicotine withdrawal scores observed.

Indeed, in an effort to clarify why Payne et al. (2007) obtained so-called ambivalent AMP scores among smokers (i.e. AMP scores distributed about zero, $F(1, 34) = 1.82$, $p =$

.19, $\eta^2 = .05$), Haight, Dickter and Forestell (2012) distinguished between the occasional versus daily types of smokers that were mixed together in Payne et al.'s analyses. Haight et al. found that daily smokers produced AMP scores that were moderately more pro-smoking (and/or less anti-smoking) than occasional smokers, $F(1, 50) = 5.45$, $p < .03$, $\eta^2 = .10$ (i.e. $r \approx .32$). Putting aside the fact that AMP scores do not necessarily reflect ambivalence just because they are distributed around zero, this finding does provide evidence that the relevant AMP scores are correlated with tobacco addiction. In addition, Haight et al. found a very similar correlation as Payne et al. (2007) between smokers' AMP scores and their smoking frequency, $r = .26$, $p = .06$.

However, Haight et al. did not corroborate Payne et al.'s (2007) speculations with regard to there being a relationship between smokers' AMP scores versus their current levels of nicotine deprivation or nicotine withdrawal. Namely, Haight et al. found no significant correlation between smokers' AMP scores versus the length of time since they had last smoked, $p > .05$, $r = -.20$, nor between smokers' AMP scores versus their WISDM scores, $p > .05$, $r = .05$. In addition, it is worth noting that Haight et al. only obtained positive findings when they were considering a particular subset of smoking-related pictures that depicted people interacting with smoking-related stimuli. When Haight et al. (2012) computed AMP scores in terms of smoking-related pictures that did not depict people interacting with the relevant smoking-related stimuli none of the foregoing analyses yielded statistically significant findings. Likewise, in a preceding version of Haight et al.'s (2012) research which they do not mention, Haight's (2011) unpublished thesis, which incorporated a 12-hour experimental manipulation of nicotine deprivation, there was no significant effect upon smokers' AMP scores, $F(1, 50) = 1.24$, $p = .271$. Moreover, Haight (2011) found smaller corresponding known-groups effects than Haight et al. (2012), and an even smaller correlation than Haight et al. between smokers' AMP scores and smoking frequency, $r = .21$.

In summary, apart from some encouraging known-groups effects involving AMP scores from two studies and a small correlation between AMP scores and smoking frequency that has been replicated just once, there is very little evidence that smoking-related evaluative priming scores bear any reliable relationship with tobacco addiction. Granted, we did mention earlier how Cameron et al.'s (2012) meta-analysis found encouraging evidence that evaluative priming effects (i.e. including the AMP) generally correlate highly with behavioural criteria that specifically encourage implicit cognition. However, even disregarding the fact that these correlations were relatively weak across domains related to addiction (i.e. r = .24 for 'impulsive behaviours'), the behavioural criteria involved in that meta-analysis were predominantly comprised of self-reported behavioural intentions and judgements measured during the same session as the relevant evaluative priming effects (i.e. only 23% of the behavioural criteria involved were not self-reported; see Cameron et al., 2012, pp. 333, 342). Thus, although such findings do support the implicit nature of evaluative priming effects in general, they provide only equivocal support for criterion validity because self-reported behavioural intentions and judgements are renowned for their lack of adherence to criterion behaviours (i.e. particularly in domains such as addiction where personal autonomy is compromised; Armitage & Connor, 2001; Sheeran, 2002; Topa & Moriano, 2010; Webb & Sheeran, 2006, p. 254; Webb, Sheeran, & Luszczynska, 2009). What is more, there is clear evidence in Herring et al.'s (2013, p. 1082) meta-analysis that such estimates are likely inflated in general by publication biases favouring statistically significant findings. Overall, therefore, existing evaluative priming techniques clearly fail to produce scores that consistently measure whatever implicit evaluative processes might be involved in tobacco addiction.

A Future for Cue-based Measures of Smoking-related Implicit Evaluating?

Existing cue-reactivity and evaluative priming techniques all suffer from the same key problem as each other: they lack the ability to determine what particular topics of implicit evaluating are involved in the scores they produce. Indeed, as illustrated above, these techniques cannot ensure even such broad distinctions as between (implicit) evaluating in positive versus negative terms. And yet, unlike attention measures which must ignore any distinctions between particular topics of evaluating by design, there is nothing definitive preventing future evolutions of priming and/or cue-reactivity techniques from employing response measures that do distinguish particular topics of implicit evaluating from each other. Specifically, if cue-based techniques incorporated controlled experimental comparisons of how cues moderate precise measures of implicit evaluating, then by experimental first principles it would be clear what particular topics of implicit evaluating were involved in the relevant comparison scores (for a sidebar on the wider importance of this point to research about the neurobiology and psychopharmacology of addiction and/or to research even more broadly on the context dependent nature of behaviour see Note 5, Appendix 1). Critically, therefore, if we are to determine how contextual cues moderate any particular evaluating involved in tobacco addiction then we must first identify measures that can distinguish implicit evaluating per se. Indeed, as we attempt to review in the following two chapters, this issue is an important recurring problem throughout the various remaining literatures on addiction-related implicit evaluating.

CHAPTER 2: The Second Wave of Research on Smoking-related Implicit Cognition: A Comprehensive Review of the IAT Literature

The Implicit Association Test (IAT; Greenwald, McGhee & Schwartz, 1998) is by far the most commonly used measure of implicit attitudes to date (Payne & Gawronski, 2010). At half the age of evaluative priming methods, the IAT and its variants command almost twice as many citations (Nosek et al., 2011). In fact, Nosek et al. (2011) estimated that across the literature on implicit evaluating the IAT and its variants account for almost all method-specific citations not already accounted for by attention and cue-based methods (i.e. all but approximately 7% of such citations).[18]

From the outset, one of the primary reasons why the IAT (in the interests of brevity, henceforth we will refer to the full range of IATs simply as the IAT) was so popular was its general ability to produce measurement effects that were much more internally reliable than other measures of implicit attitudes (see Payne & Gawronski, 2010, p. 5; Teige-Mocigemba et al., 2010, p. 118). For example, when the IAT was first introduced, evaluative priming effects had internal reliabilities that were typically less than .20, but in contrast IAT effects regularly achieved internal reliabilities in the range .70-.90 (see Nosek, Greenwald & Banaji, 2007; LeBel & Paunonen, 2011, p. 572; see also De Houwer, 2009, pp. 379-384; Gawronski & De Houwer, 2014; Roefs et al., 2011, p. 153; Wentura & Degner, 2010). Many therefore reasoned that if internal reliability is an absolute prerequisite for measuring individual differences, and if no other measure of implicit evaluating could satisfy this prerequisite, then this implies that the IAT is best equipped to measure individual differences in implicit evaluating (Dasgupta, Greenwald, & Banaji, 2003; Greenwald & Banaji, 1995; Greenwald et al., 1998; Lane, Banaji, Nosek & Greenwald, 2007, p. 61).

However, whether we take Greenwald et al.'s (2009) estimate of $r = .22$ or Rooke et al.'s estimate of $r = .18$ as the criterion validity of IATs focused on addiction, the IAT's contribution to this research area appears limited. Indeed, on balance, it is safe to say that the IAT does not outperform the addiction-related criterion validity of evaluative priming, which currently stands at $r = .23$ (Rooke et al., 2008, p. 1323). As such, it appears that high

[18] Although Nosek et al. (2011) do not directly account for attention or cue-reactivity based methods, they estimated that in the remaining literatures about implicit cognition evaluative priming methods contributed approximately 35% and IAT-based methods approximately 58%, of all method-specific citations. Therefore, by implication, only approximately 7% of all method-specific citations of implicit measures are due to methods other than those based on attention, cues, or the IAT.

internal reliability is certainly not tantamount to high criterion validity when it comes to IATs focused upon addiction. Nonetheless, it may still be useful to ask whether the IAT is capable of being further developed for greater precision with respect to the implicit evaluating involved in tobacco addiction. As we shall see subsequently, however, there is good reason to doubt that such a research agenda is feasible.

2.1. Why it is Not Feasible to Improve the IAT's Modest Criterion Validity for Addictions

Assuming that the criterion validity of an IAT is reliant upon how precisely it can distinguish among the different topics of implicit evaluating involved in a given addiction criterion, then enhancing the precision of that IAT with respect to those component processes should in principle enhance its ability to achieve the relevant form of criterion validity. Regrettably, however, the IAT is bound by design to overlook key aspects of evaluating that are commonly involved in behavioural criteria, and so the IAT is fundamentally limited in its ability to develop criterion validity in many domains. To understand why, we will consider the IAT method in detail.

The IAT is a computerized task that involves asking participants to repeatedly perform two different types of binary classification as quickly and as accurately as possible. The first classification task involves assigning various individual stimuli as belonging to one of two contrasting target categories (e.g. flowers versus insects), and the other type involves assigning various individual stimuli as belonging to one of two contrasting attribute categories (e.g. pleasant versus unpleasant). During the critical part of an IAT, participants must complete randomly alternating sequences of these two types of classification tasks using just two response keys. As such, each response key is shared between one target category and one attribute category during critical sequences of IAT trials. In addition, participants must alternate between using two different key assignment schemes depending on the particular sequence of IAT trials involved.

Crucially, the IAT effect is a comparison of how quickly participants can correctly complete both types of binary classification task using one key assignment scheme versus the other. The core assumption underpinning this design is that the maximum speed with which participants correctly classify stimuli on IAT trials should be proportional to how much each response key designates a target concept and an attribute concept that are associated in the relevant participant's memory (see Greenwald et al., 1998). For example, most participants should find it easier to categorize flowers with pleasant and insects with unpleasant than vice versa (i.e., flowers-unpleasant & insects-pleasant) because most people associate flowers with pleasant things and insects with unpleasant things. However, there is a long recognised limitation at the heart of this design: the IAT cannot measure implicit evaluating about one target category as distinct from another, and nor can it measure in terms of one attribute category as distinct from another (see Blanton & Jaccard, 2006; Fiedler, Messner, & Bleumke, 2006; Teige-Mocigemba et al., 2010, p. 133).

To illustrate, consider the earlier scenario of a participant categorizing flowers with pleasant and insects with unpleasant more quickly than vice versa. Critically, such a finding could indicate that (a) flowers are liked and insects are disliked; (b) both flowers and insects are liked, but the former are liked more than the latter; or (c) both flowers and insects are disliked, but the former are disliked less than the latter. In fact, the finding could also indicate that flowers are liked and insects are neither liked nor disliked (a neutral preference), or that insects are disliked and flowers evoke no preference (see Blanton & Jaccard, 2006). The key problem here is that the IAT cannot determine how much flowers are implicitly associated with pleasantness as distinct from relative comparisons with insects or unpleasantness. The IAT is therefore widely termed a *relative* measure because it might reflect the strength of an implicit association between a given target attitude object and a target attribute category, but only relative to (a) a contrasting attitude object and/or (b) a contrasting attribute category.

When it comes to designing questionnaire-based self-report measures of (explicit) evaluating researchers are not constrained as they are with the IAT to examining their preferences in relatively-phrased terms. Rather, using self-reports usually grants researchers the option of asking participants how much they like a given attitude object such as flowers (e.g. on a scale of 0-10) (a) without ever asking how much they dislike flowers and (b) without ever drawing comparisons with contrasting attitude objects such as insects. Therefore, the problematic nature of the IAT's relativity is perhaps best appreciated in terms of the behavioural difference it makes when self-reported evaluations are framed in relative versus non-relative terms. Much research shows, for example, that people often behave differently about an attitude object when it is being evaluated on its own versus in comparison to a contrasting attitude object (see *anchoring heuristic, social comparions, conceptual correspondence*, *structural fit* and *transfer-appropriate processing*; Bem, 1967, 1973; De Houwer, 2009, p. 367; Epley & Gilovich, 2006; Furnham & Boo, 2011; Festinger, 1957; Gschwender, Hofmann & Schmitt, 2008; Hofman, Gawronski, Gschwender, Le, & Schmidt, 2005; Payne, Buckley & Stokes, 2008; Payne & Gawronski, 2010, pp. 5-6; Suls & Wheeler, 2013). Likewise, other streams of research have long-established that people behave differently in response to a topic when they are evaluating it in (a) positive terms, (b) negative terms, or (c) positive versus negative terms (see Field, 2010, pp. 637-638; Sweeney, Pilliteri, & Kozlowski, 1996; Tversky & Kahneman, 1981; Kahneman, 2011).

Thus, the relative nature of the IAT may be particularly problematic when it comes to dealing with topics like addiction about which people are likely to be ambivalent. For example, it is characteristic for smokers to evaluate smoking in terms of positive attributes in one instance (e.g., when socializing with other smokers) but in terms of negative attributes in another context (e.g. when thinking about the costs of smoking; see Brandon, Wetter, & Baker, 1996; Cano, Lam, Adams, Correa-Fernández, Stewart, McClure, Cinciripini, & Wetter, 2014; Copeland et al., 1995; de Jonge & Gormley, 2005; Harrell & Juliano, 2012; Hine, Marks, & O'Neill, 2009; McKee, Wall, Hinson, Goldstein, & Bissonnette, 2003; Palfai, 2002; Sweeney, Pilliteri, & Kozlowski, 1996). Another important example of ambivalence is the fact that smokers typically behave differently with respect to self-reported evaluating that is framed in terms of (a) smoking (b) not smoking or (c) smoking versus not smoking (e.g. Brandon et al., 1996; Cano et al., 2014; Chassin et al., 2007; DiFranza et al., 2011; Kirchner & Sayette, 2007; Hine et al., 2009; Robinson et al., 2005; Rosenberg, 2009; Toll, Salovey, O'Malley, Mazure, Latimer, & McKee, 2008). Thus, without the ability to measure smoking-related evaluating non-relatively*,* the IAT seems particularly ill-equipped to understand what factors occasion a person to pursue versus resist addictive behaviours like smoking on different occasions (see Conner & Sparks, 2013; DiFranza, Ursprung & Carson, 2010; Lipkus, Green, Feaganes, & Sedikides, 2001; Piasecki et al., 2010; Sherman, Rose, Koch, Presson and Chassin, 2003, pp. 16-18, 31-33; Smith & Nosek, 2012; Sweeney, Pilliteri, & Kozlowski, 1996).

Granted, some researchers have attempted to adapt the IAT into a non-relative measure of implicit attitudes. However, none of these attempts have succeeded in disambiguating one attitude object from another while also disambiguating one attribute from another. For example, the first IAT variants, the Go/no-go Association Task (GNAT;

Nosek & Banaji, 2001) and the Single-Target IAT (ST-IAT; Wigboldus, Holland, & van Knippenberg, 2004; also known as the Single-Category IAT; SC-IAT; see Bluemke & Friese, 2008; Karpinski & Steinman, 2006), were both designed to measure implicit evaluating about one attitude object category at a time in positive versus negative terms. Thus, neither variant measures one attitude object relative to another, but both continue to measure one attribute category relative to another. To illustrate the core problem in practical terms, consider what it would mean if a participant categorized flowers with pleasant stimuli more quickly than with unpleasant stimuli. In principle, this type of comparison indicates that a participant associates flowers more strongly with "pleasant" than "unpleasant." However, a participant might strongly associate flowers with pleasant things when in comparison to unpleasant things, but nevertheless not associate flowers with pleasant things outside of such polarized comparisons (e.g. as when the relevant participant suffers from hay fever and normally avoids flowers but still prefer flowers relative to a negative attribute word like cancer which was used by Greenwald et al., 1998). In principle, therefore, the GNAT and the ST-IAT lack the ability to predict any behaviour that does not specifically involve a polarized comparison between positive versus negative attributes. In light of what we have already explained about the ambivalent nature of tobacco addiction, and about the fact that people behave differently when evaluating in (a) positive, (b) negative or (c) positive versus negative terms, the ST-IAT and GNAT are not likely to provide wholly adequate measures.

Other researchers have attempted to adapt two IAT variants to measure positive evaluating separately from negative evaluating. In broad terms, these IAT variants called the unipolar IAT (e.g. Jajodia & Earleywine, 2003) and the Brief IAT (Sririam & Greenwald, 2009), are both essentially the same as the original IAT format except that they are designed so that participants respond to one attribute category as being neutral.[19] Thus, for example, these modified tasks might involve associating flowers with pleasant versus neutral exemplars (thus avoiding the polar opposite attribute). The critical point here, is that if a unipolar or brief IAT was used to target smoking it might provide an indication of how positively (i.e. relative to some neutral category) a smoker implicitly evaluated smoking

[19] The unipolar IAT attempts to render the contrasting attribute category in the original IAT format as neutral by replacing it with a category of stimuli chosen as being neutral to the topic of interest, but the Brief IAT does so by instead instructing participants to respond to the contrast category stimuli as not belonging to either the target attitude object or the target attribute (i.e. as being neutral by virtue of irrelevance to the topic of interest; see Sririam & Greenwald, 2009; Teige-Mocigemba et al., 2010, p. 134).

relative to any other non-smoking activity, but it could not determine how much that smoker liked (a) smoking, or (b) the other activity, *per se* (see Friese & Fiedler, 2010; Rothermund & Wentura, 2010). Such distinctions are important, because it is possible for a smoker to implicitly like smoking more than a non-smoking activity but nevertheless not implicitly like smoking on other occasions outside of such comparisons (e.g. as when an addicted smoker wants to quit smoking but feels ill-equipped to face doing so; e.g. DiFranza et al., 2012). Once again, therefore, the unipolar and Brief IATs do not adequately address this subtle but practically important issue of relativity.

Finally, there are two rarely used IAT variants, called the *unipolar ST-IAT* (Thush & Wiers, 2007) and the *Function Acquisition Speed Test* (FAST; O'Reilly, Roche, Ruiz, Tyndall, & Gavin, 2012), that were created to provide fully non-relative measures of implicit evaluating.[20] Unfortunately, however, both measures inadvertently incorporate relative comparisons that are even more ambiguous with respect to evaluating than the relative comparisons employed by the original IAT format. Given the severe lack of research about either of these rare IAT variants we feel it would be an undue distraction to fully engage such issues here within the main text, and so instead we deal with them in Note 6, Appendix 1, along with additional collateral criticism of the unipolar IAT and Brief IAT.

In summary, therefore, all versions of the IAT either measure (a) one attitude object relative to another, (b) one attribute relative to another, or (c) both. Thus, at best, the IAT is limited by design to investigating "polarized" evaluative comparisons. Given that tobacco addiction is likely to involve many non-polarized forms of implicit evaluating, this means that the IAT is fundamentally limited in its ability to achieve criterion validity in relation to tobacco addiction. Indeed, bearing this point out, the research literature on smoking-related IATs is characterised by haphazard findings and disjointed theoretical controversies, instead of by research that systematically develops criterion validity with respect to tobacco addiction (e.g. see Roefs et al., 2011, pp. 178-181; Tibboel et al., 2011; Waters & Sayette, 2006). To illustrate these points further, we now review three intractable theoretical controversies that have dominated smoking-related IAT research to date, before then

[20] Other variants of the IAT do exist, such as the Recoding Free IAT (IAT-RF; Rothermund et al., 2009), or the Single Block IAT (Teige-Mocigemba et al., 2008), or the sorting-paired features task (SPF; Bar-Anan, Nosek, & Vianello, 2009; Bar-Anan & Nosek, 2014b). However, none of these remaining variants even attempt to resolve the relativistic nature of the original format IAT (see Teige-Mocigemba et al., 2010, pp. 131-134).

retrospectively synopsizing the central albeit tacit role played by the IAT's relativity in the escalation of these controversies. Thereafter, most crucially, we systematically review how such theoretical controversy has resulted in haphazard and heavily obfuscated findings about the criterion validity of smoking-related IATs.

2.2. Parsing Tobacco Addiction in Terms of Implicit Evaluating: A Systematic Review of the Evolution of Smoking-related IAT Research

How Theoretical Controversy involving the IAT's Relativity Drove Disjointed Research on Smoking-related IATs

One of the most robust and yet unexpected findings in the literature on smoking-related IATs is that smokers typically fail to respond more quickly to smoking/positive and contrast/negative key assignment schemes than to contrast/positive and smoking/negative key assignment schemes (for reviews see Roefs et al., 2011, pp. 178-181; Waters & Sayette, 2006, pp. 324-327). In other words, when using a standard format IAT researchers have found no evidence for a so-called implicit pro-smoking bias among regular tobacco users. Indeed, many researchers obtained significantly negative IAT effects from smokers and interpreted these as being implicitly anti-smoking (experiment 2, Andrews et al., 2010; Chassin, Presson, Rose, Sherman, & Prost, 2002, p. 492; experiments 1 & 2, Huijding, de Jong, Wiers, & Verkooijen, 2005; Perugini, 2005, p. 35; experiment 1, Rudman et al., 2008, p. 1704; experiments 1 & 2, Swanson, Rudman & Greenwald, 2001; Waters, Carter, Robinson, Wetter, Lam, & Cinciripini, 2007; Waters, Miller & Li, 2010), while most of the remainder obtained near zero IAT effects and interpreted these as indicating that smokers were implicitly ambivalent about smoking (experiment 1, De Houwer et al., 2006; experiment 1, Robinson et al., 2005; Macy, Chassin, Presson, & Sherman, 2015; experiments 1 & 2, Sherman, Rose, Koch, Presson, & Chassin, 2003; experiment 3, Swanson et al., 2001).

Although it is unclear why exactly smokers variously produced negative or near-zero IAT effects on the standard IAT, either way, it raised serious questions about whether implicit attitudes could play a causal role in tobacco addiction.[21] Moreover, the only time

[21] Anecdotally, the only consistent methodological difference between the negative versus near-zero findings was that the former used IATs which exemplified their concept categories in terms of word stimuli, but the latter used only IATs that exemplified their concept categories in terms of pictures. Critically, however, even if that were the reason why the literature obtained divergent findings, this fact would still not clarify how specifically participants evaluated smoking in either case.

that smokers have ever produced an apparently pro-smoking bias on a standard IAT was when the attitude object contrasted with smoking was more stigmatized (e.g. stealing; experiment 2, Robinson et al., 2005; Swanson et al., 2001). In response, the literature generally concluded that the IAT was consistently indicating that smokers lack pro-smoking implicit evaluating except in comparison to negative extremes. Thus, controversy ensued within the literature on smoking-related IATs about whether implicit attitudes are collateral versus integral to tobacco addiction.

The IAT's original proponents responded to the above findings by arguing that smoking-related implicit evaluating is not normally involved in tobacco addiction (e.g. Rudman, Phelan, & Heppen, 2008; Swanson et al., 2001). According to such accounts, the default is for smokers to implicitly dislike smoking as a function of negative childhood experiences with smoking and/or repeated experiences of society stigmatizing this addition. However, there is very little evidence to support this assertion, and the two research programmes that claim to provide evidence in this regard may be questioned. Swanson et al. (2001) used the data from various IATs to argue that smokers' implicit dislike of smoking is cognitively inconsistent with the fact that they implicitly identify with smoking, and in turn that this means smokers' implicit attitudes about smoking were likely to be inconsistent with and thus irrelevant to tobacco smoking itself. However, this empirical argument has no bearing upon the extent to which smokers might implicitly like smoking in other circumstances. Instead, Swanson et al. provide only post hoc, anecdotal evidence that smokers *might* implicitly dislike smoking by default.

In contrast, Rudman et al. (2008) did report an exclusive relationship between a smoking-related IAT and negative childhood experiences with smoking. Specifically, they found that their smoking-related IAT correlated significantly with the number of negative smoking-related childhood experiences smokers recalled by self-report, $r = .33$, while also not correlating with the number of recent positive smoking-related experiences those smokers' recalled by self-report, $r = .06$. However, even taking Rudman et al.'s correlation with retrospective self-reports at face value, this solitary finding does not address the possibility that smokers might often engage in pro-smoking implicit evaluating that is non-relative and therefore not detectable by the IAT.

Accordingly, some researchers refused to accept negative and/or near-zero IAT effects as conclusive evidence pro-smoking implicit evaluating is not involved in tobacco addiction. Instead, they speculated that the IAT might detect pro-smoking implicit evaluating among smokers if only its measurement effects were not so obscured by one evaluative confound or another. Thus, smoking-related IAT research diversified into three offshoots each seeking to demonstrate pro-smoking implicit evaluating among smokers using a different IAT variant. One of the first offshoots was inspired by an attempt to explain smokers' apparent lack of pro-smoking IAT effects in terms of Robinson and Berridge's (1993, 2003) well known incentive-sensitisation theory. In brief, incentive sensitisation theory marshals extensive neurobiological evidence to assert that addicts normally experience less pleasure (i.e. less liking) from addictive substances as they become more addicted to (i.e. motivated to consume) those substances. Crucially, Robinson and Berridge draw a clear evaluative distinction here between *liking* something versus *wanting* something. From this point of view, wanting (i.e. incentive) processes are thought to be central to addiction, but constructs based upon liking, such as implicit attitudes, are thought to be largely irrelevant except when addictions are first becoming established.

De Houwer et al. (2006, experiment 1) first tested these ideas with respect to smoking by juxtaposing a standard smoking-related IAT with a modified smoking-related IAT designed to target implicit wanting rather than implicit liking. Specifically, both IATs were exactly the same except that the wanting IAT replaced positive versus negative attribute exemplars with synonyms for approach versus avoid, respectively. De Houwer et al. found that smokers produced near-zero effects on the standard attitude IAT, but were significantly faster at responding to 'Smoking/Approach + Nonsmoking/Avoid' trials than to 'Nonsmoking/Approach + Smoking/Avoid' trials on the wanting IAT. Thus, confirming their expectations, De Houwer et al. interpreted these findings as indicating that smokers implicitly associated smoking more with approach than avoidance, but not more with liking than disliking or vice versa. However, in the only other study on the topic, Tibboel et al. (2011) modified a standard smoking-related IAT to use the labels 'I want' versus 'I do not want' instead of the attribute labels 'Positive' versus 'Negative' but did not find that smokers' resulting IAT effects differed from zero. Moreover, when Tibboel et al. compared smokers' wanting-IAT effects when satiated versus deprived of nicotine for 12 hours, contrary to incentive-sensitisation theory and/or the tenets of tobacco addiction, they found no significant difference. And yet, also contrary to incentive-sensitisation theory, they found that a corresponding IAT designed to measure implicit liking was nevertheless sensitive to the same nicotine deprivation manipulation.

Another important contradiction of incentive-sensitization theory was Tibboel et al.'s finding that their wanting-IAT correlated extremely highly with the corresponding liking-IAT (i.e. *r*s = .77-.78, depending upon nicotine satiation versus deprivation). In fact, Tibboel et al. found no significant main effects when comparing the liking- versus wanting-IATs in nicotine deprivation versus satiation conditions (*F*s < 1.91). Rather, both IATs displayed poor discriminant validity with respect to core variables, like nicotine deprivation, that are supposed to distinguish between smoking-related liking versus wanting (see Tibboel et al., 2011, p. 288-289). Similarly, even when De Houwer et al.'s wanting-IAT used different attribute exemplars to a corresponding smoking-related attitude IAT they still correlated significantly with each other, *r* = .39. In the end, such findings have raised serious questions about whether it is realistic to assume that verbally sophisticated humans customarily distinguish between liking versus wanting, at least in the same way as proposed by Robinson and Berridge. Namely, it is important to note that Robinson and Berridge formulated the distinction based on non-human animal research, which precludes a role for sophisticated verbal or cognitive processes (see Havermans, 2011; Tibboel et al., 2011, pp. 290-291; Tibboel, De Houwer, & Van Bockstaele, 2015). Therefore, however inconclusive the research is about standard smoking-related IATs, the research about smoking-related wanting IATs is even more inconclusive.

The second offshoot in smoking-related IAT research was inspired by findings that IATs are prone to measuring perceptions of societal mores as opposed to personal preferences when dealing with stigmatized topics (see Han et al., 2006; Olson & Fazio, 2003, 2004, 2006; Payne & Gawronski, 2010, pp. 3-4, 11; Tegie-Mocigemba et al., 2010, pp. 131-132). In an attempt to control for this possible confound, Olson and Fazio (2003; 2004) created the first 'personalized' IATs by removing error feedback from IAT trials, and by replacing the standard IAT's positive versus negative attribute category labels with the terms "I like" versus "I don't like". Given that smoking is frequently stigmatized, De Houwer et al. (2006, experiment 2) therefore hypothesised that an IAT variant capable of measuring personal implicit preferences to the exclusion of others' preferences might reveal pro-smoking IAT effects among smokers where the original IAT format had failed. And indeed, De Houwer et al. did find that regular smokers were significantly faster at completing the 'Smoking/I-like + Nonsmoking/I-dislike' key assignment scheme than its alternative.

Thus, whereas De Houwer et al.'s (2006) first experiment indicated that smokers were pro-smoking on a wanting-IAT, their second experiment indicated that smokers were also pro-smoking on a personalized-IAT. Importantly, De Houwer et al. took this as evidence that implicit liking was potentially as relevant to tobacco addiction as implicit wanting. However, when Tibboel et al. (2011) used a very similar smoking-related personalized IAT a few years later they did not replicate these findings. Instead, Tibboel et al. found that when regular smokers were satiated for smoking they produced near-zero effects on personalised IATs. Moreover, these researchers also found that when deprived of nicotine for at least 12 hours regular smokers responded faster to trials pairing smoking exemplars with the term 'I dislike' than to trials pairing smoking exemplars with the term 'I like'. Indeed, somewhat ironically, one personalized IAT that was designed to measure smokers' perceptions of the social consequences of smoking ended up reflecting smokers' perceptions of smoking-related stigmatization rather than their personal preferences about smoking (see Kahler, Daughters, Leventhal, Gwaltney, & Palfai, 2007). As such, much like the research on smoking-related wanting-IATs, the research on personalized smoking-related IATs is highly inconclusive about whether implicit evaluating is involved in tobacco addiction.

The third and final offshoot of the smoking-related IAT literature sought to uncover non-relative pro-smoking IAT effects among smokers by using the ST-IAT. Initially, Huijding and de Jong (2006) did find pro-smoking implicit attitudes among smokers with this measure (i.e. significantly faster responding on "Smoking/Positive versus Negative" key assignments than on "Smoking/Negative versus Positive" key assignments). However, using a similar ST-IAT, Glock, Müller, and Krolak-Schwerdt (2013) found near-zero effects among smokers. It is also worth noting that Bassett and Dabbs (2005) had also previously found near zero effects for smokers using the GNAT.

Overall, therefore, the literature on smoking-related IATs is characterised by three research offshoots that have each yielded confusing and inconclusive findings about what particular aspects of implicit evaluating are involved in tobacco addiction (see also Roefs et al., 2011, pp. 178-181; Waters & Sayette, 2006, pp. 324-327). From our privileged position of hindsight, this disappointing state of affairs could be seen as an inevitable consequence of the IAT's relativity. Indeed, it could be argued that the literature on smoking-related

IATs is confused, in large part, because its research is predicated upon the idea that the size and direction of smoking-related IAT effects reflect whether a person is implicitly pro-smoking versus anti-smoking or perhaps ambivalent about smoking. As explained above, however, the relativity of the IAT effect undermines the assumption that the polarity of such an effect captures how a smoker evaluates smoking in absolute terms (see Blanton & Jaccard, 2006). On balance, one could argue that IAT effects could still be useful if they are shown at least incidentally to have criterion validity with respect to tobacco addition. Unfortunately, however, the smoking-related IAT has demonstrated haphazard criterion validity across its research literature, and this is also in keeping with the idea that tobacco addiction involves many non-relative forms of evalauting which the IAT cannot.

On the Haphazard Criterion Validity of Smoking-related IATs.

The most popular method of testing the criterion validity of smoking-related IATs is in terms of known-group comparisons between smokers' versus non-smokers' IAT effects. Swanson et al. (2001) were the first to make such comparisons wherein they found that smokers implicitly identified with smoking significantly more than non-smokers (i.e. using a smoking-identity[22] IAT in experiment 2), and they also found that smokers implicitly liked smoking significantly more than non-smokers (i.e. with an attitude IAT in experiment 3). However, even if we disregard the relative nature of such findings for a moment, the key point here is that often they have not replicated in subsequent research, and even where they have there has been no systematic improvement in the magnitude of the known-groups effects achieved by smoking-related IATs since then.

For example, in cases where an IAT contrasts smoking with exercise, sweets or stealing the resulting measurement effects do not significantly distinguish smokers from non-smokers, and moreover it is unknown why this is the case (r = .10, study 1, r = .18, study 2, Swanson et al., 2001; r = .20, study 1, Robinson et al., 2005). Similarly, when Glock et al. (2013) used an ST-IAT to measure how much smokers versus non-smokers associated smoking with healthy versus unhealthy behaviours they found no significant difference (r = .06). Indeed, Larsen et al. (2014) used two different smoking-related Brief

[22] I will henceforth prefix the IAT with the term 'smoking-attitudes' versus other prefixes (e.g. 'smoking-identity'), as necessary, in order to distinguish smoking-related IATs that are designed to measure implicit attitudes (i.e. liking) about smoking versus those designed to measure other aspects of smoking-related implicit cognition (e.g. how much a person implicitly wants or identifies with smoking). As such, whenever the following text mentions an IAT it should be interpreted by default as referring to a 'smoking-attitudes' IAT unless otherwise specified.

IATs and both failed to significantly distinguish adolescent smokers from non-smokers (*p*s > .59, *r*s < .05). In another study, about child-related smoking, Chassin et al. (2002) unexpectedly found little or no relationship between either mothers' or fathers' smoking-related IAT effects and their respective smoking-status' (i.e. $.01 < \eta^2 < .02$, i.e. .0001 < *r*s < .0004, p. 491). Indeed, more generally, smoking-related IAT effects can vary as an extraneous function of the order in which key assignment schemes are presented to them (e.g. Kahler et al. 2007, p. 2017, $t(65) = 4.0$ $p < .001$; Sherman et al., 2003, p. 28, $r > .32$). Furthermore, Waters et al. (2010) found that non-smokers' smoking-related IAT effects gradually diminished upon repeated administrations so that they eventually became indistinguishable from those of smokers' (i.e. diminishing a once significant known-groups effect to non-significance).

Granted, smoking-related IAT effects have yielded significant known-groups effects about twice as often as not, but crucially, even among such statistically significant cases there is no evidence of a systematic improvement in the magnitude of the criterion effects achieved. Namely, Swanson et al. reported significant known-groups effect sizes between smokers versus non-smokers of $r = .34$ (i.e. using a smoking-identity IAT) and $r = .30$ (i.e. using a smoking-attitudes IAT) that are broadly of the same magnitude as the other significant differences obtained in subsequent smoking-related IAT research (i.e. .10 < *r*s < .52; a very small significant difference between children who had a family member that smoked versus those that did not, $r = .10$, but no significant difference between children who had tried smoking versus not, Andrews et al., 2010, p. 2399; smokers versus non-smokers, $r = .33$, Basset & Dabbs, 2005; smokers versus non-smokers, $r = .46$, Dal Cin et al., 2007; smokers versus non-smokers, $r = .39$, Huijding & de Jong 2006; smokers versus non-smokers, $r = .52$, Huijding et al., 2005, study 1, p. 952; smokers versus non-smokers, $r = .38$, Perugini et al., 2005; smokers versus non-smokers, $r = .52$, study 1, $r = .32$, study 2, Robinson et al., 2005; smokers versus non-smokers, $r = .27$, light versus heavy smokers, $r = .16$, non-smokers versus light/heavy and deprived/satiated smokers, .13 < *r*s < .28, Sherman et al., 2003, study 2, pp. 28-30). Thus, if anything, even the significant known-groups effects achieved with smoking-related IATs vary quite widely in their magnitude (i.e. .10 < *r*s < .52). Moreover, even after more than 13 years of research about the subject, the literature remains uncertain about what might be causing such variety in the magnitude of the known-groups effects as has so far been obtained (see Roefs et al., 2011, pp. 178-

181; Tibboel et al., 2011; Waters & Sayette, 2006).

Likewise, when it comes to considering the incremental known-groups validity of smoking-related IATs over analogous self-reported attitudes, the evidence is less than encouraging. In the only study of this kind, Perugini used logistical regression to demonstrate that a standard smoking-related IAT did not significantly distinguish smokers versus non-smokers (p = .31) beyond the significant level of prediction achieved by analogous self-reported attitudes (p = .001; combined R^2 = .55). Granted, when the interaction between the IAT and the relevant self-reports was entered as the second step in Perugini's logistical regression it did yield a marginally significant R^2-change = .05 (p = .06). However, even if we embrace this finding as encouraging evidence in favour of smoking-related IATs,[23] the fact remains that it is disjointed from other research in the area.

Moreover, when it comes to cross-sectional correlations between smoking-related IATs and tobacco addiction criteria only seven journal articles are relevant, and collectively they are less than conclusive in their findings. First, we consider those cross-sectional correlations that lend support to the criterion validity of smoking-related IATs, before then qualifying this supportive evidence with some other less favourable findings that are commonly overlooked by the extant literature. As detailed in Table 2.1, all seven articles in the area reported at least some significant cross-sectional correlations between smoking-related IATs and various criteria related to tobacco addiction. In summary, there are four cases of a smoking-related IAT correlating with self-reported tobacco consumption somewhere in the range, $.14 < r\text{s} < .42$, $p\text{s} < .05$; six cases of a smoking-related IAT correlating with clinical measures of nicotine dependence, $.24 < r\text{s} < .37$, $p\text{s} < .05$; and finally, four cases of a smoking-related IAT correlating with self-reported measures of tobacco craving, $.32 < r\text{s} < .45$, $p\text{s} < .05$.

In addition, Huijding and de Jong (2006, p. 185) also found that the IAT they employed significantly correlated with self-reported craving and nicotine dependence even where corresponding self-reported attitudes failed to do so. Similarly, McCarthy and Thompsen (2006, p. 439) showed via latent variable analyses that their positively-focused unipolar IAT significantly correlated with the Fagerström Test for Nicotine Dependence

[23] Self-reported attitudes about smoking are normally capable of distinguishing a very high percentage of smokers versus non-smokers (i.e. as evidenced in the first step of Perugini's logistical regression), and therefore it is perhaps impressive that Perugini's IAT managed at all to predict smoking status in addition to analogous self-reports.

(FTND; Heatherton, Kozlowski, Frecker, & Fagerström, 1991) and various aspects of tobacco consumption even controlling for the Smoking Consequences Questionnaire (SCQ; Copeland, Brandon & Quinn, 1995; Myers, McCarthy, MacPherson, & Brown, 2003), *path coefficient* = .15, $p < .01$, $N = 264$. Lastly, Chassin, Presson, Sherman, Seo, and Macy (2010, p. 674) found a small first-order correlation between smokers' self-reported plans to quit smoking in the following 18-months and their scores on a standard smoking-attitudes IAT, $r(447) = -.11$, $p < .05$. Though not strictly a criterion for tobacco addiction, self-reported intentions to quit smoking are reflective of tobacco addiction insofar as they indicate how much smokers still believe they retain at least some autonomy over their tobacco consumption.

Critically, however, Table 2.1 does not mention the many non-significant criterion correlations that also resulted from the articles it includes. Namely, five of the seven articles listed in Table 2.1 involved, but downplayed, non-significant cross-sectional correlations between smoking-related IATs and tobacco addiction criteria. In fact, in each case there were a comparable number of significant versus non-significant criterion correlations, but only the former findings were emphasized in the relevant articles. For example, Tibbeol et al. (2011) only reported significant correlations between their wanting-IAT, their liking-IAT and various self-reported urges to smoke, without ever reporting the details of a similar number of criterion correlations involving the same variables that did not achieve statistical significance (see p. 288). In addition, Kahler et al. (2007, p. 2072) found no significant correlation between their smoking-related social consequences IAT and the social subscale of the Smoking Motivation Questionnaire, nor with the FTND, and nor with CPD ($|r|'$s < .10, *p*s > .10). Similarly, Waters et al.'s (2007, p. 183) generalized estimating equation (GEE) analyses found no significant relationship between the smoking-attitudes IAT they used and the number of years a person had been smoking, and nor with CPD (*p*s > .05). Indeed, in one other study that failed to provide any positive finding to Table 2.1, Larsen et al. (2014) found smoking-related Brief IAT scores did not correlate with either CPD or mFTQ scores in a sample of adolescent smokers. Indeed, even Huijding and de Jong's (2006) headline finding that their smoking-attitudes IAT correlated significantly with self-reported cravings is questionable upon closer scrutiny. As Huijding and de Jong themselves admit in a footnote (p. 185), the correlation they found between their smoking-attitudes ST-IAT and self-reported craving only held when self-reported craving was measured just after that ST-IAT was implemented and not before. In their footnote, Huijding and de Jong interpreted this finding as indicating that the smoking-related pictures incorporated into their ST-IAT induced self-reported cravings at follow-up. Thus, at best, Huijding and de Jong's research (even if not their findings) suggests that the ST-IAT did not measure implicit evaluating related to pre-existing cigarette cravings, but instead merely induced artificial changes in self-reported cigarette craving that were correlated with the measurement effects it produced.

Table 2.1

Cross-sectional correlations for, but not against the criterion validity of smoking-related IATs (i.e. excluding non-significant cases).

Tobacco Addiction Criterion Variable	Type of Smoking-related IAT	Statistics	Originating Study
Questionnaire Measures of Tobacco Consumption, Relapose & Abstinence			
Cigarettes smoked per day (CPD)	Standard	$r(447) = .14, p < .05$	Chassin, Presson, Sherman, Seo, & Macy, 2010, p. 674
CPD	Standard	$r(32) = .29, p < .05$	Swanson et al., 2001, p. 225 (experiment 3)
CPD	Smoking-identity	$r(32) = .42, p < .005$	Swanson et al., 2001, p. 225 (experiment 3)
Cigarettes smoked per week	Positively-focused unipolar IAT	$.29 < r(262)\text{s} < .37$, $p\text{s} < .01^a$	McCarthy & Thompsen, 2006, p. 439
Time to relapse during unaided smoking-cessation	Approach/avoidance IAT measuring trait experiential avoidance	$r(30) = -57$, $p < .001$	Cameron, Reed, & Ninnemann, 2013, p. 2909

Tobacco Addiction Criterion Variable	Type of Smoking-related IAT	Statistics	Originating Study
Clinical Measures of Nicotine Dependence			
The modified Fagerström Tolerance Questionnaire (mFTQ)	Single-target IAT	$r(31) = .26, p < .05$	Huijding & de Jong, 2006
The Fagerström Test for Nicotine Dependence (FTND)	Positively-focused unipolar IAT	$.29 < r(262)\text{s} < .37$, $ps < .01$[a]	McCarthy & Thompsen, 2006, p. 439
FTND	Standard	$p < .05$	Waters et al., 2007
Social motivations subscale of the Wisconsin Inventory of Smoking Dependence Motives (WISDM)	Personalized IAT	$\lvert r(65) \rvert = .24$,[b] $p < .05$	Kahler et al., 2007, p. 2072
The Smoking Consequences Questionnaire (SCQ)	Standard	GEE analyses, $p < .05$	Waters et al., 2007
The short-SCQ	Positively-focused unipolar IAT	$.29 < r(262)\text{'s} < .37$, $ps < .01$[a]	McCarthy & Thompsen, 2006, p. 439

Tobacco Addiction Criterion Variable	Type of Smoking-related IAT	Statistics	Originating Study
Measures of Tobacco Craving			
Self-reported cravings after IAT	Single-target IAT	$r(31) = .39$, $p < .05$	Huijding & de Jong, 2006
Questionnaire of Smoking Urges (QSU)	Standard	GEE analyses, $p < .05$	Waters et al., 2007
Various measures of nicotine craving including the QSU in nicotine-deprived and/or satiated conditions.	Wanting-IAT	rs = .32-.45, ps < .05	Tibboel et al., 2011
Various measures of nicotine craving including the QSU in nicotine-deprived and/or satiated conditions.	Liking-IAT	rs = .32-.42, ps < .05	Tibboel et al., 2011

[a] McCarthy and Thompsen (2006) did not identify individual correlations between their positively-focused unipolar IAT versus CPD, the FTND or the short-SCQ. [b] Kahler et al. (2007, p. 2072) reported a negative correlation, $r(65) = -.24$, $p < .05$, between a reverse scored smoking-related social consequences IAT and a nicotine dependence subscale designed to measure social motivations for smoking (i.e. smokers who found it easier to pair smoking with negative, rather than positive, social consequences tended to report less social motivations for smoking).

When McCarthy and Thompsen (2006, p. 441) estimated how much their smoking-related IAT correlated with measures of tobacco consumption after controlling for the SCQ it resulted in a significant but very small path coefficient of just .15. In addition, they also found that unlike the positively-focused unipolar smoking-related IAT they used, their corresponding negatively-focused unipolar IAT failed to correlate with the SCQ, the FTND or with measures of weekly tobacco consumption. Granted, this might be interpreted as discriminant validity for the positively-focused unipolar IAT McCarthy and Thompsen used, in that it confirms that positively-framed evaluating is more relevant to addiction than negatively-framed evaluating, as had been suggested in earlier research on alcohol dependence (p. 441). However, even accepting this argument (i.e. by ignoring the relativity of unipolar IATs), it then becomes puzzling that there has been no research published on smoking-related unipolar IATs during the eight years since McCarthy and Thompsen (2006).

As inconclusive as the aforementioned literature involving cross-sectional correlations is, the research examining prospective correlations between smoking-related IATs and tobacco addiction criteria is even less conclusive. Namely, that literature only includes three cases of a smoking-related IAT prospectively predicting a tobacco addiction criterion (and never vice versa). Firstly, Kahler et al. (2007) found that their smoking-related social consequences IAT prospectively predicted the success of a standard smoking cessation treatment at two- and eight-week follow-ups ($r(65) = .35, p < .01$, and $r(65) = .35, p < .10$, respectively).

Secondly, Chassin et al. (2010) conducted an internet-based survey of smokers from the general population and found that their smoking-attitudes IAT scores at baseline prospectively predicted how likely they were to be abstinent from smoking 18 months later (i.e. the more ostensibly ‘pro-smoking’ a smoker’s IAT effects the less likely they were to be abstinent 18 months later, $r(447) = -.17$, $p < .001$, p. 674). Putting aside the very small magnitude of their finding for a moment, admittedly, Chassin et al. did make some attempt to systematically analyse the prospective correlation they obtained. Namely, they used complex logistical regression to model how baseline variables might be moderating the relationship between their smoking-attitudes IAT and abstinence at 18 months follow-up. The supplementary analyses suggested that those with more anti-smoking implicit attitudes are more likely to be abstinent from smoking in the long-term when they had many previous failed attempts but still planned to quit smoking regardless, or when they started out with no plan to quit and had little experience of failing to quit.[24] However, this systematic analysis is of course not the same thing as conducting systematic research on the matter. Rather, despite numerous competing explanations for Chassin et al.'s findings, neither they, nor anyone else subsequently, has attempted to tease apart these possibilities via systematic research.

Thirdly, using structural equation modelling Sherman et al. (2009, p. 316) found that children's smoking-attitudes IAT effects at baseline significantly predicted whether they were one of the 15% who initiated smoking 18 months later (with *path coefficients* ≈ .16-.22). In addition, Sherman et al. used a significant *path coefficient* ≈ .18-23 between children's versus mothers' or fathers' smoking-attitude IAT effects to suggest that they had demonstrated the 'intergenerational transmission of implicit attitudes' from mother/father to child, and that this increased a child's likelihood of initiating smoking. However, this secondary assertion is nothing more than a post-hoc speculation given that it is (a) founded upon a cross-sectional correlation instead of upon a prospective correlation or experimental methods, and (b) given that it is an unprecedented finding. A related point is that Chassin et al. (2002) made similarly unfounded claims as Sherman et al. (2009) about the potential for a prospective relationship between mothers' smoking-related implicit attitudes and their children's likelihood of smoking. Namely, using a cross-sectional χ^2 correlation, Chassin et al. claimed that children were significantly more likely to smoke when their mothers produced smoking-attitudes IAT effects that were more pro-smoking; and in turn, they used this finding to claim that mothers' implicit attitudes influenced their children's smoking (p. 493). However, even putting aside the relativity of the IAT they used (and thus its zero point), and also the fact that the correlation Chassin et al. observed was small and cross-sectional (*odds ratio* = 1.41, $p < .03$, p. 493), their claim still ignores multiple null relationships that bear directly upon it. For example, they observed a near zero cross-sectional correlation between children's versus mother's smoking-related IAT effects, $r(444) = .15$, $p < .05$; null cross-sectional relationships between children's IAT effects with their father's IAT effects, $r(444) = .06$, $p > .10$, and also with both mother's and father's smoking-status (p. 491); a null relationship between fathers' IAT effects and children's smoking; and indeed a null correlation between children's smoking and their own smoking-related IAT effects (p. 493).

[24] Specifically, Chassin et al. found that their IAT only predicted 18-month abstinence among those high in past experiences of quitting failures but who nevertheless had a plan to quit smoking at baseline, $B = -1.20$, $p = .004$, *odds ratio* = .30, and among those who had no plan to quit smoking at baseline but who had little previous experience of failing to quit smoking, $B = -.41$, $p = .10$, *odds ratio* = .67.

Likewise, Dal Cin et al.'s (2007) study examining implicit smoking-identity was another study that mistakenly used cross-sectional correlations to make causal arguments about smoking-related IATs. Their basic claim was to have found evidence that the extent to which undergraduate smokers implicitly identified with smoking at the start of a college semester (on a smoking-identity IAT) determined the extent to which those undergraduates' intentions to smoke increased across the relevant semester. Dal Cin et al. solely based this assertion upon their regression finding that smokers' smoking-identity IAT effects served as a large and unique predictor of the relevant changes in smokers' smoking intentions, $t(20) = 3.30$ ($r \approx .59$). Critically, however, the researchers did not deliver their smoking-identity IAT until after the relevant changes in smoking-related intentions had already occurred. Thus, as per the longstanding scientific convention that causes must precede their effects, Dal Cin et al.'s central claim was ill-founded (e.g. Hempel, 1970; Wagenmakers, Wetzels, Borsboom, & Van Der Maas, 2011).

Moreover, even if we were to assume for a moment that smokers' implicit smoking-identity remained unchanged across the relevant college semester, various methodological ambiguities would still prevent us from making unequivocal claims about stronger implicit smoking-identity leading to greater increases in smoking intentions. Most notably, the researchers' regression did not take into account that the changes they recorded in self-reported smoking intentions were obtained across an unspecified time within a college semester that ended with one of two different types of film clip (i.e. glamorising smoking versus not). This oversight is particularly perplexing when one considers that Dal Cin et al.'s preceding statistical analyses indicated that the type of film clip used significantly moderated implicit smoking-identity in the relevant sample (p. 561; but see Moore, 2003; Skinner, 1981).

Granted, Cameron, Reed, and Ninnemann (2013) did find a large prospective relationship between smokers' implicit evaluating on their approach/avoidance IAT and how long in hours those smokers managed to sustain an unaided attempt at smoking-cessation (see Table 2.1). Crucially, however, the relevant IAT did not measure smokers' implicit evaluating of smoking, or quitting smoking, but instead their implicit evaluating of experiential avoidance in general (i.e. without reference to smoking). And as such, they did not establish a prospective relationship between smoking-related implicit evaluating and tobacco addiction.

Furthermore, a general issue that is of concern in this area is that smoking-related IATs are relatively poor at prospectively predicting even their own effects.

Namely, on the two occasions where test-retest reliability for smoking-related IATs has so far been recorded it ranged lowly between r = .20-.56 (i.e. *test-retest* r = .20, .29 over one week for a standard smoking-IAT , Andrews et al., 2010; *test-retest* r = .56, .53 over one month respectively for a positively-focused versus negatively-focused unipolar IAT, McCarthy & Thompsen, 2006). Thus, even if smoking-related IATs did happen to correlate with tobacco addiction criteria on any given occasion, researchers would be highly unlikely to replicate any such findings on any given attempt (see LeBel & Paunonen, 2011). On balance, the test-retest reliability of a smoking-related IAT might fluctuate due to any of a range of background variables that once identified, would in principle allow researchers to achieve higher levels of test-retest reliability with smoking-related IATs (i.e. by experimentally controlling the relevant background variables). Critically, however, there is very little research examining how contextual variables moderate smoking-related IATs, and moreover as we will now review, what little research does exist on the matter is inconclusive in its findings.

The final, and perhaps most concerning, aspect of extant research findings about smoking-related IATs is the fact that they exhibit poor sensitivity to the experimental manipulation of variables that are integral to tobacco addiction. For example, Huijding et al. (2005, experiment 2) found that smokers' smoking-related IAT effects did not vary between typical smoking versus non-smoking contexts, $F(1, 38) < 1$ (i.e. $r < .16$). Similarly, both of Lochbuehler et al.'s (2013) studies indicated that children's smoking-attitudes IAT effects were not affected by whether they had just watched and/or enjoyed film clips where the lead characters were depicted smoking, Fs < .05 (i.e. rs < .02). Granted, Dal Cin et al. (2007) found that the extent to which people self-reported that they identified with a tobacco-smoking hero in a film clip significantly moderated the smoking-identity IAT effects, $t(47) = 2.05$ (i.e. r = .29 without any significant interaction with smoking-status). However, this finding is not only unprecedented and unrepeated but moreover it was obtained without assessing smokers' implicit attitudes toward smoking (i.e. the IAT assessed implicit identity), and with a male-only undergraduate sample that had an average age of just 19.7 years. As such, we must regard Dal Cin et al.'s finding that a smoking-identity IAT is moderately sensitive to smoking-related social contexts as being both preliminary and of questionable scope (e.g. it may not necessarily generalise to smokers' implicit *attitudes* about smoking nor to more established or female smokers). As such, smoking-related IATs must be regarded as being relatively insensitive to smoking-related contextually cuing.

On the matter of whether smoking-related IATs are sensitive to nicotine deprivation, the findings are again mixed. Waters et al. (2007) found that smokers produced moderately more pro-smoking IAT effects having smoked a cigarette 40 minutes beforehand as compared to when they had not, $F(1, 52) = 4.48$, $p < .05$ (i.e. $r \approx .28$). And indeed, this comports well with the significant main effect that Rydell, Sherman, Boucher and Macy (2012) obtained on their smoking-attitudes IAT between participants who were allowed to smoke just beforehand versus not, $F(1, 40) = 4.22$, $p = .05$, $\eta_p^2 = .09$ (i.e. $r \approx .30$).[25] Somewhat puzzlingly, however, Waters et al. also found that the impact of having smokers abstain from smoking for 12 hours had much the same impact on their smoking-attitudes IAT effects, $F(1, 52) = 4.61$, $p < .05$ (i.e. $r \approx .29$). And moreover, Waters et al. found no interaction on their smoking-attitudes IAT between their 12-hour abstinence manipulation and whether smoking was permitted 40 minutes before the experimental session, $p > .20$. In fact, post hoc contrasts revealed that the only experimental condition that produced IAT effects that were significantly different from any of the other three conditions was when smokers smoked 40 minutes beforehand having not abstained from nicotine, *p*s < .05 (i.e. *r*s > .36). In other words, Waters et al.'s IAT reported no impact of smoking 40 minutes beforehand among 12-hour abstainers, nor any impact of 12-hour abstaining among those who did not smoke beforehand, *p*s > .10. However, it is important to note that Waters et al. may have mistaken ruled out at least one moderate-sized effect in these latter cases. Namely, the relevant *p* values coupled with their appropriate degrees of freedom imply *r*s < .31, and this comports well with the similarly sized significant main effects Waters et al. had earlier ascribed to both experimental variables (see above).

On balance, therefore, Waters et al.'s findings provide only equivocal evidence that smoking-related IATs are sensitive to smokers' nicotine deprivation. Likewise, Tibboel et al. (2011) revealed that abstaining from smoking for 12 hours had almost no impact upon smokers' IAT effects whether measured in terms of liking, $t(43) = 2.10$ (i.e. $r \approx .22$), or in terms of wanting, $t(47) < 1$ (i.e. $r \approx .14$). And finally, worse still, Sherman et al. (2003, *p*. 28) found that abstaining from smoking for four hours had no discernible impact upon a smoking-attitudes IAT effect produced by smokers (note that none of the relevant inferential statistics were reported). Thus, at best, smoking-related IATs are rather hit and miss when it comes to detecting different levels of nicotine deprivation.

[25] Note that Rydell et al. described this experimental manipulation as a form of nicotine deprivation but that this is a highly questionable assertion given that their participants in both conditions were free to smoke up until shortly before completing the relevant smoking-related IAT.

Finally, as further evidence that researchers have yet to identify clear controlling variables for smoking-related IAT effects, existing attempts to bring about lasting changes in these effects have mostly failed. In the first such attempt, Czyzewska and Ginsburg (2007, p. 120) found that it made no difference to smokers' smoking-related IAT effects when they had just watched a 15 minute video of public service announcements against tobacco versus marijuana. Later, Andrews et al. (2010, experiment 2) found that some anti-smoking games produced marginal changes in children's smoking-related IAT effects, *F*s < 3.24 (i.e. *r*s < .23), one-tailed *p*s < .10, but that many more did not, one-tailed *p*s > .10 (i.e. from pre- to post-intervention and in comparison with a control game). Indeed, these disappointing findings are all the more remarkable when one considers the fact that the sample involved did not even have a history of smoking to bolster their smoking-related IAT effects against the relevant games (i.e. a sample of children with an average age of 12 years with only 4% of whom had previously tried smoking).

After that, Rydell et al. (2012) claimed to have found evidence that when smokers watched a brief but strongly worded anti-smoking video clip it made their smoking-related IAT effects more anti-smoking in comparison to a weakly executed anti-smoking video clip, but only when they had just smoked a cigarette, $t(41) = 2.54$, $p = .02$ (i.e. $r \approx .37$). However, this causal finding was ill-founded given that, much like Dal Cin et al., Rydell et al. did not deliver their smoking-related IAT to smokers until after the relevant changes were supposed to have taken place. In other words, Rydell et al. did not determine whether the relevant differences they obtained reflected changes in smoking-related implicit attitudes induced by the strongly worded video clip, or merely initial differences in sampling. Moreover, even if we put aside this issue, it is important to recognise that Rydell et al.'s significant finding here was merely a post hoc contrast plucked from within a non-significant main effect of message-strength, $p > .05$ (other statistics not reported), and from within a non-significant interaction between message-strength and whether or not the relevant smokers smoked just before receiving their anti-smoking message, $F(1, 40) = 1.20$, $\eta_p^2 = .03$ (i.e. r ≈ .17). As such, Rydell et al.'s key claim is not only ill-founded but it is also based upon data that might very well be a cherry-picked artefact of statistical familywise error.

Taking a slightly different approach, Glock et al. (2013) computed a regression of smokers' smoking-related IAT effects onto the number of cigarettes they smoked per day (CPD) to determine if anti-smoking warnings would diminish this relationship. The researchers found that smokers' IAT effects did not regress upon CPD when they had just been confronted with a succession of anti-smoking warnings, $\beta = -.24$, $p > .05$, but that they did when smokers had not been confronted with such warnings, $\beta = .31$, $p < .05$. However, Glock et al. did not statistically test their core assertion as to whether those two correlations were significantly different from each other. Indeed, it was not even possible to compare the relative magnitude of those relationships because Glock et al. bundled them as part of a multiple regression with three other predictors measuring the self-reported extent to which those smokers engaged in compensatory health behaviours to justify their continued smoking (i.e. doing exercise, eating healthily and reducing how much they smoked without quitting entirely).

Furthermore, as in all but one of the aforementioned studies (i.e. Andrews et al., 2010), Glock et al. only measured smokers' IAT effects just after the relevant anti-smoking warnings. Consequently, their findings cannot rule out initial sampling differences as the source of the reduced beta weights. Worse still, Glock et al. did not use a control condition but instead merely refrained from delivering the relevant anti-smoking warning messages to one group of smokers. As such, these researchers made no experimental distinction between whether the relationship they observed between smoking-related IAT effects and CPD was diminished by the anti-smoking content of the relevant warnings or perhaps merely as a function of receiving any information about smoking (i.e. regardless of whether it was pro- versus anti-smoking versus relatively neutral).

In one other study, Macy et al. (2015) did ensure the delivery of a smoking-related IAT to smokers both before and after they had received either an anti-smoking treatment versus a suitable control treatment. They found that a web-based approach-avoidance task designed to train avoidance of smoking-related stimuli brought about a moderate-to-small reduction in smokers smoking-related IAT effects, $F(1, 267) = 14.70$, $p < .001$, $\eta_p^2 = .05$ (i.e. $r \approx .22$). However, that main effect was qualified by two significant interactions that unexpectedly limited the success of the relevant approach-avoidance task to just two smoker sub-groups. Namely, the smoking-avoidance treatment only brought about anti-smoking changes in IAT effects for smokers who at three months previously, had planned to quit smoking or were without an educational attainment greater than high school; and even then the smoking-avoidance treatment effect was relatively borderline in both cases (i.e. η_p^2s = .04, .05, respectively). Moreover, an accompanying anti-smoking treatment based upon public service announcements had no impact whatsoever on smokers' smoking-related IAT effects (the relevant statistics were not reported).

Thus, in summary, smoking-related IATs might exhibit a moderate degree of known-groups validity (i.e. even if somewhat haphazardly), but when it comes to evidence that they correlate with other tobacco addiction criteria, whether in cross-section or prospectively, the extant literature is highly inconclusive. At best, smoking-related IATs achieve medium-sized correlations with tobacco addiction criteria, and often these relationships are not replicated. And more to the point, what little research does exist on the subject is certainly not systematic. This is perhaps most evident in the small research literature attempting to bring about lasting changes in smoking-related IAT effects. Despite nine years of research on the topic, so far only two studies have experimentally demonstrated an intervention changing smoking-related IAT effects (i.e. Andrews et al., 2010, experiment 2; Macy et al., 2015). And moreover, in both cases the treatment effects on the relevant IATs were small, and exceptions among a larger collection of unexpected null treatment effects. It is thus possible that the only existing evidence that smoking-related IAT effects can be modified by researcher intervention are merely statistical outliers around zero rather than bone fide treatment effects.

Furthermore, a major issue that is outstanding even within the wider IAT literature is that researchers generally fail to check how long the relevant intervention-induced changes persist on smoking-related IATs. Crucially, without such follow-up research the IAT literature simply cannot distinguish between experimental effects that reflect genuine psychological change (e.g. learning) versus experimental effects that merely reflect contextual cueing of pre-existing psychological processes (see De Houwer, Barnes-Holmes, & Moors, 2013; Han et al., 2010; Lochbuehler et al., 2013; Webb, Sheeran, & Pepper, 2012; Wiers, Gladwin, Hofmann, Salemink, & Ridderinkhof, 2013; Wiers & Stacy, 2010, pp. 566-567). And lastly, further research would still be needed to determine how well the relevant treatment effects generalised beyond laboratory settings to tobacco addiction per se.[26]

[26] In particular, this last point challenges the scope of findings wherein anti-smoking treatments are designed to train smokers how to respond in an anti-smoking manner on computerized classification tasks like those employed by IATs, but without ever providing evidence that such context-specific training generalises to reduce tobacco addiction in non-laboratory settings. To illustrate, Macy et al. obtained *some* treatment effects on their smoking-related IAT with a smoking-avoidance intervention that they specifically designed to employ stimuli that were similar to those used in the relevant smoking-related IAT, but importantly, when it came to more naturalistic aspects of smoking behaviour the same treatment did not even manage to increase smokers' willingness to receive links to websites with tips to help them quit smoking. Indeed, even putting aside reference to smoking-related IATs, preliminary evidence has only just begun to emerge showing that smoking-avoidance techniques might be effective on smoking, at least minimally (see Wittekind, Feist, Viertel, Moritz, & Fritzsche, 2015).

2.3. A Recap of the IAT's Shortcomings in Relation to Tobacco Addiction

The IAT is incapable of measuring implicit evaluating in a non-relative fashion and as a result it cannot distinguish among the many non-polarized forms of evaluating commonly involved in behavioural criteria. In particular, tobacco addiction is a condition that is characterised by the ambivalent interplay of contrasting forms of non-polarized evaluating from situation to situation (e.g. a smoker might vary between engaging in positively-framed pro-smoking evaluating when socializing with smokers but engage in negatively-framed anti-smoking evaluating during sporting activities that highlight the ongoing physical costs of smoking). Thus, the IAT is fundamentally incapable of parsing up tobacco addiction in terms of implicit evaluating because it does not distinguish among different types of implicit evaluating (i.e. in the sense of addressing different topics) that seem likely to be involved in tobacco addiction. Bearing this out, the literature on smoking-related IATs is dominated by disjointed and intractable controversies about whether implicit evaluating is even involved in tobacco addiction or not. Moreover, researchers have only ever demonstrated criterion validity for smoking-related IATs as an incidental by-product of these assorted controversies. Having initially happened upon modest and often unstable levels of criterion validity, without ever determining what specific topics the relevant implicit evaluating addressed in achieving that criterion validity, researchers have thus been unable to demonstrate any further progress in this regard. In fact, on balance, with respect to achieving critierion validity in relation to tobacco addiction, or with respect to distinguishing between competing theories about implicit evaluating in this domain, the IAT appears no better equipped than evaluative priming.

CHAPTER 3: Undercurrents in the Research on Smoking-related Implicit Cognition: A Comprehensive Review of Miscellaneous Alternatives to the IAT

Collectively, the IAT and cue-based measures are the most established and popular measures of addiction-related implicit cognition. However, it appears that both approaches are severely limited in their ability to hone theory or to systematically develop criterion validity on addiction. Therefore, we now consider the only two remaining alternatives that were routinely promoted within the literature prior to the emergence of the IRAP (see Gawronski & De Houwer, 2014; Nosek et al., 2011; Roefs et al., 2011, pp. 151-155; Rooke et al., 2008; Stacy & Wiers, 2010, pp. 558-562; Waters & Sayette, 2006, p. 324). The first of these alteratives is an assortment of various movement-based tasks that we term *approach/avoidance tasks* (AATs), and which were designed to measure a persons' implicit tendency to approach versus avoid addiction-related cues (Watson, de Wit, Hommel, & Wiers, 2012; Watson, de Wit, Cousijn, Hommel, & Wiers, 2013). The second remaing alternative, is an assortment of various questionnaire-based tasks that we term e*xpectancy accessibilty tasks* (EATs) which were designed to measure complex forms of implicit evaluating about addiction-related cues using *indirect* self-reports (i.e. based on questions that do not ask participants directly about the topic of interest; McKee et al., 2003; Read, Wood, Leiuez, Palfai, & Slack, 2004). Then lastly, having reviewed how the foregoing alternatives suffer from many of the same experimental ambiguities as the IAT and its predecessors, we offer the IRAP as a third, more promising alternative. In particular, we will provide a brief summary of the main theoretical rationales underpinning the IRAP, explaining why it is relatively uniquely equipped as a means of systematically refining our experimental understanding of tobacco addiction (and thus of how best to treat it).

3.1. Measuring Automatic Approach versus Avoidance to Tobacco Smoking Cues as a Means of Parsing Tobacco Addiction

Despite having a relatively small research base, AATs are usually granted prominence in contemporary reviews of addiction-related implicit evaluating (e.g. Roefs et al., 2011; Rooke et al., 2008; Stacy & Wiers, 2010; Watson et al., 2013). Thus, for present purposes it is worth at least considering: (a) what level of criterion validity AATs have so far exhibited in relation to tobacco addiction; and (b) what potential there is to further improve any such criterion validity. In practice, AATs have primarily been used to measure automatic motivational processes as criteria that could be used to validate various measures (and/or theories) of addiction-related attention (e.g. Larsen et al., 2014; Mogg et al., 2003; Watson et al., 2013). Indeed, addiction researchers have not only used AATs to measure motivational processes as distinct from attention processes, but sometimes as distinct entirely from mental processes (e.g. Bradley et al., 2008; Thewissen, Havermans, Geschwind, van den Hout, & Jansen, 2007; see Krieglmeyer & Deutsch, 2010; Mogg et al., 2005; Van Dessel, De Houwer, Gast & Smith, 2015; Van Dessel, De Houwer, Roets, & Gast, 2015; Watson et al., 2012). Bearing this point out, AATs are most commonly referred to as measuring *action tendencies* rather than implicit cognition (Stacy & Wiers, 2010, pp. 557-558).

Crucially, however, few had even questioned the criterion validity of AATs until very recently, much less conducted research dedicated to testing that validity (see Watson et al., 2012). Instead, what little data do exist on the subject were usually gathered inadvertently as part of wider research agendas seeking to resolve theoretical controversies about various attention and/or other cognitive processes (i.e. without considering the bearing of these findings on the validity of the relevant AATs; see Larsen et al., 2014; Mogg et al., 2005; Watson et al., 2012; Watson et al., 2013). Moreover, such data certainly do not suggest that AATs might possess any greater criterion validity for tobacco addiction than corresponding IATs or cue-based measures.

A Systematic Review of the Criterion Validity of Approach/avoidance tasks (AATs) in relation to Tobacco Addiction

There are currently five studies demonstrating that AATs are capable of distinguishing between groups with different smoking histories. Four of these studies employed an AAT known as the *Manikin* or *Stimulus Response Compatibility* task (SRC; De Houwer, Crombez, Baeyens, & Hermans, 2001; i.e. smokers versus never-smokers, $F(1, 36) = 14.70$, $p < .01$, $r \approx .54$, Bradley et al., 2004; smokers versus never-smokers, $F(1, 41) = 5.82$, $p < .01$, $r \approx .35$, Bradley et al., 2008; smokers versus never-smokers, $F(1, 37) = 6.90$, p < .01, $r = .40$, Mogg et al., 2003; low versus moderately dependent smokers, $F(1, 37) = 7.01$, $p < .05$, $r \approx .40$, Mogg et al., 2005), and one remaining study employed a relatively novel task variously known as the *zooming joystick approach/avoidance task* or the *zooming joystick AAT* (we introduce the acronym zAAT to include all AATs with zooming functionality; Krieglmeyer & Deutsch, 2010; Rinck & Becker, 2007; Watson et al., 2013; i.e. smokers versus never-smokers, $t(42) = 1.74$, $p = .04$, $r \approx .26$, smokers versus ex-smokers, $t(42) = 1.72$, $p = .05$, $r \approx .26$, Wiers, Kuhn, Jovadi, Korucuoglu, Wiers, Walter, Gallinat, & Bermpohl, 2013). Thus, according to these findings AATs appear to provide a moderate to large degree of known-groups validity with respect to tobacco addiction.

Upon closer inspection, however, the matter is far from clear cut. Though not acknowledged as such, elements of the foregoing studies suggest that AATs are prone to obtaining null, and indeed sometimes reversed smoking-related known-groups effects. For example, Bradley et al. (2008) may have obtained a significant known-groups effect between smokers versus never-smokers with their SRC, but only when they scored it in terms of an ad hoc sub-set of the smoking-related cues it involved. Namely, when they scored the SRC in terms of cues that non-smokers had rated negatively (and that smokers had rated neutrally) they obtained statistical significance for the relevant known-groups effect, $F(1, 41) = 5.82$, $p < .01$, $r \approx .35$. However, this was not the case when they scored the SRC in terms of the remaining smoking-related cues which the non-smokers had rated neutrally (and the smokers had rated positively), $F(1, 41) = 1.24$, $p = .27$, $\mathrm{r} \approx .17$, nor when they pooled both subsets of smoking-related cues together, $F(1, 41) = 3.23$, $p = .08$, $r \approx .27$.[27]

And although Bradley et al.'s (2008) findings arguably tally (indirectly) with the fact that both Bradley et al. (2004) and Mogg et al. (2003) had earlier obtained significant SRC differences between smokers versus non-smokers when non-smokers rated the relevant smoking-related cues negatively, neither of these preceeding studies compared how that SRC performed with smoking-related cues that non-smokers had rated otherwise. Moreover, Mogg et al. (2005) obtained a significant known-groups effect on a smoking-related SRC even though the groups they contrasted both rated its constituent smoking-related cues in a similarly positive fashion. Thus, it appears that participants' ratings of an SRC's smoking-related cues do not necessarily determine whether that SRC will distinguish between relevant known-groups.

Adding further to uncertainty in this area, Mogg et al.'s (2005) claim to have demonstrated known-groups validity for the SRC was not only relatively unprecedented and counter-intuitive, but in the opposite direction to what the wider literature would normally predict. Namely, they found that low dependent smokers showed greater approach to smoking cues on an SRC than moderately dependent smokers. Thus, on balance, it is currently unclear why SRCs have sometimes yielded genuine smoking-

[27] This qualitative interpretation of the relevant smoking-related cues is derived from Bradley et al.'s (2008, p. 742) results section wherein they reported that on average (a) the smokers rated one set of smoking-related cues as positive and the other as neutral, and (b) the non-smokers rated the same sets of cues as neutral and negative, respectively. Note, however, that when justifying their findings Bradley et al. did not refer to the relevant cues accordingly. Instead, rather inexplicably, they maintained that one of the sets of cues was universally interpreted as 'pleasant' and the other set as 'unpleasant' for both smokers and never-smokers alike.

related known-groups effects (i.e. thrice), and at other times have failed in this regard (i.e. twice). Likewise, in relation to zAATs, Wiers et al. (2013) may have obtained two findings in favour of their sensitivity to smoking-related known-groups, but both Watson et al. (2013) and Larsen et al. (2014) failed to explain why they found that smokers versus never-smokers did not differ on another version of the zAAT, $p > .14$ and $p > .90$, respectively (i.e. other relevant statistics not reported).[28]

Overall, therefore, some aspect(s) of smoking-related cues might be important in determining whether AATs can consistently distinguish among smoking-related known-groups, but it is unclear whether this is necessarily so, much less whether other extraneous variables are involved. One thing that is clear however is that AATs have succeeded in demonstrating smoking-related known-groups validity only about as many times as they have failed to do so. And as such, AATs certainly are prone to confounding variable(s) even if we do not yet know what those variables are. Furthermore, although AATs sometimes exhibit very good internal reliability with respect to threat-related cues (e.g. for a non-zooming joystick AAT, Experiment 1, r_{sb} = .76, Experiment 2, r_{sb} = .80, Experiment 3, r_{sb} = .71, Rinck & Becker, 2007, p. 114; but see Experiment 2, .21 < r_{sb} < .53, Krieglmeyer & Deutsch, 2010, p. 819; for SRC, Experiment 1, r_{sb} = .84-.95, Experiment 3, r_{sb} = .72-.81, Krieglmeyer & Deutsch, 2010; but again see Experiment 2, -.65 < r_{sb} < - .03, for SRC used by Krieglmeyer & Deutsch and also α = .26-.45 for the zAAT used by Klein, Becker & Rinck, 2011; for a broad review of the area see Watson et al., 2012, p. 5), this does not appear to be the case when addiction-related cues are involved (e.g. using smoking-related cues with a zAAT, α = .41; Watson et al., 2013; using alcohol-related cues with an SRC, r_{sb} = .46, Field, Caren, Fernie, & De Houwer, 2011, p. 700). Such low internal reliability further suggests that addiction-related AATs are being confounded by extraneous variables (i.e. to the extent that we assume action tendencies are reliably involved in criterion behaviour in any given context; e.g. Krieglmeyer & Deutsch, 2010; Watson et al., 2012). Indeed, even putting aside the fact that a measure's internal reliability limits the extent to which it can correlate with other variables (see Field, 2013; LeBel & Paunonen, 2011; Osborne, 2013), as we will now illustrate AATs tend to correlate rather haphazardly with tobacco addiction criteria from study to study.

In line with Mogg et al.'s (2005) anomalous SRC finding from earlier that low

[28] Note that the current literature commonly omits the statistical details and/or even mention of null correlations, and so wherever we discuss such null findings without providing statistical details it is for this reason.

dependent smokers are more inclined to automatically approach smoking-related cues than moderately dependent smokers, they also found that the relevant SRC correlated negatively with CPD, r = - .33, p < .05, and with nicotine dependence as measured by the mFTQ, r = - .39, p < .05. And yet, rather inconsistently that same SRC did not correlate with years smoking. Likewise, Mogg et al. (2005) found that the SRC correlated positively with time since last cigarette, r = .31, p = .06, but in contrast not with urge to smoke as measured by the QSU. Most problematic of all is the fact that smoking-related SRCs have consistently failed to correlate with tobacco addiction criteria on every other occasion where such comparisons were possible (null correlations with CPD, FTND, QSU, self-reported urge to smoke, years smoking, and time since last cigarette, Bradley et al., 2004; null correlations with CPD, FTND, QSU-brief, years smoking, and time since last cigarette, Bradley et al., 2008; null baseline correlations with CPD, the mFTQ, the QSU-brief, and time since last cigarette Field et al., 2005; null correlations with CPD, QSU, FTND, years smoking, time since last cigarette, and number of previous quit attempts, Mogg et al., 2003; null baseline correlations with CPD, FTND, and self-reported urge to smoke, Thewissen et al., 2007).

Other smoking-related AATs are also hit and miss when it comes to correlating with tobacco addiction criteria. Watson et al. (2013) found that self-reported craving and the FTND both regressed significantly onto smokers' zAAT scores, R^2-*change* = .14, *F-change*(2, 45) = 4.5, p = .02. However, when analyzed separately the relationship between smokers' zAAT scores and self-reported craving was modestly *positive*, whereas rather inconsistently, the relationship between smokers' zAAT scores and their FTND scores was modestly *negative*. Granted, the latter finding does lend some support to Mogg et al.'s (2005) anomalous known-groups SRC finding described above. However, at the same time, Watson et al. also failed to find correlations between smokers' zAAT scores and CPD, years smoking, or time since last cigarette. Then, in contrast, Wiers et al. (2013) found that ex-smokers' smoking-related zAAT scores correlated strongly with years smoking, r = .55, p = .01, with pack years (i.e. number of packets of cigarettes smoked per day by years smoked), r = .55, p = .01, with CPD while smoking, r = .44, p = .05, and indeed, that the zAAT scores of 2-hour nicotine deprived smokers correlated strongly with QSU, r = .56, p = .001. However, for no apparent reason, Wiers et al. also found that smokers' zAAT scores did not correlate with CPD, FTND (p > .16), years smoking, or pack years. Moreover, Larsen et al. (2014) found no correlation between adolescent smokers' zAAT scores and CPD, r = .04, or mFTQ, r = .05; and Earp, Dill, Harris, Ackerman, and Bargh (2013) found no correlation between smokers' non-zooming joystick AAT scores and their FTND scores. Thus, overall, AAT's appear to correlate haphazardly with tobacco addiction criteria in cross-section. Moreover, the literature has yet to demonstrate a prospective correlation between such variables in either direction, and what little data exists on the test-retest reliability of AATs is discouraging (i.e. with respect to threat-related cues, the only domain for which such data exists; $r \approx .35$, Reinecke, Becker, & Rinck, 2010).

Another relevant issue is the fact that there is very limited evidence for the sensitivity of AATs to experimental manipulations of contexts related to tobacco addiction. Earp et al. (2013) claimed that smokers' smoking-related AAT scores were subliminally primed to exhibit a moderately greater approach bias for smoking-related cues when 'No-smoking' signs were placed within the background of a picture rating task that those smokers had to complete before the AAT, $F(1, 28) = 4.02$, $p = .06$, $\eta_p^2 = .13$, $r \approx .36$ (i.e. as compared to when the same pictures did not contain the relevant 'No-smoking' signs). From this point of view, it would appear that the AAT is relatively sensitive to smoking-related context. However, the relevant experimental effect was ironic insofar as the 'No-smoking' prime appeared to promote a greater, not lesser, approach bias toward smoking on the AAT. Thus, despite being an interesting finding it is subject to many different competing explanations.

For example, Earp et al. claimed that the relevant priming effect was automatic and thus, with the additional assumption that the negation component of the 'No-smoking' sign cannot be processed automatically, they concluded that it functioned as a "Smoking" rather than "No-smoking" sign. However, given that half of the smokers in the experimental condition reported noticing the inclusion of the 'No-smoking' sign, it is also possible that the relevant priming effect emerged from (non-automatic) deliberative reactance to that sign. Indeed, given that Earp et al. did not measure smokers' AAT scores before their priming manipulation it is even possible that the relevant experimental effect emerged as an inadvertent consequence of sampling. As such, Earp et al.'s finding is more akin to an anomaly than a criterion effect.

Similarly, Watson et al. (2013) reported the unusual finding that when smokers smoked after an average of 13 hours of abstinence it strongly increased, rather than alleviated, their bias toward approaching smoking-related cues on the relevant zAAT, $F(1, 44) = 8.90$, $p = .01$ (i.e. $r \approx .41$); and this occurred regardless of the relevant smokers' FTND scores ($p > .14$). Although incentive sensitization theory could potentially accommodate this finding in terms of behavioural sensitisation processes (see Robinson & Berridge, 1993, 2003, 2008), the fact remains that it is an entirely unprecedented finding within the AAT literature that directly contradicts most tobacco addiction theory (Watson et al., 2013). Indeed, there is even a finding in the evaluative priming literature (Sherman et al. (2003, p. 29) that appears to contradict what Watson et al found. Specifically, an evaluative priming measure indicated that satiation significantly reduced, rather than increased, light smokers' implicit bias in favour of smoking (i.e. as compared to smokers abstaining from smoking).[29] Thus, much like Earp et al.'s equivocal finding, Watson et al.'s zAAT-based experimental effect does not qualify as a validity criterion.

In the only other finding in this area Field et al. (2005) found no effect of drinking alcohol (as compared to drinking a soft drink) on smokers' smoking-related SRC scores. Granted, their null finding based on a sample of just 19 light smokers may have been overly conservative given that the relevant interaction effect was substantial even if not significant, $F(1, 16) = 2.98$ (i.e. $r \approx .40$; see Figure 3 in Field et al., 2005, p. 69). However, even admitting this finding would not change the fact that there is only preliminary, and rather contradictory evidence that smoking-related AATs are differentially sensitive to contexts related to tobacco addiction (indeed, for similar patterns of inconsistent AAT findings in other behavioural domains see Watson et al., 2012, pp. 6-9).

Lastly, as further evidence that researchers have yet to identify clear controlling variables for smoking-related AAT effects, only one study has attempted to modify such effects within a given context; and the resulting findings are certainly less than conclusive. In the relevant study, Thewissen et al.'s (2007) main assertion was that smokers who had agreed to abstain from smoking for at least 2 hours showed more of an approach bias for smoking-related pictures on an SRC in the presence of a background colour that had been evaluatively conditioned to signal an opportunity to smoke in a tempting situation (i.e. as compared to a background colour that had been evaluatively conditioned to signal an inability to smoke in that same tempting situation). Crucially, however, Thewissen et al. did not emphasize that the statistical effect they quoted in support of this finding, $F(1, 19) = 8.15$, $p < .01$ (i.e. $r \approx .54$), was based on two sets of SRC scores: one for SRC trials involving smoking-related pictures and another

[29] Watson et al.'s smokers smoked an average of 10.8 CPD and thus were comparable to Sherman et al.'s 'light' smokers who were defined as smoking 15 CPD or less. This distinction is important because, unlike Sherman et al.'s (2003, p. 29) 'light' smokers, their 'heavy' smokers (i.e. those who smoked more than 15 CPD) did display an increased bias in favour of smoking in response to satiation (as compared to abstinence) much like Watson et al.'s (light) smokers did. Thus, with such highly contradictory and preliminary findings it is questionable whether smoking-related AATs are sensitive to tobacco satiation versus deprivation in any consistent manner.

for SRC trials involving pictures designed to be neutral with respect to smoking. Thus, the relevant statistical effect might have arisen not from SRC scores based upon smoking-related pictures but from SRC scores based upon pictures that were designed to be irrelevant to smoking. And indeed, according to a side-bar restricted to Thewissen et al.'s results section (p. 37) this is just what happened, $F(1, 37) = 4.17$, $p < .05$ (i.e. $r \approx .32$).[30] Overall, therefore, smoking-related AATs appear to be even more equivocal than smoking-related IATs in relation to tobacco addiction criteria (indeed for collateral discussion about the lack of evidence that AAT-based training modifies criterion behaviours by modifying aspects of implicit evaluating see Note 7, Appendix 1).

What is wrong with the Design of AATs in Relation to Tobacco Addiction Research?

The primary reason why AATs are not amenable to systematic development with respect to tobacco addiction is because, much like the other measures of implicit evaluating reviewed so far, they are not capable of experimentally distguishing implicit evaluating of one particular topic as distinct from implicit evaluating of other (perhaps related) topics. We begin our analysis of the matter by explaining how this problem manifests in terms of the SRC because it is arguably the most established measure of automatic approach/avoidance tendencies (i.e. particularly in relation to addiction; Krieglmeyer & Deutsch, 2010; Watson et al., 2012, pp. 5-6). Then, we will describe how subsequent attempts to improve remaining AATs, namely joystick AATs, have also failed to distinguish precisely among different types of implicit evaluating.

In broad terms, the SRC requires participants to alternate between moving a computerized manikin towards target stimuli (and away from co-occurring control stimuli) in one block of trials, versus away from those same target stimuli (and toward the co-occurring control stimuli) in another block trials. The core idea here is that the response time difference between the former versus the latter SRC blocks of trials will provide a measure of automatic approach versus avoidance in relation to the relevant

[30] Granted, this latter finding does not account for the fact that Thewissen et al.'s evaluative conditioning manipulation impacted smokers' SRC scores differently depending upon whether those smokers were explicitly instructed as to what contingency they were supposed to learn during conditioning, $F(1, 37) = 11.42$, $p < .01$ (i.e. $r = .49$). Specifically, there was a larger conditioning effect on smokers' SRC scores when they obtained explicit conditioning instructions, $F(1, 19) = 8.15$, $p < .01$ (i.e. the original finding upon which Thewissen et al. mistakenly based their claim; $r \approx .54$), than when they did not, $F(1, 18) = 3.50$, $p > .05$ (i.e. $r \approx .40$). Crucially, however, this finding did not significantly interact with whether smokers' SRC scores were based on smoking-related versus smoking-neutral pictures, and so the matter is not relevant to Thewissen et al.'s core claim (i.e. despite the fact that they reported it this way). And indeed, even if it were relevant, a similar study found that such evaluative conditioning instructions had no impact whatsoever on an alcohol-related AAT (see Wiers, Eberl, Rinck, Becker, & Lindenmeyer, 2011; moreover, for further evidence that such instructions are ineffective with pre-experimentally established implicit biases see Van Dessel, De Houwer, Gast et al., 2015; Van Dessel, De Houwer, Roets, et al., 2015).

target stimuli (e.g. cues for smoking). A central, though unacknowledged, problem with this measurement approach is that much like the IAT it is doubly relative: it measures the fluency of approach towards target stimuli such as smoking cues *relative to* (a) avoidance of those same target stimuli, and (b) at the same time *relative to* approach versus avoidance of control stimuli. Thus, much like the IAT, the SRC is limited by design to investigating "polarized" evaluative comparisons. And given that tobacco addiction is likely to involve many non-polarized forms of implicit evaluating, this means that the SRC is fundamentally limited in its ability to achieve criterion validity in relation to tobacco addiction. Indeed, if anything, the SRC is arguably even more indeterminate than the IAT, insofar as responding in terms of an ill-defined manikin actively encourages participants to shift their existing perspective in unpredictable ways (e.g. rather than interpreting the manikin in terms of the self as intended, participants might interpret it as any type of person or even non-person with little or no relevance to their own self). As such, smoking-related SRCs are necessitated by design to bear only haphazard relationships with whatever implicit evaluating might be involved in tobacco addiction.

Unlike the SRC, there are a rather large variety of different joystick-AATs. The primary concern driving the emergence of different types of joystick-AATs was that researchers could not determine what specific types of approach/avoidance were involved from trial to trial. To illustrate, the earliest joystick-AATs involved measuring how quickly participants could respond to target stimuli presentations by moving a joystick (or lever) towards their body during one block of trials, as compared to how quickly they could move the joystick away from their bodies during another block of trials involving those same target stimuli (Chen & Bargh, 1999). The core assumption here was that particular motor actions are invariably synonymous with approach versus avoidance, respectively. However, researchers have since demonstrated that participants can re-interpret any given motor movement as either approaching versus avoiding a given target stimulus. Specifically, these researchers were able to repeatedly reverse joystick-AAT effects according to whether participants were instructed to interpret movements towards their body as approaching versus avoiding a given target stimulus (see Watson et al., 2012, p. 5). Therefore, participants are prone to interpreting movements towards their body on a joystick-AAT not just as an approach towards the relevant target object as originally intended, but also as an approach towards their body *and* away from the relevant target object onscreen. And crucially, the original joystick-AAT does not allow researchers to determine whether it involved movement interpretations of one kind and/or the other.

In an effort to control how participants interpreted their movements on joystick-AATs, some researchers added zooming functionality such that joystick movements towards the body consistently increased the size of target stimuli and movements away consistently decreased the size of target stimuli (or vice versa). However, even if this succeeded in disambiguating whether participants made particular zAAT movements in terms of approach versus avoidance, it would not clarify what particular type(s) of approach versus avoidance were involved. For example, a smoker might evaluate their approach to smoking cues on a zAAT in terms of obtaining desirable experiences like stimulation, or contrastingly, in terms of reducing aversive experiences like cravings.

Indeed, making matters worse, some researchers have adapted zAATs so that participants are instructed to respond to irrelevant features of target stimuli (and control stimuli) such as orientation or position on the screen rather than content. The main reason for the introduction of these so-called *irrelevant feature zAATs* is to provide a more implicit measure of approach/avoidance processes. The core assumption here is that participants will tend to process target stimuli implicitly (rather than explicitly) during irrelevant feature zAATs because zAATs do not task participants with deliberately processing the relevant content of target stimuli. However, just because a participant is not required to deliberately respond to the relevant content of target stimuli on an irrelevant feature zAAT it does not in any way prevent participants from doing so. In fact, irrelevant feature zAATs not only allow participants to deliberately respond in many different but unrecorded ways to the content of target stimuli, but ironically, by virtue of their irrelevant feature instructions, they specifically encourage participants to respond to target stimuli in ways that are irrelevant to the topic being investigated (see Klein et al., 2011, p. 230; Krieglmeyer & Deutsch, 2010; Vandenbosch & De Houwer, 2011; Van Dessel, De Houwer, Gast et al., 2015; Van Dessel, De Houwer, Roets, et al., 2015). Indeed, somewhat concordantly, Krieglmeyer and Deutsch (2010, Experiment 2) have provided initial evidence that irrelevant feature zAATs are rather less reliable than other zAATs (see also Watson et al., 2012, pp. 5-6; but for indirect evidence to the contrary see Cousijn, Goudriaan, & Wiers, 2011, p. 1670; Watson et al., 2013, p. 255).

Ultimately, even if we put aside the fact that joystick-AATs are indeterminate with respect to implicit evaluating on any given trial, their measures would still remain highly problematic in that they are relativistic much like the SRC or the IAT. Take for example, Chen and Bargh's (1999) original joystick-AAT. It compared how quickly participants 'approached' target stimuli on some trials and 'avoided' control stimuli on other trials, versus how quickly they respectively 'avoided' target stimuli and 'approached' control stimuli across another block of trials. Thus, Chen and Bargh's joystick-AAT is doubly relative in that it notionally measures how much participants are inclined to approach target stimuli *relative to* avoiding control stimuli, and also *relative to* avoiding target stimuli and approaching control stimuli (i.e. putting aside the issue of whether 'approach' versus 'avoidance' was operationalised as such within each trial; see also Krieglmeyer & Deutsch, 2010; Watson et al. 2013).

Granted, some researchers have recently started to score participants' AAT responses to smoking versus control stimuli separately in an effort to measure smokers' automatic approach tendencies toward smoking cues as distinct from control stimuli (e.g. for an SRC example see Thewissen et al., 2007; for zAAT examples see Earp et al., 2013; Larsen et al., 2014; Wiers et al., 2013).[31] However, even putting aside the fact that participants must respond to smoking-related AATs smoking cues in the context of opposing responses to control cues, the resulting zAAT scores would still remain relative in the sense that they compare approach versus avoidance of smoking cues in a polarized fashion that does not allow for situations in which people are indifferent to the relevant smoking cues. As a result, for example, smoking-related zAATs are not likely to be consistently related to varying levels of smoking-related satiation/deprivation given that smokers should, by definition, be indifferent to smoking cues whenever they are satiated with respect to smoking.

Indeed, in a broader sense, zAATs that are scored only in terms of responses to smoking cues (i.e. ignoring responses to control stimuli) arguably suffer much the same wide range of limitations as we described earlier for smoking-related ST-IATs (i.e. being unable to distinguish between smoking-related implicit evaluating that is framed in positive/approach versus negative/avoidance terms). Overall, therefore, it is not feasible to improve upon the relatively hit and miss criterion validity of existing smoking-related AATs because even if these methods could be refined to consistently measure particular topics of approach/avoidance from trial to trial, they would still be measuring it relativistically against approach/avoidance of other topics – thus ignoring important motivational distinctions within tobacco addiction. In fact, from this point of

[31] The relevant authors did not justify this novel scoring procedure. Instead, they wrongly interpreted the resulting scores as if they reflected participants' responses to smoking cues as distinct from control cues. In fact, along similar lines, these researchers also (wrongly) interpreted the polarity of AAT scores as if they indicated something absolute about whether participants were more inclined to approach versus avoid smoking cues in general (e.g. Earp et al., 2013; Larsen et al., 2014; Thewissen et al., 2007; Wiers et al., 2013).

view, it seems possible that the haphazard criterion validity exhibited by smoking-related AATs to date merely reflects behavioural responses that are haphazardly secondary to tobacco addiction (i.e. rather than responses that are causally involved therein).

3.2. Measuring Complex Implicit Evaluating using Expectancy Accessibility Tasks (EATs)

A Conceptual Overview of EAT research

The most characteristic feature of EAT research is its focus upon measuring complex forms of implicit evaluating in terms of so called *networks* of hypothetical cognitive associations. In broad terms, all EATs involve asking participants to report their introspections under distraction and/or high time pressure using questionnaire-based measures. The core conceptual basis of the EAT methodology is the idea that networks of cognitive associations may provide a mechanism by which humans can automatically *anticipate* which combinations of cues and behaviours will lead to specific consequences (see Goldman et al., 2006; Kahler et al., 2007; Grenard, Ames, Wiers, Thush, Sussman, & Stacy, 2008; McKee et al., 2003; Palfai, 2002; Palfai & Wood, 2001; Stacy, 1997; Stacy & Wiers, 2006, pp. 502-504; Stacy & Wiers, 2010, pp. 557-566; Waters & Sayette, 2006; Watson et al., 2012). Indeed, reflecting this sentiment, many EAT proponents refer to networks of cognitive associations as *implicit expectancies* (e.g. see Goldman et al., 2006; Leigh & Stacy, 1991, 1998; Leung & McCusker, 1999; McKee et al., 2003; Palfai & Wood, 2001; Stacy, 1997, pp. 62, 70; Stacy & Wiers, 2010, p. 559; Watson et al., 2012).

Crucially, in targetting implicit expectancies, all EATs are specifically designed to capture a range of rule-based cognitive processes (i.e. incorporating conditionality) such as memory templates, schemas, implementation intentions, or even placebo/nocebo effects (see Cesario, 2014; Dijksterhuis & Aarts 2010; Goldman et al., 2006; Gollwitzer & Sheeran, 2006; Harell & Juliano, 2012; Hendricks & Brandon, 2008; Hughes et al., 2011, pp. 478-483; Kelly et al., 2006; Koole, Webb, & Sheeran, 2015; Krank, Wall, Stewart, Wiers, & Goldman, 2005; Lovibond, 2006; McKee et al., 2003; Stacy, 1994, 1995; Stacy, Leigh, & Weingardt, 1994; Stacy & Wiers, 2010, p. 565; Vogt et al., 2013; Webb, Sheeran, & Pepper, 2012; Wiers et al., 2010, pp. 465-468, 479-480). This is important because in principle it grants EAT users the ability to explain a much greater range of routinized criterion behaviours than they would be able to with any of the other implicit measures we have considered so far. And indeed, Rooke et al.'s (2008) recent meta-analysis comparing various implicit measures of addiction does provide some support for this assertion, insofar as it found that EATs measuring addiction-related implicit expectancies were among the best at achieving criterion effects ($\bar{r}$s = .29-.40; Rooke et al., 2008).[32]

With regard to the specifics of implementation, there is quite a large variety of EAT variants throughout the literature. As noted above, all EATs are based upon the same core strategy of asking participants to self-report in ways that are designed to limit introspection and/or deliberation about each cognitive process of interest. The core idea behind attempting to limit deliberation is to minimise any basis for participants to confound an EAT's intended measurement process by responding strategically to it (e.g. whether in terms of attempting to conform to the researcher's expectations, or in terms of satisfying personal concerns for social desirabilty; see Gawronski & De Houwer, 2014; Payne & Gawronski, 2010). And as such, many view (smoking-related) EATs as measuring implicit cognition insofar as these methods were specifically designed to indirectly measure inadvertent aspects of participants' explicit self-reporting (see Palfai, 2002; Stacy & Wiers, 2010, pp. 555-561; Rooke et al., 2008, pp. 1316-1317; but see De Houwer, 2006, p. 20).

Traditionally, the main way in which EATs were designed to curtail deliberation about a topic of interest was by (a) employing open-ended questions that had only indirect and equivocal connections to the relevant topic, by (b) encouraging participants to respond quickly, and/or by (c) analysing only the earliest self-reports to each question (see Krank et al., 2005; Rooke et al., 2008, pp. 1316-1323; Stacy, 1997; Stacy et al., 2006; Stacy & Wiers, 2010, pp. 558-561; Watson et al., 2012; Wiers & Stacy, 2006). However, the approach of asking indirect, ambiguous and open-ended questions meant that researchers were prevented, by definition, from targeting particular implicit expectancies for measurement on an a priori basis. In an effort to remedy this limitation, some researchers relented and designed variant EATs that do involve asking participants directly about the relevant topic in each case. These variant EATs primarily approach the problem of minimising extraneous deliberation by imposing time pressure

[32] Note that Rooke et al. (2008), like some other authors (e.g. Stacy, 1997), have choosen to distinguish among various different expectancy accessibility measures based upon their own particular theoretical ideas about what mental processes are being measured by each particular method. However, rather than subscribe prematurely to any particular hypothetical position within this area we opt like others in the literature to use the term EAT inclusively to refer to any method that targets implicit expectancies (i.e. whether conceptualised as networks of cognitive associations or as an emergent implicit effect of such networks on higher level expectancy-related cognitive systems; see Goldman et al., 2006; Kahler et al., 2007, pp. 2067-2068; Leung & McCusker, 1999; Sayette, & Hufford, 1997; Stacy, 1997, pp. 62, 70; Stacy et al., 2006; Stacy & Wiers, 2010, p. 559).

on participants' responses and/or by limiting the range of self-report options available to participants (e.g. to 'Yes' versus 'No' as opposed to open-ended response options; Goldman et al., 2006; McKee et al., 2003; Read et al., 2004; Rooke et al., 2008, pp. 1316-1323). Therefore, we adopt the foregoing broad distinction between *direct* versus *indirect* EATs in an attempt to make the methodologically diverse research literature on smoking-related EATs more tractable for critical review.

A Critical Review of Smoking-related EAT Research

There are many reasons to be sceptical about the criterion validity of smoking-related EATs. For example, upon close scrutiny Rooke et al.'s ostensibly promising findings regarding the criterion validity of addiction-related EATs are revealed as being overly optimistic. Rather than including all relevant EAT criterion effects within their meta-analysis, Rooke et al. (2008, p. 1318) specifically excluded any that resulted from experimental manipulations of criterion variables (e.g. manipulations of nicotine deprivatrion/satiation were excluded). And as a result, their EAT findings were based solely upon questionnaire-based criterion variables rather than experimental manipulations (i.e. including known-groups contrasts). This is particularly concerning given that EATs are essentially speeded questionnaire-based self-reports about word stimuli that are quite literally contrived to be (directly or indirectly) reminiscent of the very criterion variables in question. For example, words like 'inhale', 'match' or 'relaxes me' are specifically incorporated into most smoking-related EATs on the basis that they are likely to remind smokers of smoking and/or their smoking-status. And as such, observing a relationship between smokers' questionnaire-based tobacco cravings or smoking-status, and the number of speeded pro-smoking self-reports they emit on an EAT in response to smoking-related stimuli like 'relaxes me' or 'inhale' is arguably a trivial one. Just as it is possible to provide relatively accurate self-reports about smoking-status or CPD without revealing anything about the cognitive basis for tobacco addiction, the same applies to EATs even if they do happen to correlate with self-reports of smoking-status or CPD. To illustrate this point in the context of tobacco addiction, we focus now upon how smoking-related EATs have consistently failed to correlate with experimentally controlled changes in smoking-related variables. In recognition of the fact that direct EATs were developed against a backdrop of indirect EATs, we review the research literature on direct smoking-related EATs only after first dealing with that pertaining to their indirect counterparts.

Research on Smoking-related Indirect EATs

Despite the fact that research on addiction-related indirect EATs began more than 25 years ago (see Leigh & Stacy, 1991, 1998; Litz, Payne, & Colletti, 1987; Palfai, 2002, p. 322; Stacy et al., 2006), we could find only three research articles that used one of these methods in relation to smoking. And indeed, none of these articles involved experimentally controlled tobacco addiction criteria. Rather they relied solely upon questionnaire-based tobacco addiction criteria that were reminiscent of the questionnaire-based self-reports being contrived by the relevant EATs being tested. For example, the earliest research we could find involving an indirect smoking-related EAT was Kelly et al. (2006). They adopted a variation of Stacy et al.'s (1994) word association paradigm (see also Stacy, 1994, 1997) that involved asking participants to quickly report the first word they were reminded of when presented with each of 38 different words in turn.

Crucially, Kelly et al. actively concealed from participants that smoking was the topic of research. Indeed, only five of the 38 words presented to participants had any connotations with smoking, and even then those connotations were designed to be ambigious. In particular, all of five smoking-relevant stimulus words were chosen to be homographs having both smoking-related and smoking-irrelevant connotations (e.g. the word 'match' can be interpreted as an object used to light cigarettes or as a synonym for pairing objects). On this basis, Kelly et al. reasoned that the number of smoking-related self-reports participants provided across the five ambigious homographs would be indicative of the extent to which those participants possessed pre-potent networks of smoking-related cognitive associations that were established *and* elaborated (i.e. assuming that participants would not realise the smoking-related purposes of the relevant EAT). Unfortunately, however, the resulting frequency score only correlated modestly with explicit self-reports of smoking consumption ($.15 \leq r\text{s} \leq 22$, $p\text{s} < .05$ without corrections for familywise statistical error; see Kelly et al., 2006, p. 53).

Later, Grenard et al. (2008) adopted a more complex variation of Stacy et al.'s word association paradigm by combining three different word association tasks. The first task took the same approach as Kelly et al.'s EAT by asking participants to respectively self-report the word they were most immediately reminded of when presented with each word stimulus. However, the second and third tasks involved asking participants to self-report the first *behaviour* they were reminded of when presented with each word-based cue. Moreover, the second and third tasks used different word stimuli from those used in the first task, and also different word stimuli from each other. Specifically, Grenard et al.'s second word association task involved asking participants to respond to positive outcome phrases some of which were homographic for smoking (e.g. *feeling relaxed*). And, the third task involved asking participants to respond to compound phrases specifying both an outcome and its setting with some being homographic for smoking (e.g. *friend's house – feeling good*) and the remainder being smoking-irrelevant. Ultimately, Grenard et al. computed composite EAT scores by summing the number of smoking-related responses obtained across the smoking-homographes in all three tasks.

The resulting EAT was relatively impressive in its ability to correlate cross-sectionally with explicit self-reports of CPD (i.e. cigarettes smoked per day), producing an R^2-*change* of .22 (i.e. $r \approx .46$) even after controlling for background variables like working memory, education, gender and age. Interestingly, Grenard et al. also found that smoking-related EAT scores interacted significantly with working memory when regressed onto CPD. The relevant interaction indicated that smoking-related EAT scores were more closely correlated with CPD the lower a participant's working memory – a finding in keeping with the popular idea that people will tend to rely more upon implicit processing to the extent that deliberative processes are limited. However, it is important to qualify the statistical significance of this interaction with its practical significance: it only accounted for an R^2-*change* of .03 (i.e. $r \approx .17$) on a main effect of $r \approx .46$ (i.e. between CPD versus the relevant EAT scores); and both effects were cross-sectional. Most importantly, Grenard et al.'s cross-sectional findings, based as they were upon participants' narrative descriptions of their introspections, provided only anecdotal evidence that implicit expectancies are causally involved in CPD (i.e. much less whether the availability of deliberative processing moderated that relationship to a practically significant degree). Likewise, when Zwann and Truitt (2000) demonstrated the moderate ability of another type of indirect smoking-related EAT to distinguish smokers from never-smokers, $F(1, 44) = 5.38$, $p < .03$ (i.e. $r \approx .33$), this too was merely anecdotal about tobacco addiction per se.

In response to the lack of clarity afforded by these narrative-based cross-sectional methods, the only other existing study of indirect smoking-related EATs examined whether they correlated prospectively with (self-reported) tobacco addiction criteria. Specifically, Kelly et al. (2008) reused the same indirect EAT as described above for Kelly et al. (2006), and encouragingly, they found that it prospectively correlated with the number of days on which adolescents smoked in the subsequent month. However, the relevant prospective correlation was only marginally significant at $p = .05$ with a corresponding statistical effect that was relatively modest at $r \approx .25$ (i.e. translated from a Z score of 1.94). Worse still, Kelly et al. only obtained this correlation when they arbitrarily omitted all median EAT scores from their analysis (see Kelly et al., 2008. P. 646). Indeed, even if Kelly et al. had shown that indirect EAT scores can prospectively correlate with smoking without resorting to such cherry-picking, this would not be the same thing as demonstrating that such scores can predict *changes* in smoking (i.e. the latter being a causal matter). In other words, there is a complete lack of research, much less findings, about whether indirect EATs measure cognitive processes that are involved in experimentally controlled tobacco addiction criteria.

Research on Smoking-related Direct EATs

Unfortunately, the situation is only marginally better when it comes to research on direct smoking-related EATs. For example, Sayette, Martin, Wertz, Shiffman, and Perrott (2001) experimentally controlled abstinence from smoking and nicotine in order to examine how it changed the activation of smokers' networks of cognitive associations about smoking. However, only one of the resulting experimental effects even approached statistical significance and it was a small interaction effect at that, $F(1, 123) = 3.3$, $p = .07$ (i.e. $r \approx .16$). The relevant interaction effect indicated a trend for heavy smokers to (implicitly) self-report more things they liked about smoking when nicotine-deprived as compared to when "minimally" deprived, but for light smokers to do the opposite. Notably, Sayette et al. did not report any tests of the simple effects comprising the relevant 2x2 interaction effect – and so at the very least the foregoing interpretation is questionable.

Worse still, Sayette et al. only obtained their significant interaction effect by cherry-picking pro-smoking EAT data from among anti-smoking EAT data. The direct EAT that they employed asked participants to take three minutes to write as many different things that they liked or disliked about smoking. Crucially, Sayette et al. analysed the numbers of pro- versus anti-smoking EAT self-reports as separate dependent variables, even though these scores were derived from a method that put each smoker's pro- versus anti-smoking self-reports in direct competition with each other. Sayette et al. do not explain why they took this course of analysis, but it appears as though it was necessary to justify testing the only (marginally) significant effect that they did find. If they had compared pro- versus anti-smoking EATs within their analysis Sayette et al.'s only marginally significant effect would have been a simple effect within an overarching omnibus effect, and that omnibus effect seems unlikely to have been significant (i.e. given the lack of any other significant effects reported by Sayette et al.).

As such, we must view Sayette et al.'s solitary significant EAT effect as potentially arising from familywise statistical error. In fact, it seems that the main reason why Sayette et al. (2001) emphasized the particular non-significant trends they did, was because they appeared to corroborate Sayette and Hufford's (1997) findings using a similar direct EAT. Namely, Sayette and Hufford found that heavy[33] smokers listed more positive relative to negative characteristics of smoking under a 'high urge' condition (i.e. holding a cigarette without smoking it having abstained from nicotine for 12 hours), than under a 'low urge' condition (i.e. holding a smoking-irrelevant control object having smoked normally beforehand), $F(1, 64) = 5.1, p < .03$ (i.e. $r \approx .27$).[34]

Granted, there is other cross-sectional evidence for the criterion validity of other direct EATs that frame smoking in pro- versus anti-smoking terms. For example, Marks, O'Neill and Hine (2008) used one such direct EAT to show that implicit smoking-related expectancies regressed significantly on CPD, $R^2 = .19$, $r \approx .44$, $p < .01$, and were perhaps a little more predictive for experiential versus rational modes of cognition, $R^2 = .02$, $r \approx .14$, $p < .05$ (i.e. the latter corroborates their implicitness). In addition, Benthin et al. (1995) found that their direct EAT incorporating similarly relativistic tasks distinguished frequent- versus occasional- versus never-smokers, $R^2 = .14$, $r \approx .37$, $p < .001$. Indeed, Leung and McCusker (1999) found an even stronger known-groups effect showing that smokers listed a higher proportion of pro-smoking adjectives (i.e. relative to anti-smoking adjectives) within 120 seconds than non-smokers, $F(1, 76) = 37.23$, $p < .001$, i.e. $r \approx .57$. However, even taking the foregoing collection of findings at face value they still only amount to interesting anomalies about how smoking-status and/or nicotine abstinence differentially affect the *relative* accessibility of positive versus negative smoking expectancies en masse.

Crucially, as explained earlier, it is unlikely that tobacco addiction is generally motivated by the simultaneous activation of one's pro- and anti-smoking expectancies en masse in polarized competition *relative* to each other (i.e. apart from in limited situations involving ambivalence). Therefore, rather than measuring tobacco addiction processes per se, it seems likely that the foregoing known-groups effects were primarily a collateral artefact of such processes. In other words, it is possible that smokers tended

[33] Sayette et al. defined 'heavy smokers' as smoking 21 or more cigarettes per day on average, and so by this standard Sayette and Hufford's (1997) smokers were relatively heavy smokers given that they smoked an average of 16.4 cigarettes per day

[34] When Sayette and Hufford analysed the relevant positive versus negative characteristics separately it did indicate a somewhat stronger difference between high versus low urge conditions for the positive characteristics listed by heavy smokers, $F(1, 64) = 9.2$, $p < .005$, i.e. $r \approx .35$; and no corresponding difference in terms of the negative characteristics listed by heavy smokers, ps $> .15$. However, as with Sayette et al. such non-relative analyses were inappropriate given that Sayette and Hufford used an EAT that required smokers to list positive and negative characteristics of smoking *relative to each other* (i.e. side by side on the same sheet of paper albeit in separate 90 second time intervals in contradistinction to Sayette et al.).

to list more pro- versus anti-smoking expectancies than non-smokers not because the evaluative processes involved in these listing tasks overlapped with those involved in tobacco addiction; but because these listing tasks specifically entailed rationalising one's own (non)smoking in a post hoc and indeed ad hoc manner. Certainly, none of the relative implicit measures we have described so far have provided consistent findings about implicit pro- versus anti-smoking attitudes as a function of either smoking-status or nicotine deprivation (e.g. evaluative priming findings appear to directly contradict Sayette and Hufford's, and Sayette et al's findings; see Sherman et al., 2003, p. 29).

Nonetheless, there is one other type of direct EAT that we have not yet considered. Namely, whereas the foregoing direct EATs all involved asking participants to choose relativistically between pro- and anti-smoking self-reports on each trial, there are some direct EATs that do not capture relativistic responding. In broad terms, such measures involve asking smokers to successively list, under time pressure, as many consequences they could of smoking specifically without reference to other topics. Crucially, therefore, insofar as these EATs record non-relativistic self-reports they are in principle more precise than any of the foregoing (direct) EATs with respect to distinguishing smoking-related implicit evalauting of particular topics. It is important to remember, however, that while the following direct EAT's may involve non-relativistic tasks, they did nonetheless score smokers' responses to those tasks in a relativistic fashion by scoring their smoking-related responses relative to their corresponding responses to (ambigious) control topics.

For example, McKee et al. (2003) examined smokers' first responses (under time pressure) to the sentence stem "When I smoke cigarettes I expect to..." (without issuing any instructions to list pro- or anti-smoking expectencies), and found that experimental mood induction tended to activate corresponding types of implicit expectancies about smoking among those with some history of smoking, $\chi^2(4, N = 109) = 12.44$, $p = .01$ (i.e. $r \approx .34$). Specifically, the authors reported that 'ever-smokers' were more likely to (first) list smoking as being positively reinforcing in the positive mood condition (relative to neutral/negative mood induction conditions); and when in the neutral or negative mood induction conditions to (first) list smoking in negative terms (i.e. as being negatively reinforcing or as having negative consequences). Crucially, McKee et al. interpreted these findings as experimental evidence that their EAT measured implicit expectancies that varied with context in ways that are characteristic of tobacco addiction. However, even if we accept McKee et al.'s overarching rationale it still remains that they only obtained their findings by arbitrarily ignoring the 22% of their original sample who did not list any smoking-related expectancy.

Furthermore, McKee et al. did not statistically test the two particular simple contrasts upon which their claim for criterion validity rested. Namely, they did not test whether the positive mood condition induced more first-listed expectancies that smoking is positively reinforcing (i.e. as compared to the other two mood conditions); and nor did they specifically test whether the neutral and negative mood conditions induced more first-listed expectancies that smoking is negatively reinforcing or aversive (i.e. as compared to the positive mood condition). Instead, they just provided the omnibus statistical effect reported above. In addition, all of McKee et al.'s foregoing analyses confounded smokers with ex-smokers (i.e. 28% of participants had not smoked for over 12 months), even though in other analyses McKee et al. also reported that such distinctions had a major impact on the relative accessibility of positively- versus negatively-oriented expectancies (p. 222). Thus, at best, McKee et al. provided only modest and ambiguous experimental evidence for the criterion validity of direct smoking-related EATs.

It appears that the only experimental evidence for the criterion validity of *partially-relative* direct EATs like this (i.e. with non-relative tasks but relative scoring) in relation to tobacco addiction is a study by Palfai (2002). This study focused upon measuring daily smokers' pro-smoking implicit expectancies by measuring how quickly participants chose to endorse smoking-related versus control sentence stems (i.e. having the option to also answer false). In particular, Palfai compared how smokers' smoking-related implicit expectancies varied as an experimental function of 6 hours of nicotine deprivation versus normal smoking conditions. This resulted in a significant positive relationship between the accessibility of pro-smoking expectancies and nicotine deprivation, rs ≈ .25-.30, ps < .05. Unfortunately, however, the remaining research literature on non-relativistic direct smoking-related EATs is not only cross-sectional and thus anecdotal as regards the criterion validity of these measures, but also inconsistent.

Some studies indicated that non-relativisitic direct EATs can differentiate groups with different smoking-histories: (i) Fallon (1998) reported that the frequency and the speed of pro-smoking endorsements significantly and coherently related to smoking-status, $F[3, 67] = 8.19, 3.13$, ps < .05, rs ≈ .33, .21, as did the frequency of anti-smoking endorsements, $F[3, 67] = 3.39$, $p < .05$, $r \approx .22$; (ii) Litz et al. (1987, p. 306) reported that smokers endorsed pro-smoking expectancies significantly faster than never-smokers, $p < .01$, i.e. $r > .35$, and also significantly more frequently, $p < .01$, i.e. $r > .35$;

and (iii) McKee et al. (2003, p. 222) reported smoking-abstinence (in months) significantly and coherently moderated the frequency of different types of smoking-expectancies, $\chi^2[4, N = 109] = 35.89$, $p < .001$, i.e. $r \approx .57$.

Other studies have indicated correlations between partially-relative direct EATs and various self-reported criteria for tobacco addiction: (i) Hendricks and Brandon (2005) reported that various sub-scales of the SCQ correlated significantly with corresponding categories of pro- and anti-smoking implicit smoking expectancies, $.21 \leq r(99) \leq .39$, *p*s $< .05$; (ii) Palfai (2001, pp. 324-325) found that the accessibility of smokers' pro-smoking expectancies correlated with CPD, $r = .32$, $p < .05$, even when controlling for the negative reinforcement subscale of the SCQ, $R^2 = .15$, $r \approx .39$, $p < .01$, with which they also correlated, $r = .37$, $p < .05$; (iii) Palfai (2001, pp. 324-325) also found that pro-smoking implicit expectancies correlated with the FTND, $r = .25$, $p < .05$, the psychological symptoms subscale of the Withdrawel Symptoms Questionnaire [WSQ], $r = .25$, $p < .05$, and with cued self-reported urges to smoke, $r = .32$, $p < .05$.

Although the foregoing lists may appear quite encouraging, it is important to note that such cross-sectional findings were obtained by cherry-picking from among a comparable number of corresponding null findings. For example, Palfai's (2001) non-relativstic direct EAT correlated with only one of seven subscales of the SCQ (i.e. the negative reinforcement subscale), even though each of the subscales were designed to measure smokers' beliefs about the positive consequences of smoking. Likewise, the relevant EAT scores also did not correlate with any of the four WSQ subscales designed to measure different aspects of tobacco craving. Furthermore, in a follow-up to Litz et al. (1987), Fallon (1998) found that the frequency of anti-smoking endorsements on a partially-relative direct EAT significantly and coherently related to smoking-status (see above), but no such relationship was originally observed by Litz et al. Indeed, using a slightly different index of anti-smoking implicit expectancies based upon response latency, neither Fallon nor Litz et al. obtained a smoking-status effect (i.e. even though the corresponding *pro*-smoking latency measures did yield an effect in both cases).

When Hendricks and Brandon (2005) used a partially-relative direct EAT to measure various categories of smoking-related implicit expectancies they also found rather puzzling findings. Only one of four types of implicit expectancies about different aspects of positive reinforcement from smoking correlated significantly with the positive reinforcement sub-scale of the SCQ (i.e. implicit expectancies about smoking-induced social enhancement, $r(99) = .21$, $p < .05$). Worse still, Hendricks and Brandon reported that general implicit expectancies of smoking-related positive reinforcement correlated significantly with the negative reinforcement subscale of the SCQ, $r(99) = .20$, $p < .05$; and, that implicit expectancies of negative reinforcement from smoking correlated with the positive reinforcement subscale of the SCQ, $r(99) = .29$, $p < .005$.

In addition to such inconsistent findings, it appears that researchers in the area have been rather selective in reporting only significant statistical relationships with tobacco addiction criteria, while omitting any mention of null statistical relationships with other recorded tobacco addiction criteria that were even more important. For example, Litz et al. (1987) recorded both CPD and years smoking but they did not report statistical tests for the relationships between these tobacco-addiction criteria and the relevant smoking-related EAT scores they measured. Likewise, both studies by Hendricks and Brandon (2005, 2008) recorded smokers' years smoking, CPD and also their nicotine dependence (ala the FTND) but did not report on their respective statistical relationships with corresponding implicit smoking-related expectancies measured using a partially-relative direct EAT (i.e. despite reporting statistically significant relationships with other criteria like the SCQ). As such, under-reporting of null criterion effects appears to be as much of a problem in the literature on non-relativistic direct EATs as it is in the respective literatures on indirect EATs (see above) or on direct EATs that are relativistic (e.g. Sayette et al. did not report anything about the statistical relationships between their direct EAT versus CPD, years smoking or self-reported cravings).

On the Overall Feasibility of using EATs to Systematically Quantify Tobacco Addiction

EATs, and in particular direct EATs, may at first appear to be more precise than the other implicit measures we have so far reviewed because they require participants to respond to very specific topics and/or to self-report those topics from trial to trial (under time pressure). However, a widely acknowledged problem with these methods is that much like regular questionnaire-based self-report measures it is relatively easy for participants to respond to EAT trials strategically – thus making EATs characteristically prone to confounding by demand characteristics, social desirability and most of the other extraneous variables to which questionnaire-based self-reports are prone (e.g. see Stacy & Wiers, 2010, pp. 555-561; Rooke et al., 2008, pp. 1316-1317). Moreover, even if we could assume that the self-report responses captured by smoking-related EATs were not influenced by extraneous variables, all EATs are fundamentally designed to score their smoking-related trials in a relative fashion that confounds whatever various types of implicit evaluating were involved from trial to trial.

For example, partially-relative direct EATs typically involve scoring participants' responses to smoking-related trials against their responses to control trials specifically designed to measure implicit evalauting of topics with nothing obvious to do with smoking (i.e. based on the unfounded assumption that these responses would somehow be neutral, rather than indeterminate with respect to smoking). Crucially, therefore, EATs are fundamentally bound like the IAT and other relative measures to ignore important motivational distinctions between implicit evalauting of one smoking-related topic versus others (see Chapter 2) – and as such it is simply not feasible to systematically improve the haphazard criterion validity of a smoking-related EAT. Indeed, bearing this out, despite over 25 years of research, as we have just reviewed the EAT literature has yielded a similarly lacklustre profile of hit and miss criterion validity in relation to tobacco addiction as the IAT, and other relative measures of smoking-related implicit evaluating.

3.3. The IRAP as the Only Current Alternative to the IAT that is Capable of Measuring Implicit Evaluating of One Topic at a Time

A Conceptual Overview of IRAP Research

In basic terms, the Implicit Relational Assessment Procedure (IRAP; Barnes-Holmes et al., 2006) is a computer-based protocol that alternately requires participants to quickly respond to a given topic from two perfectly opposing evaluative perspectives designed to be respectively consistent versus inconsistent with their learning histories. The central rationale underpinning this approach is that under high time pressure it should take participants less time to correctly produce the prescribed IRAP responses that are most in keeping with their ongoing history of evaluative responding, than to produce those to the contrary. In particular, by comparing the speed with which a participant is able to provide opposing evaluative responses to a given topic on an IRAP (of which there are usually four), one is essentially assuming that the time difference between these two sets of responses will be mainly in proportion to how accustomed that participant is to responding in one way rather than the other, in the relevant measurement context (see Barnes-Holmes et al., 2010). Thus, for example, in most contexts smokers should in principle require less time (under time pressure) to provide a prescribed response "True" to "Smoking makes me feel – Good" as compared to a prescribed response "False" (i.e. insofar as smokers are characteristically more accustomed to deriving reasons for rather than against their smoking).

If, however, the IRAP presented participants with only one topic like "Smoking makes me feel – Good" and simply required them to alternately respond "True" versus "False" to this topic, there would be nothing preventing them from quickly providing those responses as required but regardless of their intended topic. This is why the IRAP requires participants to coordinate their responding among four different types of trials – so as to minimise the possibility of any such extraneous responding. In particular, an IRAP's four trial-types are always designed to be as topographically similar to each other as possible while also still maintaining complementary meanings relative to each other. For example, a smoking-related IRAP might generate four trial-types by pairing each of "Smoking makes me feel" versus "The Smoking-ban makes me Feel" respectively with stimuli describing positive versus negative moods. In other words, the four IRAP trial-types are essentially four different pairings beween two sets of two antonymic evaluative stimuli. And moreover, such that each of the two evaluative stimuli defining each IRAP trial-type is shared with one other trial-type, and also opposite to that in one other trial-type (i.e. four combinations of two stimuli from beween two sets of two antonymic evaluative stimuli).

Crucially, therefore, when participants are randomly presented with four such highly interrelated trial-types in succession during the IRAP, they have no alternative but to process the identifying features of both evaluative stimuli defining each trial if they are to reliably provide the various 'correct' responses required for each respective IRAP trial among many. Specifically, although there is a 50% chance that participants will *randomly* choose the 'correct' response on any given trial, this likelihood diminishes exponentially over multiple IRAP trials, and/or as one requires greater levels of response accuracy from participants. In fact, on the basis of 50% likelihood per trial, there is less than a $1x10^{-35}$ chance that a given participant would randomly attain at least the 80% response accuracy criterion usually required across the test phase of standard IRAP (i.e. assuming 144 test trials as in most IRAPs currently in the literature).

In other words, the main way in which the IRAP enforces its experimental control over what topics its participants are evaluating on each trial is by requiring them to respond as quickly as possible, in highly prescribed ways that are experimentally contingent on all features of the relevant topic presented on that trial. And moreover, when it comes to scoring IRAP responses, fundamentally, only trials that share a given topic are scored together. Therefore, rather than measuring implicit evaluating of one topic *relative* to another, IRAP measurements are specifically designed to experimentally control participants' responses so that they are bound to be in terms of one specific topic as distinct from another. Crucially, no other measure of implicit evaluating even attempts this at present (e.g. see Gawronski & De Houwer, 2014) – and in principle, this makes the IRAP rather uniquely capable of determining what aspects of implicit evaluating it is measuring; and thus also of distinguishing what aspects of implicit evaluating are most motivationally pivotal to (treating) tobacco addiction.

Granted, as in any psychological response task, it is of course possible in principle for a participant to strategically reframe some or all of the four IRAP trial-types in terms of extraneous topics. However, an IRAP always presents each of its four IRAP trial-types in random succession across trials, and as such, before a participant can correctly identify the prescribed response for a given trial they must first process both of its defining stimuli in distinction to three remaining trial-types. Recall, for example, that the literal meanings of the IRAP's four respective trial-types are specifically chosen to be in distinction to each other; and moreover, that by contrast opposing IRAP stimuli are typically chosen to be topographically similar across multiple instances (i.e. participants are usually presented with at least six topographically similar versions of each IRAP trial-type). Thus, it is in principle a much more familiar task for participants to respond 'correctly' to IRAP trials in terms of the literal meaning of their defining stimulus combination, than it is to respond 'correctly' to those trials in terms of some improvised strategy based on ad hoc topographical features.

And therefore, with all else being equal, participants should be much slower at providing 'correct' IRAP responses using the latter approach as compared to the former. Crucially, this makes it relatively difficult to distinguish one trial-type from another on a given trial, or thus the correct response required on that trial, without first processing the intended meaning of both aspects of that trial's defining stimuli. Indeed, bearing in mind the minimal response latency criterion that the IRAP typically imposes based upon pilot testing, it seems even less likely that any given participant could respond with the high levels of response accuracy required by the IRAP (i.e. usually a minimum of 80% accuracy), without responding in terms of the intended (literal) meaning of each IRAP trial-type (e.g. see Barnes-Holmes, Murphy, Barnes-Holmes, & Stewart, 2010). To illustrate, it would be akin to performing an abstract mathematical calculation, involving a network of at least 72 algebraic distinctions,[35] with at 80% accuracy on each

[35] Note that I calculated the relvant number of algebraic distinctions by assuming that there were six versions of each IRAP trial-type, thus requiring participants to repeatedly derive at least 24 highly unfamiliar topographical distinctions on each trial and then identify it as belong to one trial-type or another, currently requiring one response or another (i.e. they were required to not only topographically distinguish each of 24 trials as distinct from each other using mnemonics, but in each case, also as

of 144+ IRAP trials within a response latency window limited to somewhere between 1.5 and 3 seconds on average. In fact, bearing this out, a high degree of participants never even manage to achieve the IRAP's response latency and accuracy criteria in the first place, unless they have had the opportunity to practise those trials with some response rule that summarizes and thus guides what responses they are required to produce during each stage of the relevant IRAP (see Vahey et al., 2010). Moreover, preliminary research indicates that the IRAP is relatively immune to faking by employing strategic responding on a trial by trial basis (McKenna, Barnes-Holmes, Barnes-Holmes & Steward, 2007), and likewise, is also able to detect highly socially sensitive patterns of evaluating that are typically concealed in traditional questionnaire-based self-reports (e.g. Dawson, Barnes-Holmes, Gresswell, Hart, & Gore, 2010; Power, Barnes-Holmes, Barnes-Holmes, & Stewart, 2010; Roddy, Stewart, & Barnes-Holmes, 2010). Furthermore, as perhaps the most important empirical testament to the IRAP's experimental precision, a recent meta-analysis of 15 clinically-oriented IRAP studies, including two studies related to addiction, indicated that it achieved a relatively high degree of criterion validity as compared to all other implicit measures (i.e. $r = .45$; Vahey, Nicholson, & Barnes-Holmes, 2015). Indeed, in the only IRAP study to prospectively examine addiction processes to date, Carpenter, Martinez, Vadhan, Barnes-Holmes, and Nunes (2012) found multiple strong relationships between cocaine addicts' implicit evaluating during early abstinence and their subsequent relapse (or not) from a contingency management treatment designed to treat cocaine addiction – and moreover, these were relationships that were not detected by corresponding questionnaire-based measures.

In order to fully understand how the IRAP was developed to achieve such results, it is critically important to bear in mind that this technique was not designed to measure hypothetical mental constructs, such as networks of cognitive associations. Instead, rather uniquely, the IRAP was specifically developed in order to measure relational response biases (i.e. response probabilities) from the behaviour-analytic perspective of Relational Frame Theory (RFT; Hayes, Barnes-Holmes & Roche, 2001; see also Barnes-Holmes, Barnes-Holmes, Stewart, & Boles, 2010; Stewart, 2015; Törneke, 2010). Crucially, from this point of view, it is not necessary to adopt mentalistic assumptions in order to systematically develop empirical theories of implicit evaluating (in any given domain; e.g. see Hughes, Barnes-Holmes, & De Houwer, 2011;

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belonging to one of four respective trial-types, and to then chose which response was currently appropriate as per IRAP feeback contingencies).

Hughes, Barnes-Holmes & Vahey, 2012). Instead, RFT is based upon a functional epistemology wherein behaviour is conceptualised solely in terms of its functional (i.e. experimental) relationships with various aspects of the environment unfolding as a function of learning processes across time and context (see Hayes, 1993; Hayes, Hayes & Reese, 1988; Pepper, 1942). By implication, the IRAP was therefore not designed to measure implicit evalauting as a fundamentally different process to the explicit evaluating measured by traditional questionnaire-based self-report measures. Rather, as outlined earlier, the IRAP is designed to measure relatively brief and immediate relational responding (BIRRs) in a highly experimentally controlled fashion (i.e. in the sense of distinguishing responding in terms of one topic versus another). And as such, the only difference between the evaluative responding measured by the IRAP and traditional questionnaire-based self-reports is that the latter measures relatively elaborated and extended forms of relational responding (EERRs) with relatively little experimentally control or thus determinacy (for recent functional theoretical treatments of this continuum see Barnes-Holmes, Barnes-Holmes, Stewart, & Boles, 2010; Hughes, Barnes-Holmes & Vahey, 2012).

Nonetheless, even though the IRAP was conceptualised from a functional theoretical perspective that does not incorporate mentalistic assumptions, this does not mean that existing mental concepts cannot serve as useful guides for defining the behavioural subject matter of IRAP research. In fact, since even before its inception the founders of RFT have recommended using mental concepts as a guide to broad structures in behaviour that require functional explanation (Hayes & Brownstein, 1986, pp. 179-180). And moreover, to this end, during the past four or five years prominent researchers from both the functional and mentalistic traditions have published multiple conceptual papers exploring how these two traditions might usefully (and legitimately) interface to the practical benefit of each other (see De Houwer, 2011; De Houwer, Fiedler, & Moors, 2011; see also De Houwer, Barnes-Holmes, & Barnes-Holmes, 2015; De Houwer, Barnes-Holmes, & Moors, 2013; De Houwer, Gawronski, & Barnes-Holmes, 2013; Fiedler, 2015; Liefooghe & De Houwer, 2015; Vahey & Whelan, 2015).

In particular, mentalistic theories of implicit cognition are typically most concerned with identifying predictive relationships among hypothetical concepts, each invented to fully describe and (from a mechanistic perspective) thus explain some characteristically complex pattern of behavioural relations. Crucially, when it comes to developing a functional account of how implicit evaluating motivates tobacco addiction, these complexes of behavioural relations summarized by mentalistic theory are inherently useful in defining a functional researcher's subject matter. In other words, while discovering relations among different types of implicit evaluating might be an end in itself from the point of view of developing mentalistic theory; by contrast, it represents a starting point for the functional research whose ultimate goal is to understand what contextual variables govern each such relation (see Biglan & Hayes, 1996; Hayes & Brownstein, 1986; Whelan & Barnes-Holmes, 2010).

And yet, as reviewed earlier, the IRAP is relatively uniquely equipped to measure implicit evaluating of one topic as (experimentally) distinct from another, and as such most of the preceeding literature on smoking-related implicit cognition is based upon mentalistic concepts that are functionally nebulous (see Vahey & Whelan, 2015, pp. 349-350). On balance, therefore, even though the ultimate goal of the current research was to pursue a better functional understanding of how smoking-related implicit cognition variously motivates tobacco addiction depending upon context, initially, the current thesis first needed to identify what types of implicit evaluating were most related to tobacco addiction. In particular, it is worth emphasizing that from a functional perspective we were thus not trying to identify what aspects of implicit evaluating caused various tobacco addiction criteria per se, but rather which of those aspects participated most as a motivational facet of those criteria (e.g. cigarette consumption, tobacco cravings, relapse risk etc.; see Hayes & Brownstein, 1986, pp. 179-180; Whelan & Barnes-Holmes, 2010, pp. 349-350). According, the first half of the current thesis was largely concerned with addressing similar questions of predictive validity as typically dominate cognitive theories of tobacco addiction. And then, having established what particular aspects of implicit evaluating are most predictive of tobacco addiction, the latter half of the current research sought to develop a preliminary functional account focused upon understanding how best to contextually control those aspects of implicit evaluating apparently motivating tobacco addiction.

3.4. A Brief Summary of the Research Agenda Pursued in the Current Thesis

Chapter 4: On developing a smoking-related IAT for Maximal Criterion Validity and Juxtaposing the Criterion Validity of an Analogous IRAP against It

The IAT is by far the most popular measure of smoking-related implicit evaluating in the literature, and arguably, the measure of implicit evaluating that has achieved the best criterion validity with regard to tobacco addiction. And yet, as we have already reviewed, smoking-related IAT's have on balance tended to fluctuate unpredictably in their relationships with tobacco addiction criteria. We concluded that this state of affairs was primarily due to the fact that the relative nature of both the IAT's individual tasks, and its scoring, are bound by design to experimentally confound implicit evaluating about different topics with each other. Nonetheless, rather than ruling the IAT out on a completely a priori basis, we reasoned that it would be useful in the current context to explore what criterion validity a smoking-related IAT might achieve if we specifically sought to offset its inherent relativity in our choice of its stimuli. In particular, we sought to coordinate our choice of the four categories of IAT stimuli so that most smokers would customarily respond to all of its trials from a similar smoking-related evaluative perspective likely to be involved in tobacco addiction. Then, once we had developed such an IAT via pilot testing, we designed a corresponding IRAP by analogy. Namely, we developed this IRAP using broadly the same stimuli as the IAT, and so that it would target exactly the same implicit evaluative perspectives as we designed the IAT trials to cue.

Our primary purpose in doing so, was to demonstrate that IRAPs are capable of making key empirical distinctions about tobacco addiction that are just not possible with equivalent gold-standard IAT; and moreover, that such experimental distinctions ultimately yield greater levels of criterion validity in relation to tobacco addiction. Crucially, to our knowledge, this was the first experimental comparison of an addiction-related IRAP with an IAT. And indeed, to date, only one small study has examined smoking-related implicit evaluating with an IRAP; and even then, the primary purpose of doing so was to illustrate, test and contextualise previous IRAP optimisations more broadly (Vahey, Boles, & Barnes-Holmes, 2010). In response, our first empirical study therefore sought to provide a much more comprehensive empirical basis for deciding whether the IRAP might be better equipped to examine tobacco addiction (in particular) than the IAT.

Chapter 5: Developing an IRAP to Make Motivational Distinctions within Tobacco Addiction that are not Currently Possible with Other Implicit Measures

The IRAP we used in our first empirical study was bound by design to examine only implicit evaluating that might be captured within the structural limitations of an IAT. As a result, the relevant IRAP may have measured smokers' implicit evaluating of stereotypical reasons for and against smoking (and the Irish smoking-ban), but it was prevented from exploring how smokers' implicit evaluating of different types of reasons for smoking might relate to tobacco addiction. Our second empirical study was primarily focused upon redressing this limitation. In particular, we designed an IRAP that was capable of measuring smokers' implicit evaluating of smoking for reward versus relief during positive versus negative craving-relatd moods. Crucially, we targeted these specific aspects of smokers' implicit evaluating based upon a longstanding and empirically well supported consensus within the literature on tobacco addiction, that these motivational distinctions are pivotal to tobacco addiction. For example, as we will review in Chapter 5, there is extensive questionnaire-based self-report evidence (and indeed theory) suggesting that smokers' differentially evaluate smoking as providing reward versus relief during positive and negative moods, respectively. And moreover, that this is a highly characteristic and ritualized smoking strategy commonly used by smokers as a means of regulating their emotions on an ongoing basis. Furthermore, given that smoking for relief from negative affect is the pattern of smoking most commonly linked to a gradual intensification of tobacco addiction, we expected that the IRAP trial-type measuring this aspect of smokers' implicit evaluating would be particularly predictive of the various tobacco addiction criteria we employed (i.e. as compared to the more general pro-smoking implicit evaluating we measured in our first empirical study, and also as measured by the three remaining trial-types in this our second empirical study).

Chapter 6: A First Systematic Test of Thought Suppression as a Means of Bringing Intrusive Pro-smoking Implicit Evaluating Under Self-control

The third and fourth empirical studies contained within the current thesis sought to provide a first momentary time-course analysis of how persistently and/or consistently thought suppression impacts any (smoking-related) implicit evaluating it contradicts (see McKay, Franklin, Patapis, & Lynch, 2006; Shiffman, 2009; see also Marhe, Waters, van de Wetering, & Franken, 2013; Waters et al., 2010). In particular, from the point of view that thought suppression may sometimes be useful in temporarily postponing (tobacco) cravings, we examined how immediately and persistently effective it would be in eliminating implicit evaluating integral to tobacco addiction. And as such, the research contained within the current chapter provided a first glimpse into the dynamics of how thought suppression impacts the intrusive aspects of implicit evaluating that it is generally used to contradict. Namely, our third study focused upon the impact of the ad hoc smoking-related thought suppression most commonly used by smokers during (unaided) abstinence, on those broad aspects of smokers' pro-smoking implicit evaluating that appeared to be integral to tobacco addiction in our first empirical study.

Then, in our fourth empirical study, we refined our analysis by exploring the widely speculated idea that focused thought suppression of particular aspects of tobacco cravings might be more successful than an ad hoc approach at postponing and/or controlling any such intrusive pro-smoking implicit evaluating. In so doing, we were particularly keen to examine whether there would be a differential impact of focused thought suppression on aspects of pro-smoking implicit evaluating that it specifically contradicted versus not. Crucially, not only was this approach necessary in order to isolate the efficacy of any given instance of thought suppression (i.e. relative to the particular aspect of tobacco craving it was targeting). Moreover, it was also necessary to explore whether individual instances of focused thought suppression might inadvertently provoke secondary tobacco cravings, in turn requiring their own respective versions of thought suppression, and thus perpetuating an escalation of ad hoc patterns of thought suppression. Thus, our aim was to provide a first preliminary model of how (anti-smoking) thought suppression interacts over time, on a minute by minute basis, with the intrusive implicit evaluating it is designed to contradict.

Chapter 7: A First Examination of How Smokers' Implicit Evaluating is moderated by Suppression-oriented Nicotine Abstinence during Acute Stress

Our fifth and final study sought to extend our analysis of the feasibility of anti-smoking thought suppression to a more ecologically valid and indeed pivotal context for tobacco addiction. Namely, we examined how smokers' smoking-related and quitting-related implicit evaluating developed across 14-24 hours of suppression-oriented unaided nicotine abstinence; and indeed crucially, how this implicit evaluating responded on a minute by minute basis to acute stress during any such abstinence versus not. In particular, we deemed the latter part of our analysis as likely being critical in explaining why the literature had generally failed to find any changes in smoking-related implicit evaluating as a result of nicotine deprivation per se.

CHAPTER 4: Testing the IRAP versus the IAT as Tools for Targeting Smoking-related Implicit Evaluating (Study 1)

4.1. INTRODUCTION

The IAT literature has provided very little consensus about the role of implicit evaluating in tobacco addiction. For the most part, this lack of consensus is a direct result of the fact that an IAT is constrained by design to measure smoking-related evaluating in arbitrarily relative terms; namely, relative to some contrasting concept, and/or relative to two contrasting attributes, in unspecified ways. In particular, IAT researchers have to date been unable to identify any concept that stereotypically contrasts in a consistent manner with smoking. Instead, they have resorted to using contrast categories that are ambiguously related to smoking (e.g. ‘Sweets’, ‘Stealing’; Swanson et al., 2001), or else contrast categories that allude to some vague absence of smoking (e.g. ‘Nonsmoking’; De Houwer et al., 2006). And as a result, smoking-related IAT scores are not only indeterminate with respect to participants’ evaluations of smoking per se, but worse still, their relationship with tobacco addiction criteria has also fluctuated unpredictably from study to study as a function of changing contrast categories (see Swanson et al., 2001; Tibboel et al., 2011).

In response, rather than arbitrarily choosing an unstable benchmark for the IRAP from among existing smoking-related IATs, we made the first overt attempt to design a smoking-related IAT that would systematically minimise its instability (and thus indeterminacy) with respect to smoking-related implicit evaluating. Our basic strategy was to design a smoking-related IAT so that smokers (and non-smokers) would consistently interpret its trials in a minimum number of ways all of which would be consistently relevant to tobacco addiction. Crucially, our intention was to design the most determinate and valid smoking-related IAT possible, so as to obtain the best quality benchmark against which to compare an analogous IRAP. In doing so, we adopted a very conservative approach, whereby the current IRAP was artificially restricted by design to measure only those aspects of smoking-related implicit evaluating that were most accessible in principle to an IAT. With these things in mind, we chose ‘Smoking-Ban’ as a suitable IAT contrast category for ‘Smoking’ because of the likelihood that our chosen sample would commonly consider “the Smoking-Ban” as being opposite to smoking in just a small number ways with respect to the positive versus negative smoking-related mood attributes we incorporated. Namely, we chose positive mood attributes, like “Relaxed”, that are among the most stereotypical reasons that smokers offer for smoking; and likewise, we chose corresponding antonymic negative mood attributes, such as “Anxious”, whose reduction is also a stereotypical reason that smokers offer for smoking (e.g. DiFranza, Savageau, Fletcher, Ockene, Rigotti, McNeill, et al., 2002; DiFranza, Ursprung, & Biller, 2012; O’Loughlin et al., 2002; Pfizer Ireland, 2009, pp. 12-19; Ursprung, DiFranza, Costa, DiFranza, 2009; Vahey et al., 2009). In addition, those positive and negative mood attributes were also selected to be in keeping with how non-smokers typically report that the Smoking-ban makes them feel (i.e. at the time, there was prolonged, widespread and highly consistent discourse among the Irish public about Ireland’s introduction of the World’s first outright smoking-ban in workplaces; Clancy, 2007; Fahy, Trench & Clancy, 2012).[36]

Another important reason why we chose ‘Smoking-Ban’ as a useful contrast category for ‘Smoking’ was the opportunity it provided to contrast stigmatized versus non-stigmatized aspects of smoking-related evaluating. We reasoned that regardless of their private feelings, the current smokers were stigmatized for explicitly criticizing the Irish Smoking-ban because of the immediate and near-universal compliance of the Irish public with its restrictions (Clancy, 2007, p. 239-241; Fahy et al., 2012). On this basis, we predicted that the current smokers would explicitly approve of the Irish Smoking-ban due to societal pressures for doing so, despite implicitly affirming that it makes them feel bad (i.e. due to the nicotine deprivation and stigma it routinely imposed upon smokers in Ireland; Lonergan, 2013). By contrast, we expected the smokers to approve of smoking both implicitly and explicitly due to the fact that they were not stigmatized for explicitly affirming that smoking makes them feel good (i.e. it provides smokers and non-smokers alike with one of the most popularly accepted explanations for tobacco addiction; e.g. DiFranza et al., 2002, 2012; O’Loughlin et al., 2002; Ursprung et al., 2009; Vahey et al., 2010). Thus, we used the susceptibility of explicit evaluating to social desirability bias to highlight how the IRAP, but not the IAT, can detect conflicts between specific types of implicit versus explicit evaluating (i.e. addressing the same respective topics).

In addition, an essential part of the current approach was to coordinate our choice of IAT stimuli with highly targeted sampling of the participants. For example,

[36] We decided to use the traditional IAT format (i.e. of a target concept, a contrast concept and two contrasting attribute classes) because all IAT variants have comparably little supporting evidence, and because what evidence does exist suggests that they have failed to outperform (or sometimes even match) the traditional IAT format psychometrically (see Bar-Anan & Nosek, 2014b; see also our introductory review of the smoking-related IAT literature).

we sampled regular smokers who had not contemplated or attempted to quit smoking during the previous 12 months – we reasoned that any other smokers, who had recently been attempting to quit smoking, would have been inclined to have relatively mixed evaluations of smoking, the Irish Smoking-ban, and therefore about the stimulus combinations employed in the current IAT. Likewise, we specifically sampled only those non-smokers who would be least likely to customarily exhibit mixed evaluating of smoking or the Irish Smoking-ban; namely, non-smokers who had never regularly smoked (i.e. who had not smoked at all during the previous 12 months and had only ever tried smoking a cigarette up to a maximum of 10 times; and for those who had, the majority reported only one or zero such occasions). Overall, therefore, the central hypothesis of the current study was that the IAT and the IRAP would both vary in line with known-group differences related to smoking-status but that crucially the latter would provide more information about implicit evaluating in doing so.

Above all, the current research was not just about examining whether a smoking-related IRAP might rival a corresponding IAT in terms of correlating with tobacco addiction criteria. Rather, it was as much about demonstrating that IRAPs are capable of making key empirical distinctions about tobacco addiction that are just not possible with equivalent IATs. Therefore, in addition to incorporating standard cross-sectional evidence for the criterion validity of both implicit measures, we introduced novel statistical analyses of IAT and/or IRAP data that were specifically designed to highlight the added precision afforded by the IRAP (see Results). To our knowledge, this was the first such experimental comparison of a smoking-related IAT with a counterpart IRAP.

4.2. METHOD

Known-groups Sampling Inclusion and Exclusion Criteria

The primary aim here was to sample two groups of people, one with a stable behavioural history of regularly smoking for at least one year (i.e. smokers), versus another with a behavioural history within which smoking was generally irrelevant (i.e. non-smokers). Accordingly, of the 110 participants recruited, 18 were excluded from subsequent analyses because they were transitioning from smoker to non-smoker, or vice versa. Of the remaining 92 participants, 48 were smokers (23 females) who had not recently committed, or attempted to restrict their smoking; and 44 were non-smokers (23 females) who had not smoked any substances during the previous 12 months or indeed on more than 10 occasions before that. All participants were recruited for experimentally naivety to both the IAT and the IRAP, and all were similarly exposed to the Irish Smoking-ban (i.e. all were continuously resident in Ireland throughout the 2-3 years since its introduction at the time of the study).

Key Demographics of the Smoker and Non-smoker Samples

Participants were recruited throughout the Maynooth University campus, based upon whether they were observed to be smoking in the case of the smokers, and based upon whether they self-identified as never-smokers in the case the non-smokers. More specifically, the researcher approached potential participants in person and invited them to provide their contact details should they wish to be contacted by phone at a later point to dicuss the possibility of taking part in various studies examining tobacco addiction. Overall, smokers (M_{age} = 26.4 years; range = 18 - 44; *SD* = 7.3) and non-smokers (M_{age} = 24.9 years; range = 18 - 45; *SD* = 6.0) were closely matched in terms of age, $t(90) = 1.03$; $p = .30$. On average, the smokers reported that they had been smoking regularly for 9.2 years (*SD* = 6.9; *range* = 0.75-26), and that they consumed 15 cigarettes per day (*SD* = 7.0, *range* = 4-38).[37] Similarly, most of these smokers exhibited moderate (physiological) nicotine dependence on the modified Fagerstrom Tolerance Questionnaire (mFTQ; *M* = 3.6; *range* = 1.3-6.0; *SD* = 1.3; see Prokhorov, Pallonen, Fava, Ding, & Niaura, 1996; Prokhorov, Koehly, Pallonen, & Hudmon, 1998, p. 42); and moderate psychological tobacco dependence on the Hooked on Nicotine Checklist (HONC; *M* = 6.8; *range* = 1-10; *SD* = 2.5; cf. Wellman, Di Franza, Pbert, et al., 2005; Wellman, Savageau, et al. 2006).

Random Assignment of Smokers and Non-smokers to Experimental Groups

Smokers and non-smokers were randomly assigned to complete either a smoking-related IAT or an analogous IRAP. In addition, for counterbalancing, all participants were also randomly assigned between two levels of a *trial block order* variable such that, they either commenced the relevant IAT/IRAP with a block of trials requiring pro-smoking/anti-smoking-ban versus anti-smoking/pro-smoking-ban responses (i.e. termed *pro-smoking-first* versus *anti-smoking-first* sequences, respectively). Participants in the IAT groups were well matched with the IRAP groups insofar as they did not differ significantly in terms of their age, the number of years they had been smoking, CPD, HONC scores, or mFTQ scores (*p*s > .4).

[37] 71% of the smokers reported smoking on a daily basis throughout the 30 days preceding participation; a further 18% reported having smoked between 20 and 29 of these days; and only 11% reported smoking on 10 to 20 of these 30 days. Indeed, upon completion of the current study, 68% of smokers reported that they would 'definitely' smoke a cigarette upon leaving, and the remaining 14% and 18% respectively reported that they would 'probably' and 'possibly' do so (i.e. none selected the alternative of 'definitely not').

Apparatus and Materials

State and Trait Measures of Tobacco Addiction Intensity

Participants completed two popular trait measures of tobacco addiction: the modified Fagerstrom Tolerance Questionnaire (mFTQ; Prokhorov, Koehly, Pallonen, & Hudmon, 1998, p. 42) and the Hooked on Nicotine Checklist (HONC; DiFranza et al., 2002). Both measures are highly reliable and widely used measures of tobacco addiction (e.g. Sanouri et al., 2009; Wellman, Di Franza, Pbert et al., 2006). Whereas the more established mFTQ is primarily focused upon relatively severe behavioural and physiological features of nicotine dependence (Piper, McCarthy, & Baker, 2006), the HONC complements the mFTQ by focussing instead upon psychological aspects of tobacco dependence (i.e. subjective diminished autonomy over tobacco smoking; MacPherson, Strong, & Myers, 2008; Wellman, DiFranza, Savageau, Godiwala, Friedman, & Hazelton, 2005). In the present study the mFTQ achieved an α = .68 for smokers, and an α = .90 when smokers and non-smokers were considered collectively; the HONC achieved an α = .77 for smokers, and an α = .95 for smokers and non-smokers considered collectively. Participants also completed a measure of *state tobacco addiction intensity*: a single item Likert-type question concerning their current craving for a cigarette. It consisted of the request "Please indicate how much you currently **want** to smoke" followed by a Likert scale ranging from -3 labeled 'Not At All', to 0 labeled 'Moderately', and finally to +3 labeled 'Very Much'. Lastly, a questionnaire called the *demographic and behavioural history questionnaire (DBHQ)* was used to conduct exhaustive semi-structured interviews with participants regarding their current and historical smoking-status as well as their basic demographics (see Appendix 2). The DBHQ was crucial in implementing the known-groups agendas described above, and it also measured two additional trait tobacco addiction criteria which bore much fewer theoretical assumptions than the mFTQ and HONC. Namely, participants' self-reported estimates of: (a) how many years they had been smoking regularly (i.e. years smoking; YS); and (b) of how many cigarettes per day (CPD) they had smoked on the days they smoked during the previous 30 day period.

The IAT and the IRAP as Measures of Smoking-related Implicit Evaluating

The IAT and the IRAP software was presented to participants on a Dell desktop computer running Windows XP Service Pack 2 with a 17-inch color monitor. In both cases the relevant software was coded using Visual Basic 2008 Professional Edition and was compiled into a stand-alone executable program. Participant responses were written directly by the program to a comma-delimited text file, which also transformed the relevant raw latency data into the standardised IAT and IRAP scores described below in Section 4.3.3 respectively.

The IAT

Basic presentation and response formats. As per Table 4.1, the IAT employed four six-item classes of *target* stimuli: two *attribute* classes and two *concept* classes. Every IAT trial commenced by presenting one of these target stimuli centre-screen, whereupon the participant's task was to categorise it as quickly as possible by selecting the *category label* which best described it from the top left- versus right-hand corners of the screen. The current IAT used four such category labels with each designating one of the stimulus classes in Table 4.1: "Smoking", "Smoking-Ban", "Positive", or "Negative". As per the rationale laid out earlier, we chose six antonymic pairs of positive versus negative mood attributes as target exemplars of the positive versus negative attribute categories. In contrast, given that not all smoking-related exemplars have natural antonyms relating to the Irish smoking-ban, we independently choose six target exemplars for the smoking category and six for the smoking-ban category (see Table 4.1). Our main concern here, was to choose concept exemplars to be as emblematic of the smoking versus the smoking-ban, as appropriate, in the common vernacular. Thus, for example, we chose to present the target stimulus "NO SMOKING" in all capitals because this is how this phrase is normally presented in relation to smoking restrictions in the general Irish vernacular (i.e. on so-called no smoking signs).

Table 4.1
The four classes of target stimuli that were presented centre-screen across IAT trials.

Concept stimulus classes		**Attribute stimulus classes**	
'Smoking' concept stimuli	***'Smoking-Ban' concept stimuli***	***'Positive' attribute stimuli***	***'Negative' attribute stimuli***
Cigarette-pack	Restrict	Relaxed	Tense
Match-box	Anti-smoking	Pleasant	Unpleasant
Marlboro Lights	Non-smoking	Comfortable	Irritable
Tobacco	Prevent	Better	Worse
Inhale	Smoke-free	Sociable	Withdrawn
Nicotine	NO SMOKING	Calm	Stressed

On the trials which presented concept stimuli as targets, the labels “Smoking” and “Smoking-Ban” always appeared on opposite upper corners of the screen, and likewise on trials which presented attribute target stimuli the “Positive” versus “Negative” labels also always appeared on opposite upper corners of the screen. Furthermore, concept stimulus trials could either appear with just the concept labels as response options, or else with the concept labels positioned just under the attribute stimulus labels on opposite upper corners of the screen. And likewise, the attribute stimulus trials could either appear with just the attribute stimulus labels as response options, or else with the attribute stimulus labels above the concept stimulus labels on opposite upper corners of the screen (see Figure 4.1).

Figure 4.1. The three panels show how IAT trials were presented to participants, with the upper and middle panels representing trials that involved categorizing a target word exclusively as one of two antonymic stimulus classes, and with the bottom panel representing trials that involved categorizing a target word as one of four stimulus types in a pro-smoking manner.

Crucially, participants could only ever register their response to a given IAT trial in one of two ways using a QWERTY keyboard: either by selecting the label(s) on the upper left corner of the screen by pressing the ‘d’ key or selecting the label(s) on the upper right corner of the screen by pressing the ‘k’ key (see Figure 4.1). Therefore, when the “Smoking” and “Positive” labels appeared together on one upper corner of the screen, and the “Smoking-Ban” and “Negative” labels appeared on the other upper corner of the screen, participants were intended to categorise the target stimuli in a *pro-smoking* manner. And conversely, when the “Smoking” and “Negative” labels appeared together on one upper corner of the screen, and the “Smoking-Ban” and “Positive” labels appeared on the other upper corner of the screen, this was assumed to encourage participants to respond in an *anti-smoking* manner. The IAT actively cultivated these respective patterns of responding with both instructions and with ongoing response feedback to participants. First, the IAT involved asking participants to complete its trials as quickly and as accurately as possible. Second, the IAT presented participants with a red ‘X’ in the middle of the screen whenever they provided a trial response to a given target that did not correspond to its designated label; and the red ‘X’ remained onscreen until they emitted the relevant correct response. Namely, once participants responded by pressing the key assigned to a given target’s designated label, the IAT then displayed a blank white screen for 400 *ms* before presenting the next target word.

A summary of the IAT trial block sequencing. The IAT comprised of a total of seven blocks of trials which were demarcated by interludes offering participant instructions. We mentioned earlier that participants were counterbalanced for trial block order, and for clarity we will describe the pro-smoking-first IAT sequence in detail. The anti-smoking-first IAT sequence was the same as the pro-smoking-first sequence except that relative to the former, the latter had blocks 1, 3, and 4 switched with Blocks 5, 6 and 7, respectively (see Table 4.2).

For those in the pro-smoking-first condition the first block of trials they completed comprised of 24 trials wherein the 12 concept stimuli in Table 4.1 were randomly presented twice without replacement and in the presence of just the “Smoking” and “Smoking-Ban” concept labels as response options. Although the left-right positioning of these two concept labels was counterbalanced among participants for block 1, importantly, their positioning remained static for each participant throughout block 1.

Block 2 of the pro-smoking-first sequence then required participants to complete 24 trials wherein all of the 12 attribute stimuli in Table 4.1 were randomly presented without replacement twice and in the presence of just the two attribute stimulus labels as response options. Crucially, the left-right positioning of the attribute stimulus labels coordinated with the presentation of the concept labels in Block 1 such that throughout Block 2 participants were presented in Block 1 with the "Positive" label on the same side as the "Smoking" concept label and the "Negative" label on the same side as the "Smoking-Ban" concept label; thus for each participant the attribute labels were presented in the same left-right position throughout block 2.

Table 4.2

A summary of the basic functional architecture of the seven IAT trial blocks.

IAT pro-smoking-first sequence			
IAT Block Sequence	**Number of IAT trials**	**Stimulus classes assigned to the left response-key****	**Stimulus classes assigned to the right response-key****
1st	24	"Smoking"	"Smoking-Ban"
2nd	24	"Positive"	"Negative"
3rd	24	"Smoking" & "Positive"	"Smoking-Ban" & "Negative"
4th	48	"Smoking" & "Positive"	"Smoking-Ban" & "Negative"
5th	24	"Smoking-Ban"	"Smoking"
6th	24	"Smoking-Ban" & "Positive"	"Smoking" & "Negative"
7th	48	"Smoking-Ban" & "Positive"	"Smoking" & "Negative"

Note. For the anti-smoking-first sequence blocks 1, 3 & 4, were respectively switched with blocks 5, 6, & 7. Also, assignment of stimuli to left and right response-keys was counterbalanced across participants.

Block 3 of the pro-smoking-first sequence required participants to 'practise' 24 pro-smoking trials wherein all of the 24 stimuli in Table 4.1 were randomly presented once without replacement in the presence of all four response labels in the same respective left-right positions as in Blocks 1 and 2.

Block 4 of the pro-smoking-first sequence required participants to complete 48 pro-smoking 'test' trials wherein all of the 24 stimuli in Table 4.1 were randomly presented twice without replacement with the four response labels in the same respective left-right positions as in Blocks 3.

Block 5 was the same as block 1 of this sequence in all respects except that for each participant the left-right positioning of the "Smoking" versus "Smoking-Ban" labels were transposed.

Block 6 was the same as Block 3 except that it required participants to categorise concept and attribute target words in an anti-smoking manner (i.e. "Smoking" and

"Negative" labels with one response key, and "Smoking Ban" and "Positive" labels with the other key).

Block 7 was the same as Block 4 (48 trials), except that Block 7 used the same key assignments as Block 6, and the following message appeared onscreen at the end of the block: "That is the end of this part of the experiment. Please report to the experimenter.''

The IRAP

Basic presentation and response formats. Each IRAP trial involved the simultaneous presentation of a *label* stimulus at the centre top of the computer screen, a *target* stimulus in the middle of the screen and lastly two mutually exclusive response designators, "True" versus "False", at opposite corners of the bottom of the screen. Table 4.3 shows the two specific *'concept'* labels and six positive versus six negative *'attribute'* target stimuli employed within the IRAP.[38] Thus, the IRAP included four different IRAP *trial-types*, each having six versions (i.e. obtained by crossing the two different concept labels with the two different six-member classes of attribute target stimuli; see Figure 4.2). Two trial-types attributed various desirable versus aversive mood outcomes to smoking, and the other two trial-types attributed those same mood outcomes to the smoking-ban. Therefore, the "True" versus "False" response options on each IRAP trial always specified particular *pro-* versus *anti-smoking* response options defined by that trial's particular label and target combination (see Table 4.3 & Figure 4.2).

Table 4.3
The current IRAP's two 'concept' labels and two 'attribute' target stimulus classes.

'Concept' Labels		'Attribute' Targets	
Smoking Concept	Smoking-Ban Concept	Positive Moods	Negative Moods
Smoking makes Me Feel	The Smoking Ban makes Me Feel	Relaxed Pleasant Comfortable Better Sociable Calm	Tense Unpleasant Irritable Worse Withdrawn Stressed

[38] As an aide-mémoire, we incorporate IAT nomenclature here in referring to the IRAP label stimuli as *'concept'* label stimuli, and to the IRAP target stimuli as *'attribute'* target stimuli. Note however, that these terms do not generalize to all IRAPs. Rather, there are no *a priori* constraints upon the stimuli that can be used together as label, target and response designator stimuli within a given IRAP's trials (i.e. including even characteristic pictures, sounds or textures), other than the fact that these stimuli must together convey an unambiguous collective meaning for the participant on each trial.

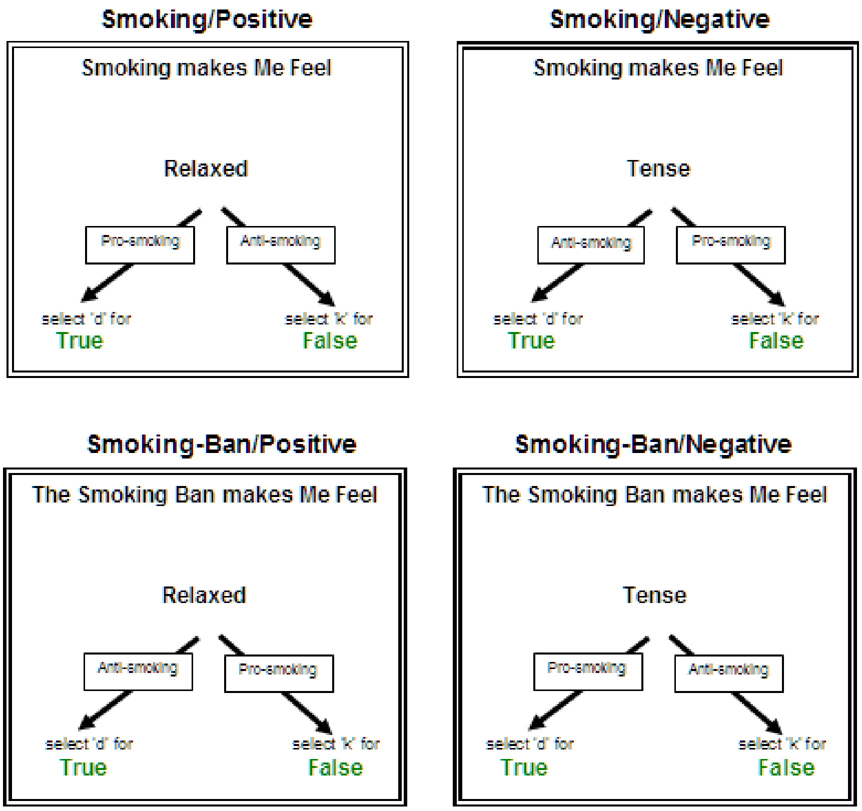

Figure 4.2. The pro- versus anti-smoking relational response options offered by each of the four IRAP trial-types. Arrows with superimposed text boxes indicate which response options were deemed pro-smoking versus anti-smoking; boxes and arrows did not appear on screen. One concept label stimulus ('Smoking makes Me Feel' versus 'The Smoking-Ban makes Me Feel'), a mood-based attribute target stimulus (the 'positive' versus 'negative' moods listed in Table 4.3), and both response designators (*True* versus *False*) appeared simultaneously on each trial. The left-right locations of the *True* and *False* response designators varied randomly from trial to trial with the constraint that they did not appear in the same locations across more than two successive trials.

How the IRAP cultivated fast and accurate responses to its trials. A core feature of the IRAP was that it tasked participants with respectively providing only pro- versus anti-smoking responses on alternate blocks of trials; and with as much speed and accuracy as practicable. In line with previous research, we established via pilot testing that 3000*ms* was the fastest median response latency that the bulk of the current population could realistically maintain at a response accuracy of 80% across any given block of trials (i.e. with the particular stimulus composition and instruction contingencies employed by the current IRAP; cf. Vahey et al., 2010). For example, given that the IRAP randomly swapped the left-right onscreen positioning of the "True" versus "False" response designators from trial to trial (i.e. unlike the IAT's response options which remained static within each block of trials these randomly switching options counterbalanced for the likely confounding effects of right versus left hand dominance in fast responding), participants were instructed on each trial to select 'd' for the response designator presented on the bottom left of screen and to select ‘k’ for the response designator presented on the bottom right of screen (see Figure 4.1, noting that the ‘d’ and the ‘k’ keys are respectively on the middle left and middle right sides of a QWERTY keyboard). Thereafter, selecting the pro-smoking response option during a pro-smoking IRAP trial, or the anti-smoking option during an anti-smoking IRAP trial, cleared the screen for 400 *ms* before the next trial was presented; if the anti-smoking option was chosen during a pro-smoking trial, or the pro-smoking option during a anti-smoking trial, a red ‘X’ appeared on screen underneath the target word until the participant emitted the alternative, correct response. In addition, at the end of each block of trials, the IRAP issued summary feedback and instructions about the accuracy and speed of the participant’s responding during the block of trials just completed.

Crucially, participants were not allowed to progress to the IRAP test trials until they completed a given pair of pro- versus anti-smoking practise blocks with respective median response latencies of less than 3000*ms* and with respective average accuracies equal to or greater than 80%. In line with previous findings, the researcher took particular care during IRAP practise trials to coach participants in achieving and maintaining these response criteria (i.e. in addition to the automated instructions and feedback contingencies issued by the IRAP; see Vahey et al., 2009; 2010). In particular, the researcher instructed participants before each pro-smoking practise block to “Respond as if smoking makes you feel *good*, and the Irish Smoking-ban makes you feel *bad*”, and before each anti-smoking practise block to “Respond as if smoking makes you feel *bad*, and the Irish Smoking-ban makes you feel *good*”. Upon successfully completing the IRAP practise phase, participants were then instructed to strive for ever quicker responding so long as it did not undermine their response accuracy from trial-to-trial.

A summary of IRAP trial block sequencing. The complete IRAP comprised of between one and four successive pairs of pro-smoking versus anti-smoking blocks of practise trials (i.e. depending upon how quickly participants achieved the aforementioned response latency and accuracy criteria), which were always followed by exactly three successive pairs of pro-smoking versus anti-smoking blocks of *test* trials. Crucially, as is standard, only the IRAP test trials were used for measurement purposes. In any case, each IRAP trial block comprised of 24 trials (i.e. one trial for each of the six versions of each IRAP trial-type; see Table 4.3), presented in quasi-random order such that the same IRAP trial-type was not repeated across two successive trials. In addition, like with the IAT, we counterbalanced the order in which successive participants received pro- versus anti-smoking trial blocks from their first pair of practise blocks onwards to the end of the IRAP test phase (i.e. the trial block order variable mentioned above). Otherwise, the pro- versus anti-smoking-first IRAP conditions were identical to each other.

Measures of Explicit Evaluating

Feeling Thermometers

We used separate feeling thermometers, derived from those used in earlier IAT research (Swanson et al., 2001), to measure participants' global explicit evaluating of both smoking and the Irish Smoking-ban. These feeling thermometers were essentially vertical visual analogue scales graduated in 10 point increments from 0 to 99, with zero points labelled "Extremely Cold (Unfavourable)", midpoints at 50 labelled "Neutral", and their apexes at 99 labelled "Extremely Warm (Favourable)". Participants were asked to use their initial "gut" responses to complete the two statements using the thermometer scale: either "*Smoking* Makes Me Feel" or "The *Smoking-Ban* Makes Me Feel", respectively. Having done so, they were then asked to further clarify their answer in the following way: "Having placed an X on the thermometer please specify exactly which number it represents on the 0-99 scale."

Semantic Differentials

We also used six semantic differential scales ranging from -3 to + 3 to address the question "How does *Smoking* make you feel?" with one scale for each of the attribute stimulus pairs listed in Tables 4.2 and 4.3 for the IAT and IRAP, respectively; and a further six scales for the question, "How does *The Smoking Ban* make you feel?" using the same six attribute stimulus pairs. In other words, the semantic differentials were specifically designed to measure explicit evaluating that corresponded to the implicit evaluating measured by the current IRAP (and perhaps the current IAT). In order to discourage inattentive responding, we sometimes labelled positive mood attributes with +3 and negative mood attributes with -3 and at other times labelled them vice versa.

Procedure

To minimize floor or ceiling effects in smoking-related cravings, the researcher individually reminded participants (via text-messaging) to smoke as usual up until an hour before taking part in the study before then abstaining fully until completion (see Field et al., 2009; Schuh & Stitzer, 1995). Before participating each smoker was required by the researcher to confirm that they had smoked as normal up until an hour beforehand and fully abstained thereafter. Importantly, these precautions appear to have been successful insofar as the smokers in the IRAP and IAT groups both reported moderate tobacco cravings at completion (i.e. mean tobacco craving for the IAT and IRAP smoking groups were respectively -.2 and +0.6, with *SD*s of 1.8 and 1.9).

Following completion of the IAT versus the IRAP (i.e. depending upon experimental assignment), all participants completed the four measures of explicit evaluating. Then, all participants completed the state and trait questionnaires measuring smoking compulsion intensity. Apart from initially receiving instructions and/or coaching during IRAP practise trials from the researcher, each participant completed these research tasks alone in a sound-proofed experimental cubicle. Then, lastly the researcher interviewed all participants using the DBHQ in an open-ended fashion to reveal any unanticipated details with relevance to the current known-groups sampling agenda.

Ethical Considerations

The present research, as in Studies 2, 3 and 4 to follow, was approved by the Research Ethics Committee at Maynooth University. Each participant provided informed consent on their own behalf based explicitly on the idea that their participation was on an anonymous basis; and on the idea that they could voluntarily withdraw their participation from the study at any point in time and without explanation.

4.3. RESULTS

4.3.1. An Overview of the Current Statistical Analyses

First, having performed various data preparation procedures, we began with a series of standard known-groups comparisons and in so doing showcased a new approach to IRAP data analysis. Crucially, this new approach of analyzing each IRAP's trial-type effects by crossing the IRAP concept-type and target-type variables with each other provided the first means of systematically estimating the extent to which an IRAP's four trial-type effects operated independently of each other with respect to any given experimental variable such as smoking-status. By contrast, IRAP research to date has typically combined IRAP trial-type scores with little if any stated empirical or a priori rationale for doing so (e.g. see Hussey, Thompson, McEnteggart, Barnes-Holmes, & Barnes-Holmes, 2015, pp. 160-162). And as such, the current approach to analysing the independence of trial-type effects offered the IRAP literature a more objective, systematic and indeed precise means of determining whether or not it is functionally appropriate to combine IRAP trial-type scores in one's analysis or not

Second, we widened the known-groups analysis to compare each implicit measure's sensitivity and specificity with respect to smoking-status. As part of this analysis we introduced the first direct means of quantifying the full range of trade-offs between the *sensitivity* (i.e. true positive rate) versus *specificity* (i.e. true negative rate) of any diagnostic technique. And in so doing, we revealed evidence of the inherently confounded nature of IAT (but not IRAP) scores.

Third, we juxtaposed how well each implicit measure correlated with five core tobacco addiction criteria: years smoking (YS), tobacco cravings (TC), and three measures of trait tobacco addiction intensity (i.e., CPD, mFTQ and HONC). Fourth, we introduced the first causal model of smoking-related IAT/IRAP scores to test the extent to which they might strengthen over time (YS) as a casual function of trait tobacco addiction intensity. We offered this novel analysis to illustrate how an IRAP, but not an equivalent IAT, is capable of distinguishing what types of implicit evaluating might be causally involved in tobacco addiction from those which are unlikely to be. Fifth, we used stigmatized versus non-stigmatized aspects of smoking-related explicit evaluating to highlight how an IRAP, but not an IAT, can reveal which types of (pro-smoking) evaluating are masked in corresponding measures of explicit evaluating.

Sixth, we compared the internal reliabilities yielded by both implicit measures. A core goal here was to highlight how varying patterns of internal reliability among IRAP trial-types could itself measure properties of implicit evaluating that were not only inaccessible to an IAT, but which had not previously been calibrated within the IRAP literature. As part of this analysis, we highlight for the first time why it is deeply misrepresentative to directly compare the internal reliability of D_{IAT}s to *trial-type* D_{IRAP}s without accounting for the number of trials comprising each measure, and also the precision with which they target specific types of implicit evaluating.

Lastly, we based all subsequent analyses in the current thesis upon effect size statistics in recognition of the fact that prominent journals such as *Psychological Science* have effectively banned *null hypothesis significance testing (NHST)* in favour of an emphasis upon the replicability of effect size statistics (Eich, 2014; see also Trafimow & Marks, 2015). Given that the particular IAT and IRAP used here had not previously been used in the literature we decided to adopt the relatively conservative approach of disregarding effect sizes that would conventionally be considered small according to Cohen's (1988) widely-used effect size standards (e.g. ds < .35; rs < .20; *Cramer's V*s < .10, .07, .06 for dfs = 1, 2, 3; η_p^2s < .04). Crucially, while awaiting calibration within a given domain,[39] Cohen's (1988, pp. 25, 284-287) standards offer a

[39] As Cohen (1988, p. 25) himself cautioned such effect size standards are somewhat arbitrary until calibrated via empirical research within a given domain.

consistent preliminary basis upon which to compare research findings (i.e. unlike *p* values which fluctuate not just as a function of the effects in question but also as an irrelevant function of sample size). Thus, even though not strictly necessary, we included *p* values in the current analyses only to accommodate the vast majority of psychological researchers who are still practicing NHST. We hope that by providing *p* values in the context of an analysis driven by effect size standards it will better illustrate for any *NHST*-practitioner how an effect size analysis operates in practical terms (i.e. by repeatedly exemplifying where the two analytic frameworks agree versus disagree).

4.3.2. Screening the IRAP dataset for Accuracy and Speed

Based on pilot testing, we decided to exclude a participant's entire IRAP test data whenever it did not adhere to a minimum average response accuracy of 80% and a maximum average response latency of 3000*ms*. In all, two smokers and one non-smoker had their IRAP data excluded on this basis yielding 22 smokers and 22 non-smokers with eligible IRAP data (i.e. out of the 49 participants assigned to the IRAP condition).

4.3.3. The IAT and IRAP Scoring Algorithms

Participants naturally exhibit differing response speed variability due to a range of extraneous individual differences such as in age, cognitive ability, motor skills, or previous experience with the IAT (see Back, Schmukle, Egloff, & Gutenberg, 2005; Cai, Sriram, Greenwald, & McFarland, 2004; Greenwald et al., 2007; Mierke & Klauer, 2003; Nosek, Bar-Anan, Sriram & Greenwald, 2012). We sought to control for such contaminating effects using so-called *D-algorithms*. In basic terms, these *D*-algorithms served to statistically standardize pro- versus anti-smoking latency difference scores, whether applied to IAT or IRAP data. The original method, called the 'improved scoring algorithm', was developed for the IAT by Greenwald, Nosek, and Banaji (2003; p. 213), and due to its effectiveness this scoring algorithm has become customary within the IAT literature (for review of the algorithm's development see Nosek, Greenwald, & Banaji, 2007, pp. 272-273). The *D*-algorithm we applied to the IRAP data here is a direct adaptation of Greenwald et al.'s algorithm. Indeed, it uses exactly the same statistical rationale as the D_{IAT}-algorithm and it has likewise proven similarly effective at taking account of confounding variables related to cognitive ability (O'Toole & Barnes-Holmes, 2009; Vahey et al., 2009, 2010). However, whereas the *D*-algorithm yielded one score called a D_{IAT} when applied to the IAT, the version adapted for the IRAP yielded four so-called *trial-type* D_{IRAP} scores. For brevity's sake, the D_{IAT}-algorithm is detailed in Appendix 3, and the D_{IRAP}-algorithm in Appendix 4 (and for details of the overall accuracy and latency characteristics underlying the D_{IAT} versus *trial-type* D_{IRAP} scores obtained here see Appendix 5).

4.3.4. Validity with Respect to Smoking-status

Known-Groups Differences

The IAT

A preliminary 2x2 *Analysis of Variance (ANOVA)* crossing smoking-status with trial block order on the D_{IAT} score yielded a main effect for smoking status, $F(1, 39) = 9.67$, $p = .004$, $\eta_p^2 = .20$, and also a main effect for trial block order such that those in the pro-smoking-first condition exhibited D_{IAT}s that were more pro-smoking/anti-ban and/or less anti-smoking/pro-ban, $p = .21$; $\eta_p^2 = .04$ (i.e. $r \approx .20$). However, there was no interaction effect on D_{IAT} between smoking-status and block order, $p = .78$; $\eta_p^2 = .002$.

The IRAP

The IRAP data were entered into a preliminary 2x2x2x2 mixed-repeated measures ANOVA, which crossed *smoking-status* (i.e. smokers versus non-smokers) and *trial block order* (i.e. pro-smoking-first versus anti-smoking-first) with the two repeated measures IRAP 'trial-type' variables, *Concept Label* ("Smoking makes Me Feel" versus "The Smoking Ban makes Me Feel") and *Attribute Stimulus Class* ("Positive Moods" versus "Negative Moods"; see Appendix 6 for a detailed rationale justifying the incorporation of these nested IRAP variables). Four main effects were obtained (ps $\leq .05$, η_p^2s $\geq .09$), but critically these were qualified by a large three-way interaction effect between smoking-status, concept label and attribute class, $F(1, 40) = 12.21$, $p = .001$, $\eta_p^2 = .23$. In broad terms, this indicates that all four *trial-type* D_{IRAP} scores functioned differently from each other in comparing smokers versus non-smokers. Crucially, this three-way interaction was not qualified by a four-way interaction with trial block order ($p = .71$, $\eta_p^2 = .001$), thus allowing us to interpret its constituent simple effects without reference to the trial block order variable. Nonetheless, there was a two-way interaction between smoking-status and trial block order, $F(1, 40) = 3.10$, $p = .09$, $\eta_p^2 = .07$. We focus first upon the interaction between smoking status and the two trial-type variables given that trial block order moderated smoking-status D_{IRAP} comparisons similarly across both trial-type variables.

The three-way trial-type D_{IRAP} *interaction among smoking-status, concept label and attribute stimulus class.* The three-way interaction is illustrated in Figure 4.3. In brief, the smokers showed two large pro-smoking effects, one moderately pro-smoking effect, and one moderately pro-smoking-ban effect. In contrast, the non-smokers showed only one non-zero IRAP effect and this was a large effect in the pro-smoking-ban direction. A series of follow-up tests were used to explore the nature of the interaction.

A mixed 2x2 *ANOVA* on the two smoking-related *trial-type* D_{IRAP}s yielded a large interaction between smoking-status and attribute stimulus class, $F(1, 42) = 8.55$, $p = .006$, $\eta_p^2 = .17$. Two follow-up independent measures *t*-tests confirmed that the smokers exhibited *Smoking-Pos* D_{IRAP}s that were more pro-smoking than the non-smokers, $t(42) = 3.92$, *one-tailed* $p = .0001$, $\eta^2 = .27$, but the groups did not differ in their *Smoking-Neg* D_{IRAP}s, $t(42) = .39$, $p = .34$, $\eta^2 = .004$. Two paired-samples *t*-tests confirmed that the smokers were more pro-smoking on the *Smoking-Pos* D_{IRAP} than on the *Smoking-Neg* D_{IRAP}, $t(21) = 3.71$, $p = .001$, $\eta^2 = .40$, but that the non-smokers did not differ in this regard, $t(21) = -.66$, $p = .52$, $\eta^2 = .02$.

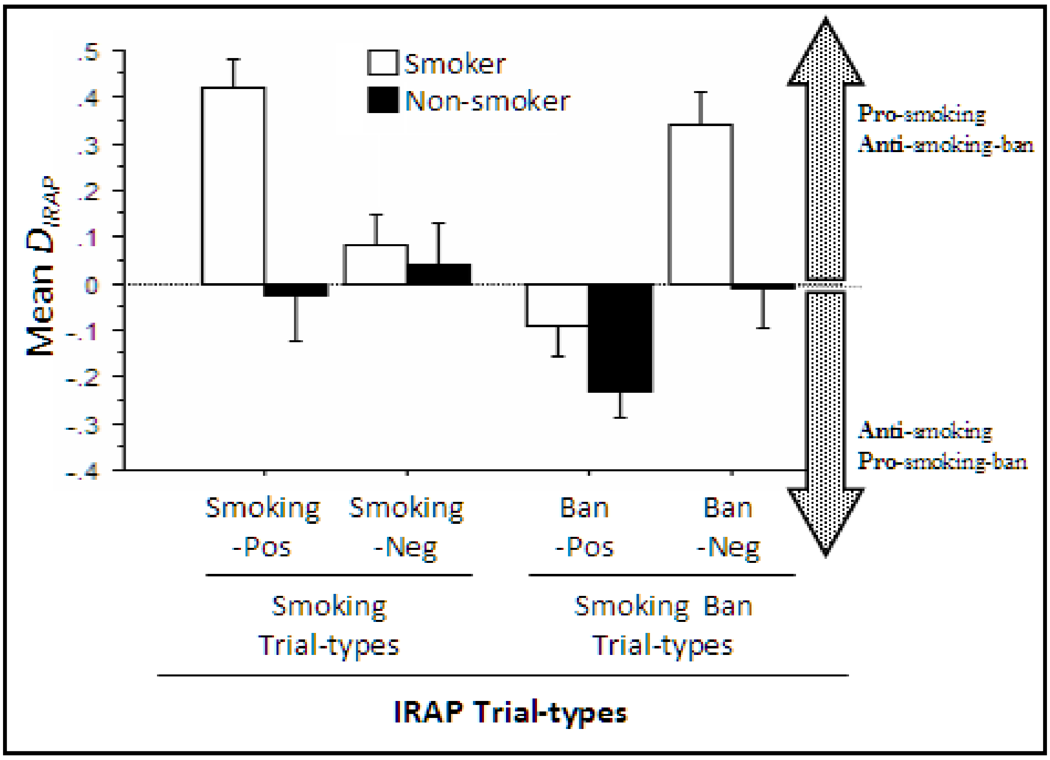

Figure 4.3. Mean *trial-type* D_{IRAP} scores, with standard error bars, for smokers versus non-smokers. The labels on the bar chart's horizontal axis show the attribute stimulus class variable (positive versus negative moods) nesting within the concept variable (the smoking label versus the smoking-ban label). A positive D_{IRAP} score for the 'Smoking-Pos' trial-type indicated faster *True* rather than *False* responses to label-target combinations like *Smoking makes Me Feel – Relaxed*. Similarly, positive *Smoking-Neg* D_{IRAP}s indicated faster *False* rather than *True* responses to label-target combinations like *Smoking makes Me Feel – Tense*. Likewise, positive *Ban-Pos* D_{IRAP}s or *Ban-Neg* D_{IRAP}s indicated faster anti-smoking-ban responses than pro-smoking-ban responses. By corollary, negative *trial-type* D_{IRAP}s indicated the opposite to their positive counterparts, and those in the neighbourhood of zero (e.g. $-.1 < D_{IRAP} < .1$) indicated that pro- versus anti-smoking(-ban) responses were produced with comparable fluency to each other.

A second 2x2 ANOVA on the smoking-ban *trial-type* D_{IRAP}s revealed large main effects for both smoking-status, $F(1, 42) = 9.05$, $p = .004$, $\eta_p^2 = .18$, and attribute stimulus class, $F(1, 42) = 30.77$, $p < .0001$, $\eta_p^2 = .42$; the interaction was moderately sized, $F(1, 42) = 3.21$, $p = .08$, $\eta^2 = .07$. All four follow-up *t*-tests were at least moderately-sized and in the expected directions, $1.61 \leq t(42)\text{s} \leq 6.07$, one-tailed *p*s ≤ .057, $.06 \leq \eta^2\text{s} \leq .64$,.

Eight one-sample *t*-tests confirmed that the smokers' *Smoking-Pos* D_{IRAP}s and *Ban-Neg* D_{IRAP}s were both very largely pro-smoking/anti-smoking-ban, $t(21)\text{s} \geq 4.66$, *one-tailed p*s < .0001, $\eta^2\text{s} \geq .51$; their *Smoking-Neg* D_{IRAP}s were moderately pro-smoking, $t(21) = 1.31$, *one-tailed* $p = .10$, $\eta^2 = .08$; and their *Ban-Pos* D_{IRAP}s were moderately pro-smoking-ban, $t(21) = 1.35$, *one-tailed* $p = .10$, $\eta^2 = .08$ (see Figure 4.3). In contrast, the non-smokers' *Ban-Pos* D_{IRAP}s were strongly pro-smoking-ban, $t(21) = -4.15$, one-tailed $p = .0001$, $\eta^2 = .45$, but their three remaining *trial-type* D_{IRAP}s did not differ from zero, $t(21)\text{s} \leq .51$, one-tailed *p*s ≥ .31, $\eta^2\text{s} \leq .01$.

The two-way interaction between smoking-status and trial block order. As illustrated in Figure 4.4, the order in which the smokers completed the IRAP blocks appeared to have little overall impact on their *trial-type* D_{IRAP}s. In contrast, trial block order had a large overall impact on non-smokers' IRAP responses: those who began the IRAP with an anti-smoking block exhibited a strong anti-smoking bias across the four *trial-type* D_{IRAP}s, but those who began with a pro-smoking block exhibited a weak pro-smoking bias overall.

Four follow-up between-groups ANOVAs were used to explore the nature of the interaction. The first pair of ANOVAs indicated that overall, the smokers were a large degree more pro-smoking than the non-smokers in the anti-smoking-first IRAP sequence, $F(1, 90) = 19.90$, $p < .0001$, $\eta^2 = .20$, but only to a small-to-moderate degree in the pro-smoking-first IRAP sequence, $F(1, 90) = 3.25$, $p = .07$, $\eta^2 = .04$. As per Figure 4.4, this discrepancy was almost entirely due to the impact of trial block order upon the non-smokers' *trial-type* D_{IRAP}s (i.e. rather than the smokers' *trial-type* D_{IRAP}s). And indeed, the second pair of ANOVAs confirmed this showing that there was no overall difference between the two block orders for the smokers, $F(1, 86) = .56$, $p = .46$, $\eta^2 = .01$), but a moderate difference for the non-smokers, $F(1, 86) = 9.43$, $p = .003$, $\eta^2 = .10$. Moreover, whereas the smokers implicitly evaluated in a similarly large pro-smoking manner overall in both block order conditions, $t\text{s} = 4.55$, $p\text{s} < .0001$, $\eta^2\text{s} = .65$, the non-smokers implicitly evaluated in opposite directions depending upon block order. Namely the non-smokers implicitly evaluated in a largely pro-smoking manner in the pro-smoking-first condition, $t(10) = 1.86$, $p = .09$, $\eta^2 = .26$, but in a largely anti-smoking manner in the anti-smoking-first condition, $t(10) = -2.24$, $p = .05$, $\eta^2 = .33$.

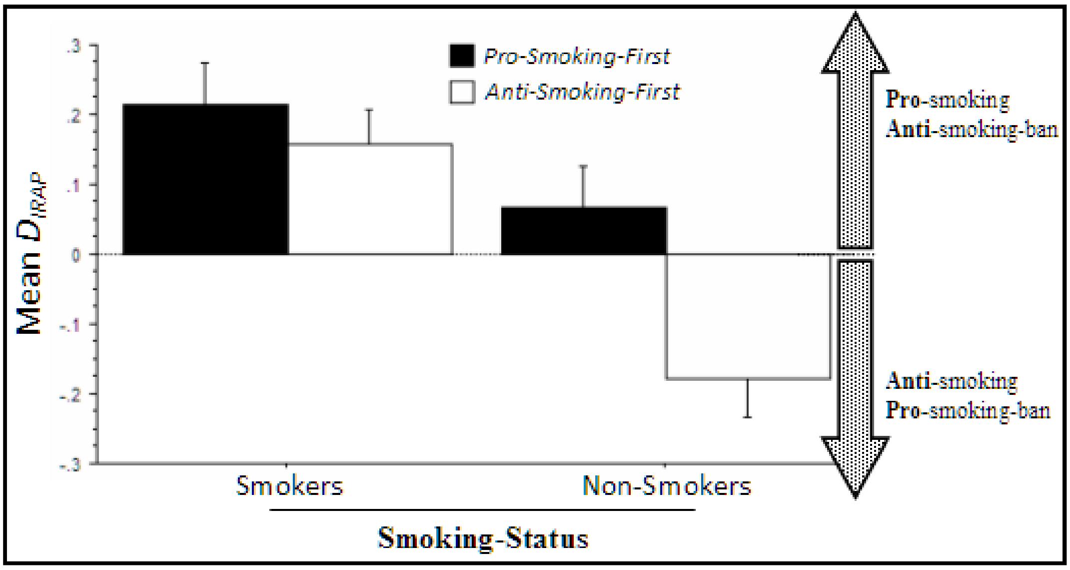

Figure 4.4. The interaction effect, with standard error bars, between smoking-status and trial block order across the four mean *trial-type* D_{IRAP}s aggregated as one overall D_{IRAP} score. Thus, positive overall D_{IRAP}s indicate typically faster responding on the pro-smoking/anti-smoking-ban relative to the corresponding anti-smoking/pro-smoking-ban response classes among the four trial-types; negative overall D_{IRAP}s indicate the opposite response pattern; and, finally, an overall D_{IRAP} in the neighbourhood of zero (e.g. $-.1 < D_{IRAP} < .1$) indicates that on average participants provided pro- versus anti-smoking(-ban) responses with similar fluency.

Known-Groups Sensitivity and Specificity: D_{IAT} versus the trial-type D_{IRAP}s

The Receiver Operating Characteristic (*ROC*; Fawcett, 2006; IBM Corp., 2012, pp. 839-844; Rice & Harris, 2005) was used with the *Area Under the Curve (AUC)* statistic to compare the D_{IAT} versus the various *trial-type* D_{IRAP}s in terms of their specificity and sensitivity in classifying participants as smokers versus non-smokers (see Appendix 7 for a more detailed technical explanation of *ROC* and *AUC*).

The Sensitivity and Specificity of the D_{IAT} for Diagnosing Smoking-status

The D_{IAT} smoking-status *ROC* curve achieved a moderately sized smoking-status $AUC = 0.74$, $p < .01$, $d = .63$, $r \approx .30$ (see Figure 4.5).[40] This means that if a smoker and a non-smoker were chosen at random from the present sample, then 74% of the time the smoker would have a higher D_{IAT} than the non-smoker (i.e. as compared to 50% of the time by chance). In addition, in Figure 4.6 we improvised the first direct means of analysing the full range of trade-offs between the *sensitivity* versus *specificity* of any diagnostic technique, *the sensitivity versus specificity plot*. This plot revealed that the D_{IAT} cut-off which achieved the highest combined smoking-status sensitivity and

[40] *Cohen's d* calculated using Lowry's (2012) software implementation of Hanley and McNeil's (1982) *Z*-test algorithm for comparing the significance of the difference between the areas under two independent ROC curves.

specificity within the current sample was $D_{IAT} = .13$; it correctly classified smokers 68% of the time (i.e. sensitivity) and non-smokers 81% of the time (i.e. specificity).

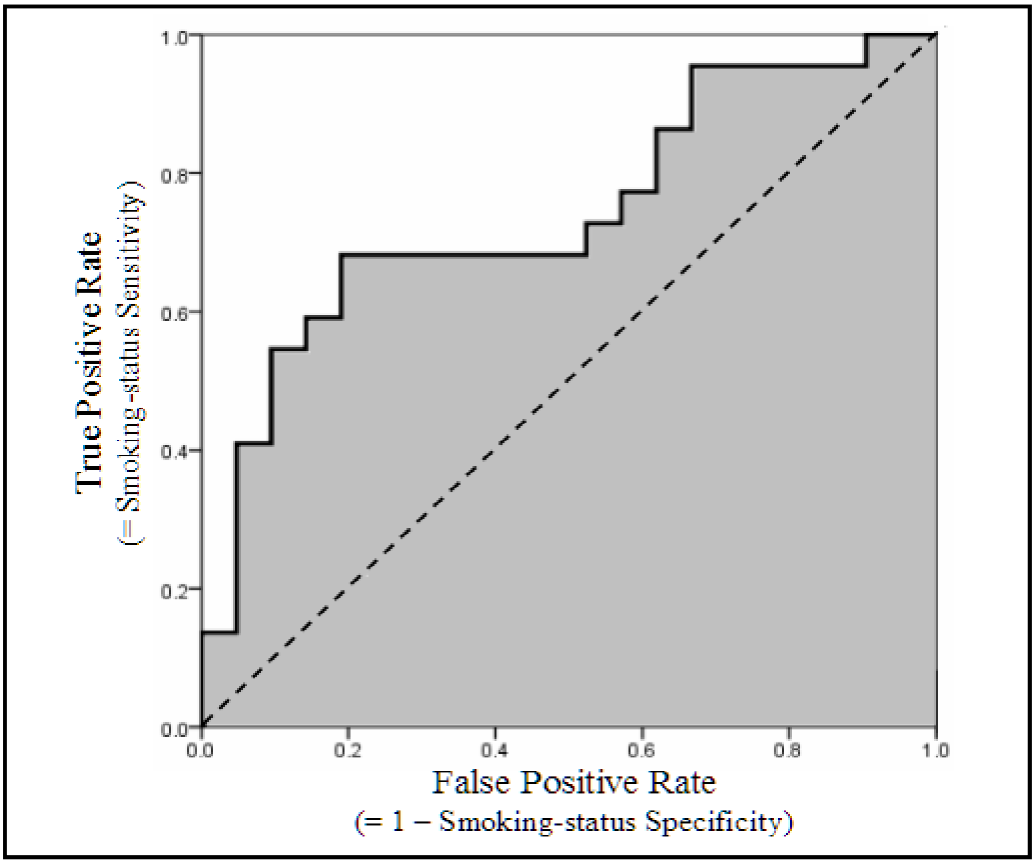

Figure 4.5. The D_{IAT} smoking-status ROC curve with a shaded $AUC = .74$, $p < .01$. The dashed diagonal line corresponds to the ROC curve of measures that merely discriminate smoking-status by chance (i.e. *AUC*s = .5, *p*s = 1.0). The plateau in D_{IAT} sensitivity in the middle of the *ROC* curve indicates that the trade-off between sensitivity versus specificity was not uniform throughout the observed range of D_{IAT}s (i.e. $-.89 < D_{IAT} < +.97$).

Travelling across the neighbourhood of zero in Figure 4.6 from the optimal cut-off at $D_{IAT} = .13$, to $D_{IAT} = -.23$, the observed D_{IAT}s plateau in sensitivity while simultaneously plummeting in specificity from .81 to .47 (i.e. the plateau is also evident in Figure 4.5 between false positive rates .2-.5 approximately). This means that the non-smokers' D_{IAT}s were distributed continuously across the neighbourhood of $D_{IAT} = 0$, but that the smokers scored on either side of the region of $D_{IAT} = 0$ but not within it. In effect, smokers responded in a polarized, bimodal fashion to the IAT such that they tended to either strongly favour pro-smoking/anti-smoking-ban IAT trials over anti-smoking/pro-smoking-ban IAT trials, or vice versa (see Appendix 8 for detailed statistical analyses which corroborate the dramatic nature of this discontinuity in smokers' D_{IAT}s).

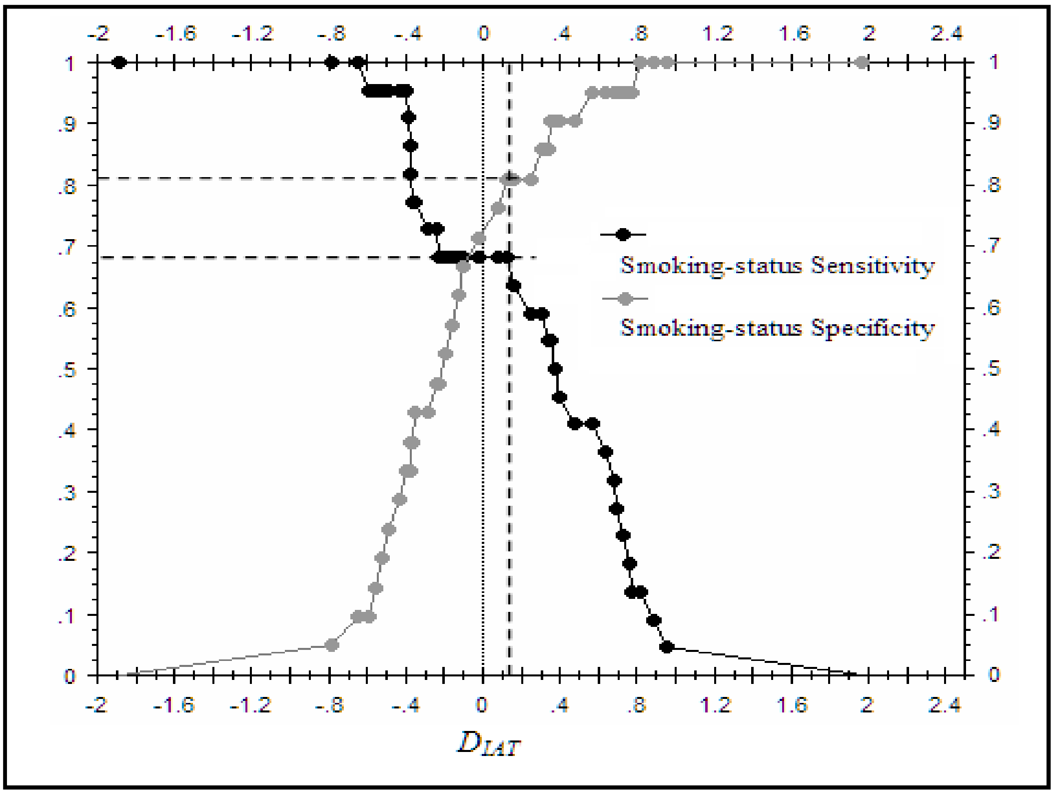

Figure 4.6. The *sensitivity versus specificity plot* of D_{IAT} with respect to identifying smokers at each of its ROC curve coordinates. With lower values of D_{IAT} these cut-offs come to include a higher proportion of the smoker sample's D_{IAT}s (i.e. with sensitivity indicating the exact proportion); but conversely, the higher the value of these cut-offs the higher the proportion they will include of the non-smoker sample's D_{IAT}s (i.e. with specificity indicating the exact proportion). The intersection between the dashed vertical line and the two dashed horizontal lines, at D_{IAT} = .13, represents the point at which D_{IAT} has maximal combined sensitivity and specificity for identifying smokers from the present sample.

The Sensitivity and Specificity of the trial-type D_{IRAP}s for Diagnosing Smoking-status

The smoking trial-type D_{IRAP}s. The *Smoking-Pos D_{IRAP}* was diagnostic of smoking-status to a moderate-to-large degree with an $AUC = 0.79$, $p < .001$, $d = .78$, $r \approx .36$. In contrast, the *Smoking-Neg D_{IRAP}* was not diagnostic of smoking-status, $AUC = .52$, $p = .79$, $d = .05$, $r \approx .02$ (see Figure 4.7). The *Smoking-Pos D_{IRAP}* achieved its optimal combined sensitivity and specificity for identifying smokers at a threshold of *Smoking-Pos D_{IRAP}* = .196; it correctly classified 91% of smokers and 62% of non-smokers in the current sample (for relevant plots see Appendix 9). The regular shape of the *Smoking-Pos D_{IRAP} ROC curve* indicates that unlike the D_{IAT} the trade-off between its smoking-status sensitivity versus specificity was approximately uniform throughout the range of *Smoking-Pos D_{IRAP}*s obtained (i.e. -.86 < *Smoking-Pos D_{IRAP}* < +1.13).

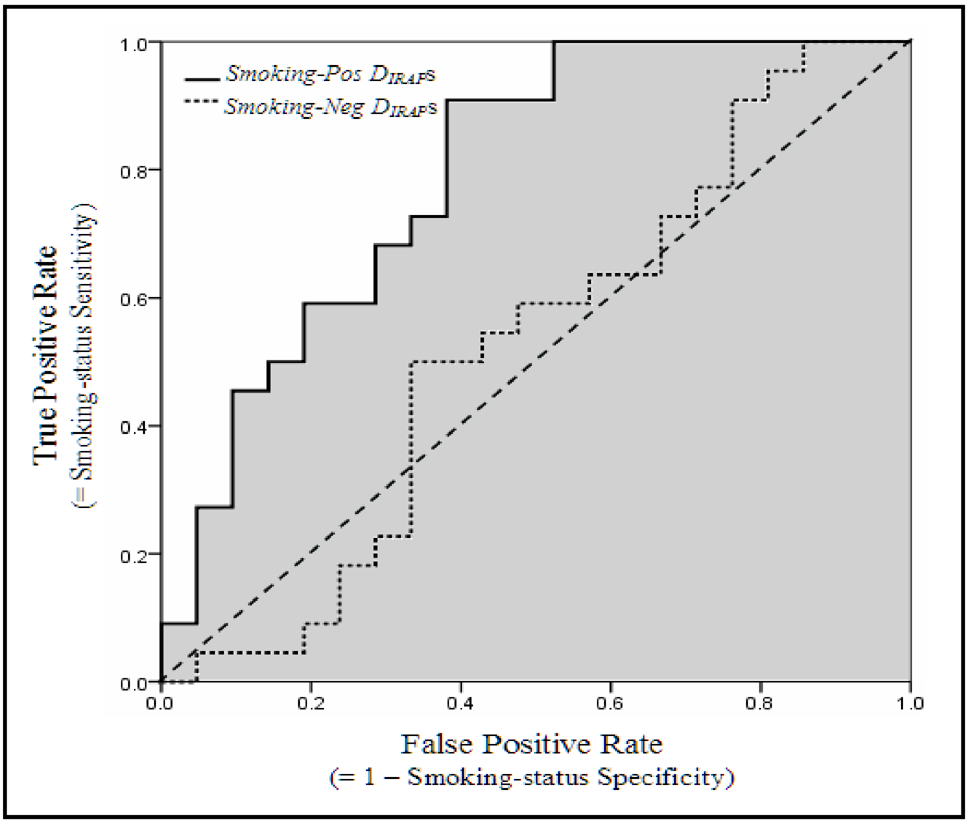

Figure 4.7. The smoking-status *ROC* curves for the two 'smoking' *trial-type* D_{IRAP}s: *Smoking-Pos* D_{IRAP}, *AUC* = .79, *p* < .001; *Smoking-Neg* D_{IRAP}, *AUC* = .52, *p* = .79. Note that the ROC curve for *Smoking-Neg* D_{IRAP} was in close correspondence with the dashed diagonal line representing chance levels of smoking-status diagnosis (i.e. a ROC curve with an *AUC* = .5). The uniformly graduated shape of the *Smoking-Pos* D_{IRAP} *ROC curve* indicates that unlike the D_{IAT} the trade-off between its smoking-status sensitivity versus specificity was approximately uniform throughout the range of *Smoking-Pos* D_{IRAP}s obtained (i.e. -.86 < *Smoking-Pos* D_{IRAP} < +1.13).

The smoking-ban trial-type D_{IRAP}*s*. *Ban-Neg* D_{IRAP} had a moderately sized smoking-status *AUC* = .75, *p* = .006, *d* = .65, *r* ≈ .30, but *Ban-Pos* D_{IRAP} was not diagnostic of smoking-status, *AUC* = 0.63, *p* = .14, *d* = .32, *r* ≈ .16 (see Figure 4.8). *Ban-Neg* D_{IRAP} achieved its optimal combined smoking-status sensitivity and specificity at *Ban-Neg* D_{IRAP} = .11 whereupon it correctly classified 77% of smokers and 62% of non-smokers in the current sample (for relevant plots see Appendix 9). Again, the uniformly graduated shape of the *Ban-Neg* D_{IRAP} *ROC curve* indicated that unlike the D_{IAT} the trade-off between sensitivity versus specificity for smoking-status was approximately uniform throughout the range of *Ban-Neg* D_{IRAP}s obtained (i.e. -.68 < *Ban-Neg* D_{IRAP} < +1.28). Otherwise, *Z*-tests indicated that the *AUC* for D_{IAT} was nearly identical to the *AUC*s for both *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP}, |*Z*s| = .09, .49; *p*s = .92, .63; *d*s = .03, .15; *r*s ≈ .01, .07.

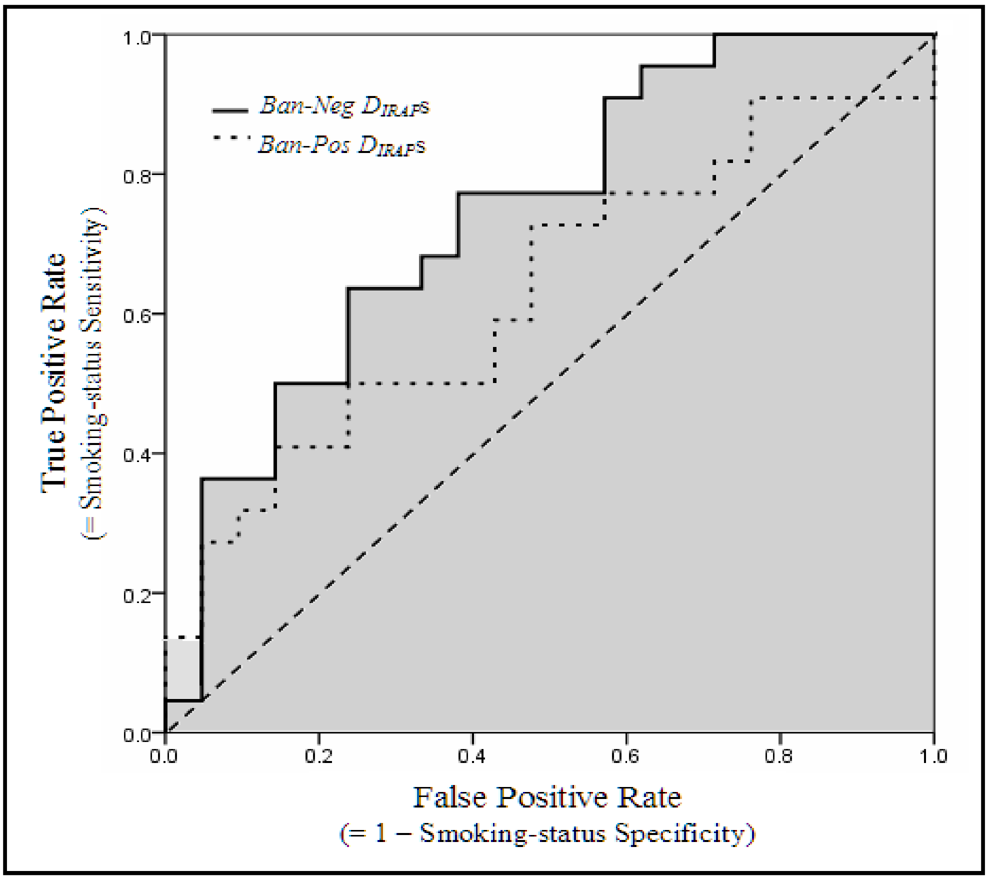

Figure 4.8. The smoking-status *ROC curves* of the smoking-ban *trial-type* D_{IRAP}s: *Ban-Pos* D_{IRAP}, $AUC = .63$, $p = .14$; *Ban-Neg* D_{IRAP}, $AUC = .75$, $p = .006$. The uniformly graduated shape of the *Ban-Neg* D_{IRAP} *ROC curve* indicates that unlike the D_{IAT} its trade-off between sensitivity versus specificity for smoking-status was approximately uniform throughout the range of *Ban-Neg* D_{IRAP}s obtained (i.e. $-.68 <$ *Ban-Neg* $D_{IRAP} < +1.28$).

4.3.5. Validity in Relation to Five Tobacco Addiction Criteria: D_{IAT} versus the *trial-type* D_{IRAP}s

Preliminary Correlation Analyses

The five tobacco addiction criteria all correlated with each other to a remarkably high degree in both the IAT group and the IRAP group (see Table 4.4; i.e. IAT group, $.41 < r\text{s} < .94$; all $p\text{s} < .0001$; IRAP group, $.68 < r\text{s} < .91$; all $p\text{s} < .0001$).[41] In addition, D_{IAT} correlated to a moderate degree with all five of these tobacco dependence criteria, $.22 \leq r\text{s} \leq .40$ (i.e. even if only the correlation with CPD was significant at a Bonferroni corrected alpha of $p \leq .01$; see Table 4.4). And indeed, *Smoking-Pos* D_{IRAP} appeared to correlate even more strongly than D_{IAT} in terms of its correlations with all five tobacco addiction criteria listed in Table 4.4, $.36 \leq r\text{s} \leq .55$, $p\text{s} \leq .01$ (all five correlations were significant at the appropriate Bonferroni-corrected $\alpha = .01$). Likewise, *Ban-Neg* D_{IRAP} tended to exhibit large correlations with the three criteria addressing trait tobacco

[41] Confirming that the IAT group versus IRAP group were well matched, a family of correlation *Z*-tests did not indicate any consistent between-group difference across the 10 correlations among YS, CPD, mFTQ, HONC and current tobacco cravings ($.94 < |Z\text{s}| < 1.34$, $.18 < p\text{s} < .35$, $r\text{s} < .20$).

addiction intensity, $.39 \le r$s $\le .47$, $.0008 \le$ one-tailed ps $\le .005$ (i.e. CPD, mFTQ and HONC); even if it only correlated moderately with years smoking (YS), $r = .33$, and with TC, $r = .27$. *Ban-Pos* D_{IRAP} did not correlate with YS, $r = .16$, or HONC, $r = .18$, but did correlate to a moderate degree with CPD, mFTQ and TC, $.28 \le r$s $\le .39$. In contrast, *Smoking-Neg* D_{IRAP} did not correlate with any of the five tobacco addiction criteria, rs $\le .14$.

Table 4.4
A correlation matrix for the IAT group data, and another for the IRAP group data, with each incorporating the same five tobacco addiction criteria: Years Smoking (YS), CPD, mFTQ, HONC and tobacco cravings.

	The Four Trial-type D_{IRAP}s & The Five Tobacco Addiction Criteria				
	YS	*CPD*	*mFTQ*	*HONC*	*Tobacco Cravings*
IAT Group (*n* = 43)					
D_{IAT}	**.22**	**.40****	**.30***	**.31***	**.30***
YS	--	.77****	.69****	.71****	.41**
CPD		--	.94****	.85****	.77****
mFTQ			--	.84****	.65****
HONC				--	.66****
IRAP Group (*n* = 44)					
Smoking-Pos D_{IRAP}	**.54******	**.50*****	**.50*****	**.55******	**.36****
Smoking-Neg D_{IRAP}	**.08**	**.08**	**-.11**	**.01**	**.14**
Ban-Pos D_{IRAP}	**.16**	**.28***	**.30***	**.18**	**.39****
Ban-Neg D_{IRAP}	**.33***	**.43*****	**.47*****	**.39****	**.27***
YS	--	.72****	.66****	.83****	.71****
CPD		--	.91****	.82****	.81****
mFTQ			--	.88****	.72****
HONC				--	.72****

[#]$p \le .1$, *$p \le .05$, **$p \le .01$, *** $p \le .001$, **** $p \le .0001$ (all ps one-tailed). *Note.* All ps are uncorrected, but note Bonferroni corrected $p < .01$ should be applied to each quintuple of correlations between the various *D*s and the five tobacco dependence criteria because each constitutes a different domain of variability (Howell, 2012a).

Models of D_{IAT} and the trial-type D_{IRAP}s in terms of the Tobacco Addiction Criteria

We used the Hayes (2012) '*PROCESS*' software for bootstrapped casual path analysis to compare the extent to which these implicit measures might have co-developed with CPD, mFTQ or HONC across YS. That is, CPD, mFTQ and HONC were modelled separately as candidate mediators between YS and the various *D*s.[42]

[42] CPD, mFTQ and HONC were tested *separately* as candidate mediators between YS and each of the *D*s because of the high degree of collinearity and conceptual reciprocity among them (i.e. both here and in

Crucially, we did this in order to model the extent to which D_{IAT}s, versus the four *trial-type* D_{IRAP}s, might each be casually involved in tobacco addiction.

Modelling D_{IAT}

We found that that both CPD and mFTQ were dominant mediators between YS and D_{IAT}, κ^2s = .34, .20, *p*s ≤ .05; as was HONC to a lesser extent, κ^2s = .13, $p < .05$ (i.e. i.e. $\kappa^2 = .34$ means that path *ab* was 34% of the maximum mediation effect possible given the observed patterns of variation between YS and D_{IAT}; for relevant model tables and path diagrams see Appendix 10). However, given that YS accounted for only 5% of the variance in D_{IAT}, $R^2 = .05$, $p = .17$, this implies that CPD mediated a null 1.7% of D_{IAT} at most; mFTQ at most 0.6%; and HONC at most 0.2% (i.e. obtained by multiplying $R^2 = .05$ by the corresponding κ^2 in each case).[43] In contrast, even if HONC only explained a null 2.8% of D_{IAT} when controlling for YS, mFTQ and CPD respectively explained 10% and 15% of D_{IAT} when controlling for YS, *F*s = 4.35, 7.08, *p*s = .04, .01. This implies that 15% and 10% of D_{IAT} respectively resulted from, rather than in, CPD and mFTQ; but that only a null 2.8% appeared to result from HONC.[44]

Modelling the trial-type D_{IRAP}s

The relationship between YS and *Smoking-Pos* D_{IRAP} appeared to be dominantly mediated by all three tobacco addiction intensity traits (i.e. κ^2s = .22, .17, .30; *p*s ≤ .05; for CPD, mFTQ and HONC; see Appendix 10). Moreover, the relationship between YS and *Smoking-Pos* D_{IRAP} was large, $F(1, 41) = 10.23$, $R^2 = .20$. Thus, for example, approximately (κ^2 =) 30% of the 20% shared variance between YS and *Smoking-Pos* D_{IRAP} could, in principle, have developed in tandem with HONC; which approximately amounts to a moderate 6% of *Smoking-Pos* D_{IRAP} overall (or an $r \approx .25$). And likewise, a moderate-to-small 4.4% and 3.4% of *Smoking-Pos* D_{IRAP} (or *r*s ≈ .21, .18) could in principle have developed in tandem with CPD and mFTQ, respectively. Our analysis

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the literature they constitute indeterminately overlapping aspects of the trait intensity of smoking compulsions; see DiFranza, Ursprung, & Biller, 2012; O'Loughlin et al., 2002, *p*. 359; Wellman et al., 2006).

[43] According to Preacher and Kelley (2011, *pp*. 107-108) κ^2 should be judged according to the same size conventions as Cohen's (1988) standards for coefficients of determination. They recently introduced the κ^2 statistic as the first effect-size statistic capable of characterizing the size of indirect (e.g. mediation) effects in a generally interpretable manner (i.e. a standardized, bounded, efficient and unbiased estimator of effect size). Specifically, κ^2 quantifies the size of the observed indirect effect as a proportion of its maximum size possible with the observed profile of shared variances among the various modelled variables involved.

[44] In principle, the additional variance in *Enjoy-Pos* D_{IRAP} respectively explained by the three criteria in addition to YS may have reflected some recent causal influence of implicit evaluating upon tobacco addiction (i.e. within the previous year). However, our participants were specifically sampled for stable patterns of smoking during the preceding year, and therefore it seems likely that the majority of the relevant partial correlations instead reflected the causal influence of tobacco addiction (as per CPD, mFTQ or HONC) upon D_{IAT}.

also indicated that CPD, mFTQ and HONC all accounted for *Smoking-Pos* D_{IRAP} even while controlling for YS, respective R^2*-changes* = .09, .05, .10, $F(1, 41)$s = 5.30, 2.91, 6.08, *p*s = .03, .10, .02. We therefore estimated that up to 9%, 5% and 10% of *Smoking-Pos* D_{IRAP} respectively resulted from, rather than in, CPD, mFTQ and HONC.

Smoking-Neg D_{IRAP} did not correlate with YS, CPD, mFTQ or HONC. We therefore deemed it pointless to model *Smoking-Neg* D_{IRAP} with respect to any of these four aspects of tobacco addiction. Similarly, *Ban-Pos* D_{IRAP} did not qualify for meditational analysis on the grounds that it did not correlate with YS or HONC, *r*s = .16, .18. Granted, *Ban-Pos* D_{IRAP} did correlate with both CPD and mFTQ, *r*s = .28, .30. However, given that YS did not account for any variance in *Ban-Pos* D_{IRAP} in addition to CPD or mFTQ, R^2*-changes* = -.05, .002, $F(1, 41)$s = .01, .10, *p*s = .91, .76, it ruled out either CPD or mFTQ as mediators between YS and *Ban-Pos* D_{IRAP} (i.e. otherwise, CPD or mFTQ may have acted as *suppressor mediators* between YS and *Ban-Pos* D_{IRAP} such that higher YS scores would yield lower *Ban-Pos* D_{IRAP}s while controlling for CPD or mFTQ). In contrast, mFTQ explained approximately 6% of *Ban-Pos* D_{IRAP} when controlling for YS, R^2*-change* =.06, $F(1, 41)$ = 3.01, *p*s =.09; even if CPD and HONC did not appear to influence *Ban-Pos* D_{IRAP} independently of YS, R^2*-changes* = .02,.03, *F*s = .70, .21, *p*s = .41,.65. Overall, therefore, we estimated that up to 6% of *Ban-Pos* D_{IRAP} respectively resulted from, rather than in, mFTQ; but that it was unrelated either way to CPD or HONC.

The relationship between YS and *Ban-Neg* D_{IRAP} appeared to be dominantly mediated by CPD, mFTQ and HONC, respectively, κ^2s = .19, .19, .19 (for relevant model tables and path diagrams see Appendix 10). However, given that the relationship between YS and *Ban-Neg* D_{IRAP} was only moderate to begin with, $F(1, 42) = 5.23$, R^2 = .11, $p = .03$, therefore only a null 2% of *Ban-Neg* D_{IRAP} could have developed in tandem with CPD, mFTQ or HONC across YS (i.e. $r = .14$). In contrast, 5-8% of *Ban-Neg* D_{IRAP} was respectively explained by each of the three smoking-compulsion intensity traits while controlling for YS, R^2*-changes* = .08, .07, .05, $F(1, 41)$s = 3.92, 3.52, 2.34, *p*s = .05, .07, .13. Therefore, we estimated that up to 8%, 7% and 5% of *Ban-Neg* D_{IRAP} respectively resulted from, rather than in, CPD, mFTQ and HONC.

4.3.6. Validity with Respect to Explicit Evaluations: D_{IAT} versus *trial-type* D_{IRAP}*s*

All four explicit measures were rescored so that the polarity of their scores concurred with those of the various D_{IRAP}s (i.e. positive ≡ pro-smoking, and negative ≡ anti-smoking), and so that zero ratings purported to be neutral. The resulting smoking and smoking-ban related semantic differentials both had extremely high internal reliability with *Cronbach alphas* of $\alpha = .96$ and $\alpha = .94$, respectively.

Table 4.5

The means (M) and standard deviations (SD) of the two semantic differential scores, and the two feeling thermometer scores, for the IAT group versus the IRAP group split by smoking-status.

	Semantic Differentials		**Feeling Thermometers**	
	Smoking	*Smoking-Ban*	*Smoking*	*Smoking-Ban*
IAT Group				
Smoker	*M* = 1.43, *SD* = 1.11	*M* = -.32, *SD* = 1.26	*M* = 11.05, *SD* = 27.07	*M* = -15.59, *SD* = 27.31
Non-smoker	*M* = -1.97, *SD* = .86	*M* = -2.5, *SD* = .85	*M* = -39.23, *SD* = 14.53	*M* = -44.96, *SD* = 10.65
IRAP Group				
Smoker	*M* = 1.21, *SD* = 1.19	*M* = -.47, *SD* = 1.40	*M* = 24.77, *SD* = 16.98	*M* = -7.59, *SD* = 28.08
Non-smoker	*M* = -1.51, *SD* = .99	*M* = -1.90, *SD* = 1.10	*M* = -32.33, *SD* = 19.34	*M* = -35.48, *SD* = 18.27

Note. Thermometer scores ranged from -50 to +50, and semantic differential scores ranged from -3 to +3. Positive scores are pro-smoking, negative scores are anti-smoking, and neutral scores purport neutrality with respect to smoking and/or the smoking-ban.

Table 4.6

Correlations of D_{IAT} and the trial-type D_{IRAP}s each with the four explicit evaluation scores.

	Semantic Differentials		**Feeling Thermometers**	
	Smoking	*Smoking-Ban*	*Smoking*	*Smoking-Ban*
IAT Group				
D_{IAT}	.43**	.34**	.42**	.26*
IRAP Group				
Smoking-Pos D_{IRAP}	.43**	.54****	.41**	.44***
Smoking-Neg D_{IRAP}	.24#	.14	.09	.23#
Ban-Pos D_{IRAP}	.24#	.21#	.24#	.23#
Ban-Neg D_{IRAP}	.30*	.27*	.24#	.07

$p \le .10$, * $p \le .05$, ** $p \le .01$, *** $p \le .001$, **** $p \le .0001$, with all *p*s one-tailed.

Overall, as detailed by Table 4.5, smokers explicitly evaluated smoking as moderately favourable, $t(43)$s ≥ 5.08, one-tailed *p*s ≤ .0001, $\eta^2 \ge .38$, and to a lesser extent also explicitly evaluated the Irish smoking-ban as moderately favourable, $t(43)$s ≤ -1.99, one-tailed *p*s ≤ .03, $\eta^2 \ge .04$.[45] In contrast, the non-smokers explicitly evaluated both smoking very unfavourably, $t(42)$s ≤ -12.11, one-tailed *p*s ≤ .0001, $\eta^2 \ge .78$, and the Smoking-ban very favourably, $t(42)$s ≤ -14.24, one-tailed *p*s ≤ .0001, $\eta^2 \ge .83$.[46] As

[45] Note that here we interpreted the degree of (dis)favour in relation to the respective Likert-type scales rather than in relation to the size of the relevant statistical effect.

[46] Note, as detailed in Table 5, that the IAT group matched the IRAP group closely on all four varieties of explicit evaluating scores, $|t(85)s| \le 1.60$, *p*s ≥ .11, $\eta^2_s \le .03$; even when split according to smoking-status,

per Table 4.6, D_{IAT} correlated strongly with explicit evaluating of smoking, and perhaps somewhat less so with ban-related explicit evaluating. In contrast, *Smoking-Pos* D_{IRAP} correlated strongly with both explicit evaluating of smoking and of the Smoking-ban, but the remaining three *trial-type* D_{IRAP}s did so only to a moderate-to-small degree if at all.[47]

4.3.7. Validity in Relation to Internal Reliability: D_{IAT} versus the *trial-type* D_{IRAP}s

The *Spearman-Brown corrected* split-half correlations (r_{sb}) for each group's IAT effect was computed by correlating the D_{IAT}s produced by odd versus even IAT data trials. Similarly the r_{sb} for a given *trial-type* D_{IRAP} was computed by correlating the *D* scores produced by odd versus even data trials from that IRAP trial-type. Whereas D_{IAT} r_{sb} was impressively high regardless of smoking-status, r_{sb}s ≥ .80, one-tailed *p*s < .0001, in contrast the internal consistency of *trial-type* D_{IRAP}s varied dramatically with both smoking-status and trial-type (see Table 4.7).

Table 4.7

Spearman-Brown split-half reliabilities (r_{sb}) for smokers' versus non-smokers' various Ds. 'Compensated D_{IRAP} r_{sb}s' approximated what r_{sb}s the various D_{IRAP}s would have if, with all else equal, they were comprised of the same number of trials as a D_{IAT} (see Appendix 11 for relevant algorithm).

	D_{IRAP} r_{sb}	*Z*-test of D_{IRAP} r_{sb} with	*Compensated* D_{IRAP} r_{sb}	*Z*-test of *Compensated* D_{IRAP} r_{sb} with
Smokers				
		D_{IAT} r_{sb} (= .93****)^a		D_{IAT} r_{sb} (= .93****)^a
Smoking-Pos D_{IRAP} ^{b}	.68****	*Z* = 2.52**	.90****	*Z* = .57
Smoking-Neg D_{IRAP} ^{a}	.03	*Z* = 5.02****	.06	*Z* = 4.93****
Ban-Pos D_{IRAP} ^{c}	.08	*Z* = 4.73****	.26	*Z* = 4.17****
Ban-Neg D_{IRAP} ^{c}	.01	*Z* = 4.94****	.05	*Z* = 4.82****
Non-Smokers				
		D_{IAT} r_{sb} (= .80****)^b		D_{IAT} r_{sb} (= .80****)^b
Smoking-Pos D_{IRAP} ^{c}	.67****	*Z* = .85	.89****	*Z* = -.96
Smoking-Neg D_{IRAP} ^{b}	.63***	*Z* = 1.07	.87****	*Z* = -.70
Ban-Pos D_{IRAP} ^{c}	.69****	*Z* = .74	.90****	*Z* = -1.11
Ban-Neg D_{IRAP} ^{c}	.10	*Z* = 2.95**	.32$^{\#}$	*Z* = 2.27*

^{a} *n* = 22; ^{b} *n* = 21; ^{c} *n* = 20. $^{\#}$ *p* ≤ .10, * *p* ≤ .05; ** *p* ≤ .01; *** *p* ≤ .001; **** *p* ≤ .0001; all *p*s for r_{sb} one-tailed but all *p*s two-tailed for correlation *Z*-tests.

And yet, of the four group *trial-type* D_{IRAP}s which exhibited good internal consistency only the smokers' *Smoking-Pos* D_{IRAP}s and the non-smokers' *Smoking-Neg*

|*t*s| ≤ 2.09, *p*s ≥ .04, η^2_s ≤ .05 (i.e. noting that a Bonferroni-type correction should apply to these effect-sizes).

[47] On balance, we interpreted the few, sporadic null correlations between the trial-type D_{IRAP}s and the explicit evaluating measures as being instances of *type II* family-wise error.

D_{IRAP}s had lower internal reliabilities than D_{IAT}, respectively, ps = .01, .28; Zs = 2.52, 1.07; rs ≈ .55, .23. Moreover, when we compensated *trial-type* D_{IRAP} internal reliability for the four times as many trials which comprised the current D_{IAT}s all such differences disappeared (e.g. *mean compensated internally consistent trial-type* D_{IRAP} r_{sb} = .89 versus *mean* D_{IAT} r_{sb} = .87; for details of the compensation algorithm see Appendix 11). And reassuringly, the compensation algorithm did not spuriously grant internal consistency to any *trial-type* D_{IRAP}s that had not been internally consistent to begin with.

4.4. DISCUSSION

The current IAT design succeeded in providing an uncommonly high-quality benchmark against which to compare the criterion validity of any smoking-related IRAP. It outperformed its predecessors in terms of correlating with *all* of the cross-sectional tobacco addiction criteria that we employed. Moreover, in all cases, the size of the relevant criterion correlations were either unprecedented, or else at the upper end of those previously achieved in the domain. It is therefore impressive, that the smoking-related IRAP we used performed consistently better in terms of the known-groups co-variations it achieved with all relevant tobacco addiction criteria. Indeed, as we will now review, the IRAP added something crucial to our analysis that was lacking from the IAT data; namely, the ability to measure different types of implicit evaluating individually.

Testing Implicit Measures for Relationships with Tobacco Addiction Criteria

Known-groups Contrasts of Implicit Evaluating

Whereas the IAT yielded only one known-groups smoking-status difference that did not specify what particular types of implicit evaluating were involved, the IRAP yielded four known-groups smoking-status effects which did. Indeed, collectively, the four IRAP trial-type effects were differentially impacted by smoking-status specifically in line with known-groups predictions about the particular types of implicit evaluating each effect measured. Namely, the IRAP confirmed that smokers were implicitly much more pro-smoking than non-smokers when addressing smokers' usual reasons for smoking as per *Smoking-Pos* D_{IRAP}; or when addressing the nicotine withdrawal symptoms that smokers typically experience due to smoking-bans as per *Ban-Neg* D_{IRAP}. By contrast, when the IRAP addressed positive feelings toward the Irish Smoking-ban as per *Ban-Pos* D_{IRAP}, the smokers were only moderately less pro-ban than the non-smokers (i.e. in line with Irish smokers' immediate and near universal adherence to the Irish Smoking-ban despite the aforementioned inconveniences it posed for them; see Clancy, 2007; Fahy et al., 2012; Lonergan, 2013). And indeed, when the IRAP addressed the current smokers' characteristically ambivalent misgivings toward smoking as per *Smoking-Neg* D_{IRAP}, there was no smoking-status effect at all. Accordingly, the *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} both diagnosed smoking-status with similar sensitivity and specificity as D_{IAT}, but neither *Smoking-Neg* D_{IRAP} nor *Ban-Pos* D_{IRAP} was diagnostic of smoking-status. At the very least, the IRAP thus highlighted the need not only to measure implicit evaluating of different attitude-objects separately, but also to measure positively-framed smoking-related implicit evaluating separately from corresponding negatively-framed implicit evaluating.

Known-groups Conflicts between Implicit versus Explicit Smoking-related Evaluating

The IRAP detected one known-groups conflict and seven known-groups agreements between corresponding types of implicit and explicit evaluating. Most notably, smokers' pro-ban explicit evaluations were at odds with their strongly anti-ban *Ban-Neg* D_{IRAP} effects (i.e. as per widespread social pressure for pro-ban explicit evaluating; see Introduction section). Indeed, this was despite the fact that the smokers evaluated smoking favourably on both explicit measures of smoking, and also on both *Smoking-trial-type* D_{IRAP}s. In contrast, as expected, the non-smokers exhibited no evaluative conflicts between the four *trial-type* D_{IRAP}s and corresponding measures of explicit evaluating. Crucially, the IAT was unable to detect any such conflicts or agreements because it could not distinguish particular types of implicit evaluating.

Known-groups Correlations with Tobacco Addiction Criteria

Even though D_{IAT} correlated moderately with all five continuous tobacco addiction criteria we measured (i.e. YS, CPD, mFTQ, HONC and TC) these correlations did not provide any information about the types of implicit evaluating involved. In contrast, the four *trial-type* D_{IRAP}s yielded a differential pattern of correlations with these five criteria that comported well with a priori known-groups expectations about the particular types of implicit evaluating each IRAP effect measured. For instance, *Smoking-Pos* D_{IRAP} correlated to a large degree with all five tobacco addiction criteria – and this supported the literature's foundational assumption that the more addicted a person is the more they will implicitly affirm that smoking is emotionally satisfying (e.g. Tibboel et al., 2011). In contrast, the *Smoking-Neg* D_{IRAP} did not correlate with any of the five criteria – and there was no a priori basis for predicting that it should have. To the contrary, our known-groups sampling specified that it was irrelevant to the current smokers' ongoing smoking behaviour for them to focus upon whether or not smoking makes them feel bad (i.e. insofar as none of them had planned to restrict their smoking during the previous 12 months). Indeed, from this point of view, the current smokers were unlikely to have frequently engaged in relating the perceived benefits and drawbacks of smoking directly to each other – and confirming this, *Smoking-Pos* D_{IRAP} and *Smoking-Neg* D_{IRAP} did not correlate among the smokers.

Although there was no basis for predicting that the current smokers' implicit evaluating of the Smoking-ban might influence their level of tobacco addiction (e.g. the ban had yet to impact smoking-cessation rates within the Irish population; see Lonergan, 2013, p. 14), there were clear a priori reasons for predicting the converse. In particular, the heavier a smoker is the more likely they are to be frustrated on an ongoing basis by any smoking-ban (see Bancroft, Wiltshire, Parry, & Amos, 2003). Confirming this, stronger anti-ban bias for *Ban-Neg* D_{IRAP} correlated at a moderate-to-large degree with all three criteria addressing aspects of tobacco consumption, namely CPD, mFTQ and TC. Likewise, the smaller the smokers' pro-ban bias on the *Ban-Pos* D_{IRAP} the more it predicted increased tobacco consumption, but to a lesser degree than their anti-ban bias on *Ban-Neg* D_{IRAP}. The key point here is that in the former trial-type coordinated the Smoking-ban with positive attributes whereas the latter trial-type coordinated it with negative attributes. Insofar as the smokers will have experienced negative rather than positive reactions to the ban (e.g., when prevented from smoking while in a state of deprivation) the *Ban-Neg* trial-type should thus correlate more strongly with actual smoking behaviour than the *Ban-Pos* trial-type. In addition, YS and HONC are both related, albeit indirectly, to how often a Smoking-ban might frustrate a smokers' opportunity to smoke. Thus, likewise, it makes sense that *Ban-Neg* D_{IRAP} correlated with YS and HONC, but that *Ban-Pos* D_{IRAP} did not. Granted these latter interpretations are somewhat speculative. However, at the very least, even when IRAP findings are unanticipated like this they may still be useful insofar as they highlight precisely which types of implicit evaluating do versus do not merit further investigation in relation to tobacco addiction – something that the IAT is incapable of.

Testing the Potential of Implicit Measures to Reveal Tobacco Addiction Processes

On the Development of Tobacco Addiction via Implicit Evaluating

All standard measures of tobacco addiction are based upon rational, questionnaire-based self-reports and so they are fundamentally limited in their ability to disentangle the irrational nature of tobacco addiction (see for example Wiers, Houben, Roefs, de Jong, Hofmann, & Stacy, 2010). By contrast, implicit measures are specifically designed to measure automatic evaluating, which is presumably the basis for irrational conditions like tobacco addiction. We have already argued that implicit evaluating is causally involved in driving the responses that individuals provide on self-report measures of the intensity of tobacco addiction. To test this basic idea we modelled the extent to which the current IAT and IRAP effects were involved in driving self-reported measures of addiction intensity (i.e. CPD, a standard self-report measure of smoking consumption; mFTQ, a standard measure of the physiological aspects of tobacco dependence; and finally, HONC, a standard measure of the psychological aspects tobacco addiction). We reasoned that the more a particular type of implicit evaluating (e.g. *Smoking-Pos* D_{IRAP}) is involved in driving such intensity, the stronger that implicit effect should be as YS increases.

Consistent with the foregoing rationale, we used mediational analyses to rule out the possibility that D_{IAT} functioned in this way (i.e. presumably because it could not target particular types of implicit evaluating in isolation). By contrast, mediational analyses of the IRAP data confirmed multiple known-groups predictions about which types of implicit evaluating might (versus not) be involved in addiction intensity. Specifically, the only IRAP trial-type to have its relationship with YS mediated by CPD, mFTQ or HONC was the one which addressed smokers' stereotypical reasons for smoking, namely, Smoking-Pos. Of course, the current findings do not allow us to precisely determine to what extent the *Smoking-Pos effect* was involved in causing CPD, mFTQ or HONC – that would require showing that experimental manipulations of *Smoking-Pos* effect bring about consequent changes in these three intensity measures. The most important point here is that the IRAP, but not the IAT, provides the means to both guide and pursue the required experimental research.

On the Development of Implicit Evaluating via Tobacco Addiction

Given that one cannot be addicted to a behaviour that has not yet become "habitual", implicit evaluating is relatively unlikely to drive the *initial development* of that habit. As the behaviour becomes established, however, implicit evaluating likely strengthens (via repeated evaluating of that behaviour), and may thus feedback to drive the relevant behaviour on other occasions. It is therefore important to distinguish between the extent to which implicit evaluating is *integral* to existing tobacco addiction (i.e. drives the intensity), or emerges as a *collateral* result of it (i.e. without necessarily feeding back into that intensity). For example, imagine a smoker who repeatedly evaluates smoking as being enjoyable in response to his or her relief from nicotine withdrawal. When pro-smoking evaluating initially arises in this way, it is unlikely to influence any further smoking in that immediate context because the smoker has already relieved themselves of their cravings. However, the more often this occurs, the more likely it will be that the relevant smoker implicitly evaluates smoking favourably even before they relieve their cravings by smoking. Crucially, if the smoker later attempts to quit smoking they may thus implicitly evaluate smoking positively in proportion to how much they are experiencing tobacco cravings – in which case, the relevant implicit evaluating is integral in tobacco addiction to some extent, while also continuing to arise collaterally in other contexts (e.g., when smoking reduces a craving).

As noted previously, only the *Smoking-Pos* (D_{IRAP}) trial-type's relationship to YS was mediated by measures of addiction intensity (i.e., CPD, mFTQ and/or HONC). As such, this trial-type effect appears to be integral, rather than solely collateral, to tobacco addiction. Insofar as the D_{IAT}'s relationship to YS was not mediated by intensity it could be argued that the relationship was largely collateral. Given that one of the key aims of the current research programme was to develop an implicit measure of the "driving processes" of tobacco addiction, rather than its secondary effects the IRAP, or more precisely the *Smoking-Pos* effect appears to have outperformed the IAT in this regard.

Exploring the Involvement of Implicit Evaluating in Explicit Evaluating

The current D_{IAT} correlated with all four explicit measures of smoking-related evaluating, but could not reveal what types of implicit evaluating were involved. In contrast, the four trial-types structure of the IRAP provided more information. In summary, *Smoking-Pos* D_{IRAP} correlated strongly with both explicit evaluating of smoking and of the Smoking-ban, but the remaining three IRAP trial-type effects did not correlate strongly in this regard. This suggests that both smoking- and ban-related explicit evaluating were most influenced by the positive implicit evaluating of smoking. Notably, such findings raise the possibility of identifying what types of implicit evaluating are particularly likely to interfere with (or strengthen) a smoker's ongoing explicit evaluating about whether to continue smoking or not.

On Re-casting Old Psychometric Problems as New Types of Measurement

In line with classic psychometric practice, the implicit cognition literature has traditionally viewed implicit measures that exhibit trial block order effects, or low internal reliability, as a sign that they lack validity due to extraneous influences (e.g. Teige-Mocigemba, Klauer, & Sherman, 2010; Levin, Hayes, & Waltz, 2010). We argue to the contrary here that both variables can in principle provide us with new insights into tobacco addiction provided that they are applied to scores like those from IRAP trial-types which experimentally control what topic participants are implicitly responding in terms of in each case. Crucially, to the best of our knowledge, no other measure of implicit evaluating is specifically designed to experimentally compare implicit responding for versus against a topic phrased specifically as a propostion (see Chapters 1-3). And therefore, unlike other implicit measures, as follows each IRAP trial-type score is interpretable in terms of the participant implicitly evaluating some specific topic regardless of whether it has reduced internal consistency and regardless of whether trial block order cues variation in its absolute size. In principle, therefore, unlike other implicit measures, each IRAP trial-type score is interpretable in terms of a given participant implicitly evaluating some specific topic – and crucially, as follows, even when background variable(s) like trial block order or smoking-status affect the absolute size and/or internal consistency of that IRAP trial-type score it is still interpretable as an evaluative response bias toward a particular topic.

Trial-type D_{IRAP} Internal Reliability as a Measure of Implicit Indecisiveness

The internal reliability of a *trial-type D_{IRAP}* is, by definition, a measure of how consistently participants implicitly evaluate with respect to that trial-type's topic. Therefore, conversely, the lower the internal reliability of a *trial-type* effect the more it indicates that the corresponding implicit evaluating is inconsistent in the relevant context. We would argue that the lack of such *trial-type D_{IRAP}* internal reliability could be interpreted as a measure of *implicit indecisiveness* – that is, the extent to which implicit evaluating of a given topic fluctuates automatically between opposing perspectives on that particular topic within a given context. This could be important because conflicted evaluating is at the heart of what defines the problem of (tobacco) addiction. Consider, for example, when a smoker is trying to quit smoking and perhaps cycles between evaluating smoking as undesirable in one moment, and as compellingly desirable in the next.

Confirming the foregoing rationale, the current smokers exhibited good internal reliability on the only trial-type to address the perceived benefits of smoking, *Smoking-Pos D_{IRAP}*; but no internal consistency on the remaining trial-types – all of which addressed topics relevant to anti-smoking arguments, about which smokers are characteristically conflicted. Likewise, the non-smokers exhibited good internal reliability on all *trial-type D_{IRAP}*s except *Ban-Neg D_{IRAP}* – the only trial-type to address a relatively conflicted topic for non-smokers. That is, despite the fact that Irish non-smokers overwhelmingly supported the Irish Smoking-ban, they often complained about its implications (e.g. social gatherings commonly gravitating to makeshift outdoor smoking areas that were cold and wet; see Currie & Clancy, 2011, pp. 16-17; Lonergan, 2013, pp. 18-19).

Interestingly, the D_{IAT} exhibited exemplary, and sometimes higher internal reliabilities than the four IRAP trial-type effects, but crucially the IAT effect was unable to provide any information about the so-called implicit indecisiveness implicit evaluating with regard to any particular topic. Furthermore, when we compensated the internal reliability of the IRAP trial-type effects for the fourfold greater number of trials comprising the current IAT (see Appendix 11), the latter no longer outperformed the internal reliability of any of the IRAP trial-type effects that addressed a topic about which participants were likely to be unambivalent. And reassuringly, the relevant compensation algorithm did not spuriously grant internal reliability to any of the D-IRAP *trial-types* that did not already possess it.

IRAP Trial Block Order Effects as a Measure of Perspective-switching Tendencies

The trial block order variable determined whether participants commenced the current IRAP (or IAT) with a pro-smoking/anti-ban block of trials versus an anti-smoking/pro-ban block of trials. The current IRAP specifically involved instructing participants (during practise) to respond with one overarching pro-smoking response rule for the blocks of trials with pro-smoking response contingencies; and with the opposite overarching response rule for the blocks of trials with anti-smoking response contingencies (see Method). Thus, by the time participants completed the first IRAP trial block, they had already spent 24 trials adopting whatever pro- or anti-smoking perspective the response rule for that block required. And moreover, when it came to formulating how to respond to the second block, the IRAP explicitly informed participants that the previous response rule would be reversed during the next block:

> *IMPORTANT: DURING THE NEXT PHASE THE PREVIOUSLY CORRECT AND WRONG ANSWERS ARE REVERSED. THIS IS PART OF THE EXPERIMENT. PLEASE TRY TO MAKE AS FEW ERRORS AS POSSIBLE.*

As participants proceeded to each subsequent pair of IRAP blocks all that was required of them was to repeat the foregoing sequence. In effect, the IRAP trial block order variable could be seen as an experimental manipulation of whether participants were instructed to respond from an overarching pro-smoking perspective or from an overarching anti-smoking perspective (for a related point of view regarding the IAT, see Klauer & Mierke, 2005; Nosek et al., 2005).

Crucially, however, the foregoing instructions manipulated by IRAP trial block order did not ensure that participants coded their pro- and anti-smoking IRAP responses in terms of any particular rule. For example, smokers are characteristically pre-inclined to evaluate smoking from pro- but not anti-smoking perspectives. Therefore, even when an anti-smoking-first IRAP instructs a smoker to adopt an anti-smoking perspective, in principle, they are more likely on each trial, at least *initially*, to adopt a pro-smoking perspective than an anti-smoking perspective. Moreover, when participants are responding as fast as they can it minimizes any possibility of participants strategically adopting secondary perspectives. And as such, smokers are relatively unlikely to respond even to the anti-smoking blocks of an anti-smoking-first IRAP from an anti-smoking perspective. Instead, smokers are pre-inclined to make the required anti-smoking responses in opposition to whatever pro-smoking evaluative perspective they would normally adopt, at least initially (e.g. using a response rule like 'in anti-smoking trials respond with the opposite response that I would normally give'). By contrast, non-smokers do not frequently evaluate smoking from either pro- or anti-smoking perspectives, it is less likely that they might have any pre-inclinations that would compete with adherence to either type of IRAP trial block order instructions. In other words, there was nothing preventing the current non-smokers from adopting pro- and anti-smoking perspectives throughout the pro- and anti-smoking first IRAP conditions, respectively. In fact, the current non-smokers were young adults, and as such, they were characteristically similarly (un)accustomed to switching between the pro- and anti-smoking perspectives dealt with by the current trial-types depending upon context (see Chassin et al., 2007; Clancy, 2007; Dal Cin et al., 2007; Fahy et al., 2012; Fitz, Kaufman, & Moore, 2015; Pfizer Ireland, 2009).

Overall, therefore, with all else being equal, block order effects should only emerge on a given trial-type effect to the extent that participants have a similar amount of experience being cued to adopt each of the two opposing evaluative response perspectives it targeted in a given context (e.g. *True* versus *False* with respect to 'Smoking makes me feel Relaxed'). And by extension, when participants are characteristically pre-inclined like smokers to adopt pro- over anti-smoking evaluative perspectives in most contexts, it follows that they will be less prepared to respond in line with any block order instructions contradicting those pre-inclinations. Crucially, in this way, the IRAP block order variable has the potential to measure an individual's automatic tendency in any given context to sustainably switch between opposing implicit evaluating perspectives when instructed or otherwise cued to do so. Indeed, confirming this rationale, there was a moderate interaction effect between block order and smoking-status, whereby the non-smokers but not the smokers exhibited an overall block order effect. In particular, it appeared as though the smokers were relatively disinclined to sustain switches from pro- to anti-smoking perspectives (i.e. recall that all four of the smokers' IRAP effects were mainly in the pro-smoking direction and did not differ across the block order variable). In contrast, the trial-type effects for the non-smokers did show some sensitivity to the block order variable, suggesting a tendency to adopt either anti- or pro-smoking perspectives in a sustained manner.

However, no such information was available from the IAT block order variable owing the IAT's inability to distinguish particular types of implicit evaluating. Rather, both smokers and non-smokers alike exhibited moderate block order effects on D_{IAT} such that those in the pro-smoking-first condition were more pro-smoking/anti-ban than those who completed the IAT in the anti-smoking-first condition. In other words, IAT block order did not interact with any of the above known-groups differences, much less identify them. As such, whereas IRAPs have the potential (pending further research) to measure an individual's automatic tendency to sustainably switching between opposing (pro- versus anti-smoking) implicit evaluating perspectives within a given context, the IAT certainly does not. Crucially, tobacco addiction might broadly be characterised as a pervasive inability to sustain anti-smoking (implicit) evaluating as opposed to pro-smoking (implicit) evaluating. And therefore, in having the potential to measure an individual's tendency for such things, the IRAP might be better equipped than the IAT, in yet another way, to facilitate more differentiated understandings of tobacco addiction.

On the Confounded Nature of D_{IAT} with respect to Implicit Evaluating

All four of the smokers' trial-type IRAP effects were continuously and normally distributed, and consistent with very precise known-groups block order predictions – thus supporting the idea that they measured only one type of implicit evaluating each. However, in contrast, the smokers' D_{IAT}s were distributed in a polarized, bimodal fashion around zero such that they tended either to favour pro-smoking/anti-smoking-ban IAT trials over anti-smoking/pro-smoking-ban IAT trials, or vice versa. As such, the IAT confounded not just qualitatively different types of implicit evaluating among participants, but contrasting types; and moreover it did so even among smokers who were specifically sampled for pro-smoking evaluating as opposed to anti-smoking evaluating (i.e. frequent smokers who had not contemplated quitting within the previous 30 days). In addition, the current pattern of D_{IAT} block order effects suggested that the IAT not only confounded implicit evaluating among smokers (i.e. as per the bi-modal distribution of their D_{IAT}s), but also among contexts as follows.

Specifically, it is interesting that both the smokers and non-smokers showed block order effects on the IAT. One might expect such an effect for the non-smokers given that their implicit evaluating of smoking would be relatively weak and thus malleable by minor procedural variables. However, insofar as smokers' possess relatively established implicit evaluating of smoking it seems unlikely that smokers' evaluating should be similarly malleable by IAT block order. Therefore, the fact that we observed a similarly sized IAT block order effect for the smokers as for the non-smokers suggests that the smokers' D_{IAT}s were measuring different types of implicit evaluating in the pro- versus anti-smoking-first conditions. Moreover, this was not the case for the four IRAP trial-type effects – which suggests again that participants' implicit evaluating was homogenous on each IRAP trial-type between block orders.

Overall, therefore, it appears as though the IRAP trial-type effects each measured implicit evaluating that was relatively homogeneous both from participant to participant, and from pro- to anti-smoking contexts (i.e. insofar as the pro- and anti-smoking-first conditions were pro- and anti-smoking contexts, respectively). And in contrast, the current IAT effects appeared to have been comprised not just of different post hoc rationalisations of smoking from smoker to smoker (i.e. as per the bimodal distribution of smokers' D_{IAT}s), but it also appeared as though the smokers' IAT effects were comprised of arbitrarily different mixes of implicit evaluating from context to context (i.e. as per the similarly sized IAT block order effects for both smokers and non-smokers). Certainly, this interpretation tallies well with the fact that smoking-related IAT effects have typically failed to exhibit good test-retest reliability (see Chapter 2).

Granted, D_{IAT} internal reliability was high for both smokers and non-smokers. However, all this indicated was that participants tended to persist throughout a given IAT session with whatever particular response strategy or approach they initially adopted for completing the relevant IAT at the beginning of that session. It says nothing about the extent to which different participants might respectively respond using various different approaches to the same IAT within a given situation (see for example the quadruple process model; Conrey, Sherman, Gawronski, Hugenberg, & Groom, 2005); or indeed about the extent to which each participant might adopt various different approaches to completing a given IAT from occasion to occasion (e.g. as when the IAT exhibits low test-retest reliability). In summary, therefore, the bottom line is that even if we had found that the current IAT effects were at least somewhat integral to tobacco addiction, we would not have been able to identify what particular types of implicit evaluating were responsible like we did with the *Smoking-Pos* D_{IRAP}.

Conclusions

On balance, the current research convincingly demonstrated that the IRAP has greater potential than the IAT as a means of systematically guiding research toward a more differentiated understanding of tobacco addiction. Even though the IAT outperformed its predecessors, its counterpart IRAP provided much more information about smoking-related implicit evaluating. In all cases, the additional information resulted from the IRAP's ability to measure different types of implicit evaluating individually. Moreover, this was the case even though the current IRAP was artificially restricted, by design, to measure only those aspects of smoking-related implicit evaluating that were most accessible in principle to an IAT. Therefore, in order to explore the IRAP's full promise as a means of discovering the nature of tobacco addiction, the remainder of the current thesis addressed smoking-related implicit evaluating that is simply not discernible with an IAT. In particular, the next chapter will begin this exploration by pursuing a more precise understanding of smokers' typical reasons for smoking, which was the topic addressed by the most valid trial-type IRAP effect from the present study, *Smoking-Pos* D_{IRAP}.

CHAPTER 5: Testing the IRAP as a Means of Determining the Relative Motivational Importance of Smoking for Reward versus for Relief (Study 2)

5.1. INTRODUCTION

The preceding empirical chapter was primarily about demonstrating that the IRAP is more effective than the IAT as a means of investigating tobacco addiction. A key part of this demonstration was establishing that the IRAP but not the IAT could distinguish what types of smoking-related implicit evaluating are integral to tobacco addiction. In particular, the IRAP revealed that implicit evaluating of stereotypical reasons *against* smoking (e.g. feelings about the Irish Smoking-ban, or the potential drawbacks of smoking) was not integral to tobacco addiction, but that implicit evaluating of stereotypical reasons *for* smoking was. Crucially, however, the IRAP used in Study 1 did not distinguish among different types of stereotypical reasons for smoking – rather it focused upon just one particular type of pro-smoking implicit evaluating, namely, addressing how much smoking makes one feel good. Therefore, in order to improve our understanding of which types of implicit evaluating are most integral to tobacco addiction, the current study sought to develop an IRAP that would distinguish among additional stereotypical reasons *for* smoking.

There are of course many competing ways in which the literature implicates reasons for smoking in tobacco addiction (for reviews see Conklin et al., 2004; Köpetz et al., 2013; Piasecki et al., 2010; Robinson & Berridge, 1993, 2003; West, 2001). And yet, crucially, the literature is united in pivoting around the core issue of how much (tobacco) addiction is governed by *reward-* and/or *relief-focused* evaluating. That is, addiction theorists usually describe (tobacco) addiction as being some sort of interplay between behaviour dominated by the pursuit of pleasing feelings such as increased confidence, and behaviour dominated by the pursuit of relief from displeasing feelings such as drug-withdrawal symptoms (see Baker, Piper, McCarthy, Majeskie, & Fiore, 2004; Chassin et al., 2007; DiFranza et al., 2012; DiFranza, 2015; Everitt & Robbins, 2005; Glautier, 2004; Hyman et al., 2006; Koob, & Le Moal, 2008; McCallion & Zvolensky, 2015). Granted, some prominent theorists have argued over the past 10-15 years that reward- and relief-focused evaluative processes are merely secondary to (tobacco) addiction. Nevertheless, their so-called *incentive-sensitization* theories were generally formulated in a manner that attempts to clarify how reward- and relief–focused evaluating each interacts with addiction (Everitt & Robbins, 2005, p. 1483;

Robinson & Berridge, 1993, 2003, 2008). As such, by far the most unified way in which the literature distinguishes people's stereotypical reasons for tobacco addiction is between the feelings of reward versus relief that they attribute to smoking.

We therefore deemed it a major priority for the current study to employ an IRAP that could distinguish implicit evaluating of stereotypical reward- versus relief-focused reasons for smoking. In addition, most theories of tobacco addiction at least implicate mood as a primary moderator of pro-smoking (implicit) evaluating (Conklin et al., 2004; Robinson & Berridge, 2003; Spada, Albery, & Moss, 2015). In particular, it is a generally well-established fact that negative moods are an important trigger for pro-smoking evaluating (i.e. cravings) and thus also for relapse during abstinence from smoking (for meta-analysis see Heckman, Kovacs, Marquinez, Meltzer, Tsambarlis, Drobes, & Brandon, 2013; Perkins, 2009 see also Childs & De Wit, 2010; Conklin & Perkins, 2005; Germeroth, Wray, Gass, & Tiffany, 2013; Hwang & Yun 2015; McKee, Sinha, Weinberger, Sofuoglu, Harrison, Lavery, & Wanzer, 2011; Parrott & Murphy, 2012; Perkins, Ciccocioppo, Conklin, Milanak, Grottenthaler, & Sayette, 2008; Perkins, Karelitz, Giedgowd, & Conklin, 2013; Torres & O'Dell, 2015; Wetter, Kenford, Smith, Fiore, Jorenby, & Baker, 1999; Wray et al., 2013). Indeed, even though the evidence is less compelling, some researchers have claimed that positive emotions can trigger pro-smoking evaluations and/or smoking (Cook, Spring, McChargue, & Hedeker, 2004; Heckman et al., 2013; Veilleux, Conrad, & Kassel, 2013). Moreover, there is extensive self-report evidence that smokers' differentially apply reward versus relief evaluating to positive and negative moods, respectively, in order to regulate their emotions on an ongoing basis (see Bancroft et al., 2003; DiFranza, 2012, 2015; Koole, Webb, & Sheeran, 2015). Namely, smokers typically describe smoking as a discretionary way to enhance their enjoyment of positive moods regardless of their level of addiction; and in contrast, as they become more addicted they are characteristically more likely to describe themselves as needing to smoke in order to regulate their negative moods. In summary, there is extensive evidence that smokers explicitly evaluate smoking in highly *mood-consistent* reward- and relief-focused ways that are characteristic of tobacco addiction.

Furthermore, many cognitive theories of addiction are based around the idea that complex evaluating, like the foregoing mood-specific types, can gradually become habitual and thus implicit over time by repetition (e.g. as in constructs such as schemas, implementation intentions, implicit outcome expectancies, motivational orientations; see Goldman, Del Boca, & Darkes, 1999; Goldman, Reich & Darkes, 2006; Hendricks

& Brandon, 2005, 2008; Koole et al., 2015; McKee et al., 2003; Palfai & Wood, 2001; Rooke et al., 2008; Stacy & Wiers, 2010, p. 559; Tiffany, 1990; Watson et al., 2012). Crucially, however, the addiction literature has not yet succeeded in measuring any such mood-specific types of implicit evaluating as distinct from each other. The main reason for this state of affairs is that most implicit measures have been specifically designed to measure simple rather than complex types of implicit evaluating. And moreover, of those few methods that were designed to measure more complex instances, none apart from the IRAP have been designed in such a way as to allow them to distinguish one type of implicit evaluating from another (see Chapter 3). We therefore set out to address this important research deficit by designing an IRAP to measure not just how much individuals implicitly affirm versus deny reward- and/or relief-focused reasons for smoking, but also how much they do so in the mood-regulating ways outlined in the previous paragraph.

We began by coordinating our selection of IRAP label and target stimuli based upon extensive surveys of the vernacular ways in which smokers stereotypically phrase their reward- versus relief-focused reasons for smoking (see Bancroft et al., 2003; DiFranza 2012, 2015; McEwen et al., 2007; O'Connor et al., 2007; Pfizer Ireland, 2009). This resulted in an IRAP that incorporated the concept labels "I ENJOY SMOKING when I'm" and "I NEED to SMOKE when I'm", combined with six target words describing positive moods (e.g. *Delighted*) and six target words describing negative moods (e.g. *Upset*), and the response options "True" versus "False" (see Method). Thus, in summary, the first and second IRAP trial-types were designed to measure implicit evaluating of whether smoking is *enjoyable* when one is feeling good versus bad, respectively (i.e. for brevity we call these the *Enjoy-Pos* and *Enjoy-Neg* trial-types). And likewise, the third and fourth IRAP trial-types respectively measured implicit evaluating of whether smoking is *needed* when one is feeling good versus bad, respectively (i.e. for brevity we call these the *Need-Pos* and *Need-Neg* trial-types).

In particular, we expected that committed daily smokers (versus non-smoking controls) would implicitly evaluate reward- versus relief-focused reasons for smoking in a highly mood-consistent fashion. That is, we expected that such smokers would implicitly evaluate smoking as being enjoyable during positive moods but not during negative moods; and to implicitly evaluate smoking as being needed during negative moods but not during positive moods. Moreover, out of all the stereotypical reasons smokers offer for their habit, evaluating a need to smoke for relief from negative affect is the literature's most popularly cited reason for the gradual intensification of tobacco addiction (i.e. as opposed to initiating smoking which is often viewed as being motivated by reward-focused evaluating; see Baker et al., 2004; Conklin et al., 2004; DiFranza, 2012, 2015). As such, we were particularly interested in testing whether the *Need-Neg* trial-type effect might be more integral to tobacco addiction than the reward-focused *Smoking-Pos* effect reported in Study 1. Indeed, with similar concerns for discriminative validity in mind, we also sought to test whether the *Need-Neg* effect might be more integral to tobacco addiction than the other three trial-type effects measured by the current IRAP.

5.2. METHOD

Participants

We implemented the same known-groups sampling strategy as in Study 1 of the current thesis, but more stringently. As before, all participants were randomly assigned without replacement between the two levels of the IRAP trial block order variable (see below). In all we recruited 37 undergraduate participants, excluding five for failure to progress to the IRAP's test phase within four pairs of practise blocks (see below), and retaining 16 smokers (8 females) and 16 non-smokers (8 females). The qualifying non-smokers had not smoked any substances during the previous 12 months or indeed on more than one occasion before that (i.e. as opposed to a maximum of 10 previous occasions in Study 1).

By contrast, the smokers reported that they had not attempted, or even *contemplated* restricting their smoking during the previous 12 months as defined by the transtheoretical model of behavioural change (e.g. see Norcross, Krebs, & Prochaska, 2011; i.e. as opposed to within the previous 30 days in Study 1). Indeed, 75% of the smokers had never attempted to quit smoking before, and the remainder had only ever attempted to quit once before. On average, the current smokers had: been smoking regularly for 4.6 years (*SD* = 1.6; *range* = 2-7); smoked on 99% of the previous 30 days; consumed 11.9 CPD on those days (*SD* = 4.6, *range* = 6-20); and, were moderately nicotine dependent both physiologically and psychologically as per respectively the mFTQ (*M* = 4.3; *SD* = 1.1; *range* = 3-7; see Prokhorov et al., 1996; Prokhorov et al., 1998, p. 42) and the HONC (*M* = 3.3; *SD* = 2.4; *range* = 1-7; cf. Wellman, Di Franza, Pbert, et al., 2005; Wellman, Savageau, et al. 2006). Moreover, at the end of the current study the smokers reported moderate-to-large tobacco cravings (TC) on average (*M* = 2.1; *SD* = .6; *range* = 1-3; see below for scaling), and 81% of them reported that they would 'probably' or 'definitely' smoke a cigarette upon leaving (i.e. only 19% reported that they would 'possibly' do so, and none selected the remaining alternative 'definitively not'). Otherwise, the smokers (M_{age} = 21.7 years; *SD* = 1.1; range = 20-23) and non-smokers (M_{age} = 21.3 years; *SD* = 1.7; range = 19 - 24) were closely matched in terms of age, $t(30) = .76$; $p = .46$.

Apparatus and Materials

State and Trait Measures of Tobacco Addiction Intensity

We used all of the measures of trait tobacco addiction intensity from Study 1; namely, CPD, mFTQ, the HONC. Also, as a measure of state tobacco addiction intensity, we used a slightly modified version of the TC scale from Study 1 (i.e. we renumbered it such that it now scaled from 0 to 6 instead of from -3 to +3. Moreover, we screened participants using a DBHQ based upon that from Study 1, but with many additional clarifications about sampling which emerged across participant interviewing in Study 1 (see Appendix 12). In addition, unlike Study 1, the current study employed Gifford's (2002) *Avoidance and Inflexibility Scale*, a 13-item questionnaire designed to determine how inclined smokers' are to adopt experientially-avoidant and inflexible strategies for managing tobacco cravings (see Appendix 13). A recent large-scale study of the AIS indicated that its total score could optionally be sub-divided into a 9-item "thoughts/feelings" factor (focused upon cravings experienced as thoughts and feelings), and a 4-item "somatic sensations" factor (focused upon cravings experienced as bodily sensations; see Farris, Zvolensky, DiBello, & Schmidt, 2015). Crucially, Farris et al. concluded that both AIS scales exhibited highly promising psychometric properties as measures of smoking-specific experiential avoidance (e.g. *Cronbach's α*s ≥ .89; *test-retest r*s = .52-.76). Confirming this, the current study obtained a *Cronbach's α*s ≥ .95 on both the AIS total scale and its sub-scales whether the smokers were considered with the non-smokers or alone.

An IRAP Measuring Implicit Evaluating of Mood-dependent Smoking

We employed the same version of the IRAP program as in Study 1, but using different label and target stimuli. As before, participants were presented with the response options "True" and "False" on every IRAP trial but this time the concept label stimuli were "I ENJOY SMOKING when I'm" and "I NEED to SMOKE when I'm", and the target stimuli used in Study 1 were modified for compatibility with these label stimuli (i.e. especially from a typical smokers' perspective). Namely, the positive target words were: *Relaxed*, *Happy*, *Delighted*, *Chilled-out*, *Satisfied*, and *Confident*. And the negative target words were: *Tense*, *Suffering*, *Upset*, *Anxious*, *Irritated*, and *Embarrassed*. The researcher instructed participants before each mood-consistent practise block to "Respond as if you *enjoy* smoking when you feel *good*, and as if you *need* smoking when you feel *bad*", and before each mood-inconsistent practise block to "Respond as if you *enjoy* smoking when you feel *bad*, and as if you *need* smoking when you feel *good*." Otherwise, the current IRAP was the same as that used in the Study 1. For half the participants all of the odd numbered IRAP blocks required a mood-consistent pattern of responding and the even numbered blocks mood-inconsistent responding; for the remaining participants the opposite applied (odd numbered blocks required mood-inconsistent responding and even numbered blocks mood-consistent responding).

Measures of Explicit Evaluating

We employed four feeling thermometers, much like those used in Study 1, but which were designed to be respectively analogous to the current IRAP's four trial-types. In each case, participants were asked to mark a thermometer visual analogue scale ranging from zero labelled "Extremely Cold (Disagree)" to ninety-nine labelled "Extremely Warm (Agree)", in response to a statement summarizing one of the IRAP trial-types. The four statements in question were "I ENJOY SMOKING when I'm Feeling GOOD", "I ENJOY SMOKING when I'm Feeling BAD", "I NEED to SMOKE when I'm Feeling GOOD", and "I NEED to SMOKE when I'm Feeling BAD". In addition, we also employed four different types of semantic differential scales, also respectively addressing the four IRAP trial-types, but which individually incorporated each IRAP antonymic mood word pair. Thus, in all, we employed 12 semantic differential scales ranging from -3 to + 3. As in Study 1, we sometimes labelled positive mood attributes with +3 and negative mood attributes with -3, and at other times labelled them vice versa in order to discourage inattentive responding,

Procedure

Participants completed the IRAP, followed by the four measures of explicit evaluating, and then by the state and trait tobacco addiction questionnaires. Thereafter, the researcher interviewed participants using the DBHQ in an open-ended fashion to reveal any unanticipated details with relevance to the current known-groups sampling agenda. Apart from during the interview, and during the IRAP practise trials, the participants completed their tasks alone in a sound-proofed cubicle.

5.3. RESULTS

5.3.1. Scoring the IRAP Data

We scored the IRAP test data in exactly the same way as we did in Study 1 – using the D_{IRAP}-algorithm described in Appendix 4, and such that participants were excluded from analysis if they responded slower than 3000*ms*, or less accurately than 80%, on average to the IRAP test trials. All 32 participants who completed the IRAP test phase qualified by a comfortable margin for inclusion in the current analysis. Specifically, the smokers completed the mood-consistent and -inconsistent test blocks with average respective latencies of 1807*ms* and 2125*ms*, and average respective accuracies of 92% and 87%. Likewise, the non-smokers completed the mood-consistent and -inconsistent test blocks with average respective latencies of 1955*ms* and 2057*ms*, and average respective accuracies of 91% and 89%.

5.3.2. Validity with Respect to Smoking-status

Known-Groups Differences

The IRAP data were entered into a preliminary 2x2x2x2 mixed factorial ANOVA, which crossed *smoking-status* (i.e. smokers versus non-smokers) and *trial block order* (i.e. mood-consistent-first versus mood-inconsistent-first) with the two repeated measures IRAP 'trial-type' variables *concept type* (i.e. "I ENJOY SMOKING when I'm" and "I NEED to SMOKE when I'm") and *target type* (i.e. positive versus negative moods). We obtained a large main effect for smoking-status, $F(1, 28) = 12.67$, $p = .001$, $\eta_p^2 = .31$, but no main effects for the remaining three variables, η_p^2s $\leq .03$, $p \geq .54$.

However, as illustrated in Figure 5.1, the main effect for smoking-status was qualified by a moderate four-way interaction with both trial block order and the two IRAP variables, $F(1, 28) = 1.93$, $p = .18$, $\eta_p^2 = .07$. In addition, concordantly, smoking-status interacted moderately with concept type, $F(1, 28) = 3.68$, $p = .07$, $\eta_p^2 = .12$, and there were moderate interactions between smoking-status and target-type, $F(1, 28) = 2.23$, $p = .15$, $\eta_p^2 = .07$, and between smoking-status, target-type and trial block order, $F(1, 28) = 1.79$, $p = .19$, $\eta_p^2 = .06$, as well as between smoking-status, concept-type, and target-type, $F(1, 28) = 2.20$, $p = .15$, $\eta_p^2 = .07$. Thus, overall, it appeared as though each of the four *trial-type* D_{IRAP}s operated differently with respect to the interaction between smoking-status and block order. We therefore conducted four follow-up 2x2 between-groups ANOVAs to respectively analyse the impact of smoking-status and block order on each of the four *trial-type* D_{IRAP}*s*.

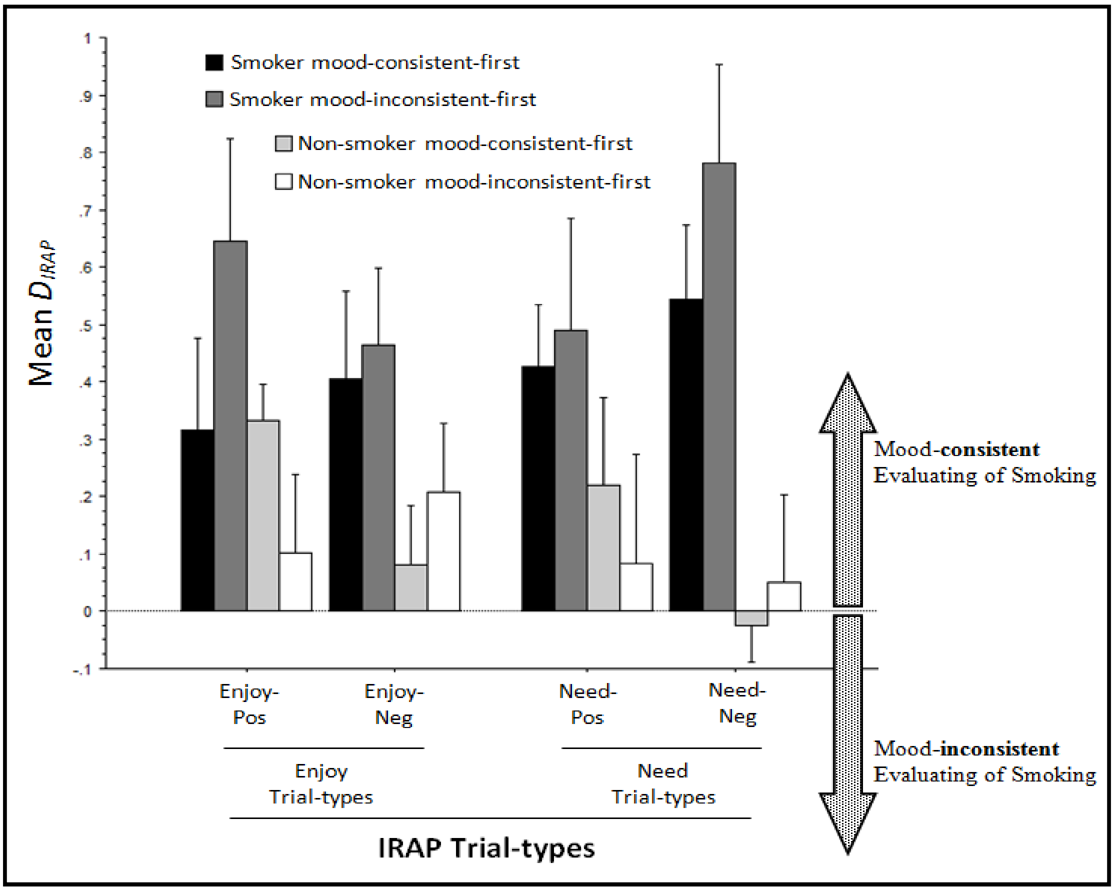

Figure 5.1. Mean *trial-type* D_{IRAP} scores, with standard error bars, for smokers and non-smokers split by trial block order, by IRAP concept-type and by IRAP target-type. A positive D_{IRAP} score for the *Enjoy-Pos* trial-type indicated faster *True* rather than *False* responses to label-target combinations like *I ENJOY SMOKING when I'm – Relaxed*. Positive *Enjoy-Neg* D_{IRAP}s indicated faster *False* rather than *True* responses to label-target combinations like *I ENJOY SMOKING when I'm – Anxious*. And likewise, positive *Need-Pos* D_{IRAP}s or *Need-Neg* D_{IRAP}s also indicated faster mood-consistent than mood-inconsistent responses. By corollary, negative *trial-type* D_{IRAP}s indicated the opposite to their positive counterparts; and those in the neighbourhood of zero (e.g. $-.1 < D_{IRAP} < .1$) indicated that mood-consistent and -inconsistent responses were produced with comparable fluency to each other.

Exploring Interactions between Smoking-status and Block Order

Smoking-status by block order on Enjoy-Pos D_{IRAP}. There was a moderate-to-large main effect of smoking-status, $F(1, 28) = 3.51$, $p = .07$, $\eta_p^2 = .11$, on *Enjoy-Pos* D_{IRAP}; but no main effect of block order, $F(1, 28) = .12$, $p = .73$, $\eta_p^2 = .004$. However, block order did interact to a moderate-to-large degree with smoking-status, $F(1, 28) = 3.94$, $p = .06$, $\eta_p^2 = .12$, and so we conducted multiple follow-up contrasts to unpack this interaction. Most notably, block order not only impacted smokers' D_{IRAP}, but it did so in an ironic fashion which was also in the opposite direction to its impact upon non-smokers' D_{IRAP}s. Namely, the smokers' D_{IRAP} was more mood-consistent in the mood-inconsistent-first condition than in the mood-consistent-first condition (i.e. rather than vice versa), $t(14) = 1.38$, $p = .19$, $\eta^2 = .12$. And in contrast, the non-smokers' D_{IRAP} was more mood-consistent in the mood-consistent-first condition than in the mood-inconsistent-first condition, $t(14) = 1.53$, $p = .15$, $\eta^2 = .14$. Thus, whereas the non-smokers' responses to the *Enjoy-Pos* trial-type were influenced in line with the block order manipulation, the smokers' responses to the *Enjoy-Pos* trial-type appeared to rebound against it. Ultimately, this resulted in there being no difference between smokers and non-smokers on *Enjoy-Pos* D_{IRAP} in the mood-consistent-first condition, $t(14) = .09$, $p = .93$, $\eta^2 = .0006$, but a large smoking-status difference in the mood-inconsistent-first condition, $t(14) = 2.43$, $p = .03$, $\eta^2 = .30$. Indeed, the non-smokers implicitly evaluated smoking in a similarly large mood-consistent fashion on the *Enjoy-Pos* trial-type during the mood-consistent-first block order, $t(7) = 5.16$, $p = .001$, $\eta^2 = .79$, as the smokers did during both block orders of the *Enjoy-Pos* trial-type, $t(7)$s = 1.99, 3.64, $p = .09$, .01, $\eta^2 = .36$, .65. In contrast, however, the non-smokers implicitly evaluated smoking in only a moderately mood-consistent fashion in the mood-inconsistent-first condition, $t(7) = .76$, $p = .47$, $\eta^2 = .08$.

Smoking-status by block order on Enjoy-Neg D_{IRAP}. There was a large main effect of smoking-status, $F(1, 28) = 5.03$, $p = .03$, $\eta_p^2 = .15$, on *Enjoy-Neg* D_{IRAP}; but no main or interaction effects with block order, $F(1, 28)$s $\leq .53$, $p \geq .47$, $\eta_p^2 \leq .02$. In summary, the smokers and the non-smokers both implicitly evaluated smoking mood-consistently, but the smokers did so, $t(15) = 4.40$, $p = .0001$, $\eta^2 = .56$, to a greater degree than the non-smokers, $t(14) = 1.82$, $p = .09$, $\eta^2 = .18$.

Smoking-status by block order on Need-Pos D_{IRAP}. There was a moderate-to-large main effect of smoking-status, $F(1, 28) = 3.43$, $p = .08$, $\eta_p^2 = .11$, on *Need-Pos* D_{IRAP}; but no main or interaction effects with block order, $F(1, 28)$s $\leq .36$, $p \geq .56$, $\eta_p^2 \leq .01$. In summary, the smokers and the non-smokers both implicitly evaluated smoking mood-consistently, but the smokers did so, $t(15) = 4.22$, $p = .001$, $\eta^2 = .54$, to a greater degree than the non-smokers, $t(14) = 1.27$, $p = .22$, $\eta^2 = .10$.

Smoking-status by block order on Need-Neg D_{IRAP}. There was a large main effect of smoking-status, $F(1, 28) = 22.86$, $p < .0001$, $\eta_p^2 = .45$, on *Need-Neg* D_{IRAP}; but no main or interaction effects with block order, $F(1, 28)$s ≤ 1.31, $p \geq .26$, $\eta_p^2 \leq .04$.[48] In summary, the smokers implicitly evaluated smoking mood-consistently, $t(15) = 6.12$, p

[48] The main effect of block order was on Cohen's threshold for a moderate effect, $F(1, 28) = 1.31$, $p = .26$, $\eta_p^2 = .04$. Crucially, however, this main effect was clearly non-existent as a simple effect for the non-smokers, as per Figure 5.1, even though the relevant interaction was null by the same standards, $F(1, 28) = .35$, $p = .56$, $\eta_p^2 = .01$. On balance, therefore, given the relatively small sample size involved in each experimental cell ($n = 8$), we concluded that it would be most prudent to interpret this borderline main effect as null until further evidence emerges to the contrary.

< .0001, η^2 = .71, but in contrast the non-smokers implicitly evaluated smoking indecisively[49], $t(14) = .17$, $p = .87$, $\eta^2 = .002$.

Known-Groups Sensitivity and Specificity in Diagnosing Smoking-status

The *Need-Neg* D_{IRAP} ROC curve exhibited the largest *AUC* = .88, *p* = .07, *d* = 1.15, (i.e., $r \approx .50$), of the four IRAP trial-type effects. This means that if a smoker and a non-smoker were chosen at random from the present sample, then 88% of the time the smoker would have a higher *Need-Neg* D_{IRAP} than the non-smoker (i.e. as compared to 50% of the time by chance). Not only did *Need-Neg* D_{IRAP} achieve a higher *AUC*, *Z* = 1.37; *p* = .09; *d* = .49 (i.e., $r \approx .24$),[50] than the *Need-Pos* D_{IRAP}, *AUC* = .69, *p* = .07, *d* = .49, (i.e., $r \approx .24$) (see Figure 5.2), but likewise it also achieved a higher *AUC* than both of the Enjoy-related *trial-type* D_{IRAP}s, respectively, *Z*s = 1.90, 1.37; *p*s = .03, .09; *d*s = .72, .49 (i.e., *r*s ≈ .34, .24). Specifically, the *Enjoy-Pos* D_{IRAP} exhibited a null *AUC* = .66, *p* = .12, *d* = .27 (i.e., $r \approx .14$), and the *Enjoy-Neg* D_{IRAP} did only slightly better insofar as it achieved a moderate-to-small sized smoking-status *AUC* = 0.73, *p* = .03, *d* = .43 (i.e., $r \approx .21$; see Figure 5.3).

[49] Note however, that this could indicate either of two types of implicit indecisiveness. For example, if an IRAP effect arises in the neighbourhood of zero with a lack of internal reliability, in principle it would indicate *active indecisiveness* insofar as its lack of internal reliability reflected that participants were pre-inclined to respond in both competing ways to its constituent trials. In contrast, if for example an IRAP effect arises in the neighbourhood of zero with good internal reliability, and without being affected by block order, it would in principle indicate *passive indecisiveness* – insofar as good internal reliability indicates that participants are not pre-inclined to respond in *both* competing ways to the relevant trial-type, and insofar as an IAT effect close to zero and not moderated by block order indicates that participants were also not pre-inclined to respond in *one* way more than the other on that trial-type. In other words, passive indecisiveness denotes that a participant is relatively unfamiliar with whatever topic of implicit evaluating is in question. Crucially, we can only determine this latter eventuality by knowing the vectoring of an IRAP effect *and* its internal reliability *and* whether it was moderated by block order.

[50] Calculated using Lowry's (2012) software implementation of Hanley and McNeil's (1982) *Z*-test algorithm for comparing the significance of the difference between the areas under two independent ROC curves.

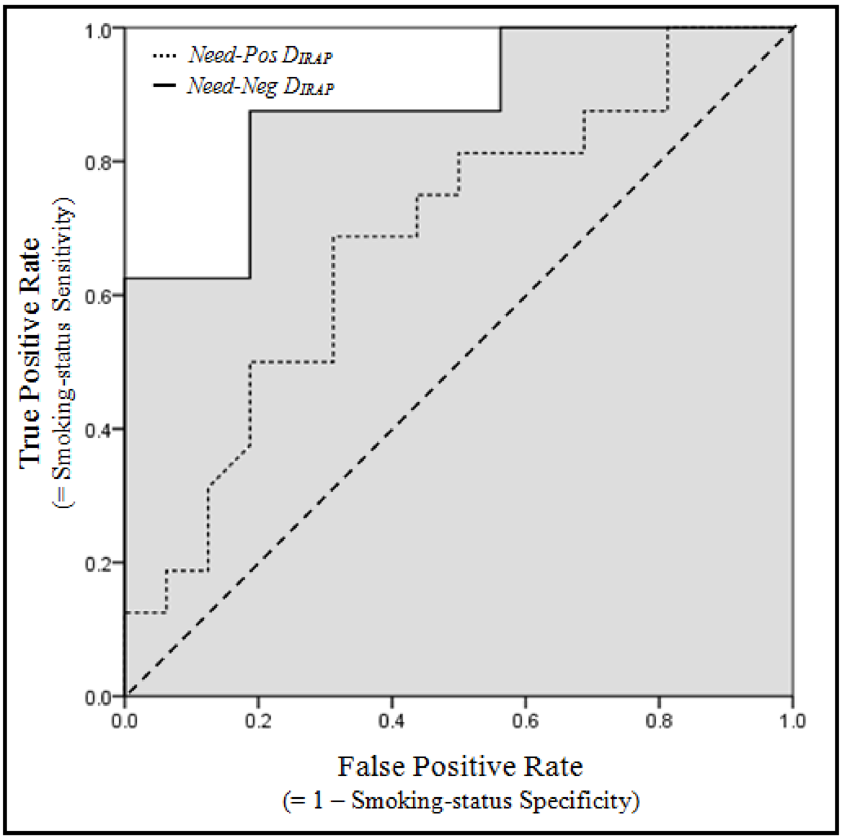

Figure 5.2. The smoking-status *ROC* curves for *Need-Pos* D_{IRAP} and *Need-Neg* D_{IRAP}. Note that the dashed diagonal line represents chance levels of smoking-status diagnosis (i.e. a ROC curve with an *AUC* = .5).

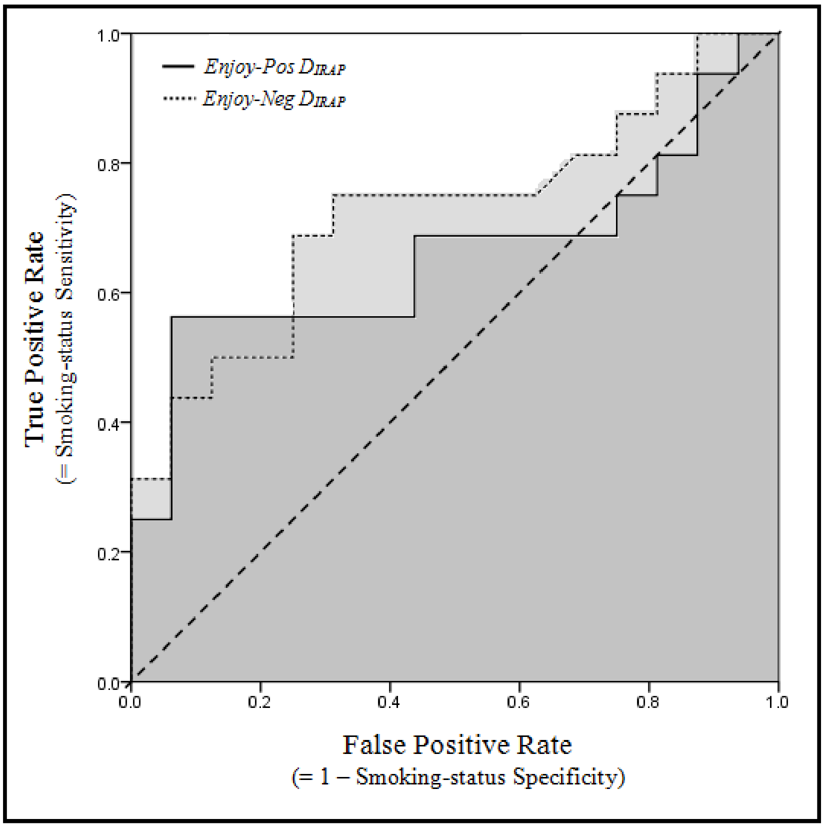

Figure 5.3. The smoking-status *ROC* curves for *Enjoy-Pos* D_{IRAP} and *Enjoy-Neg* D_{IRAP}. Note that the ROC curve for *Enjoy-Pos* D_{IRAP} in particular was in close correspondence with the dashed diagonal line representing chance levels of smoking-status diagnosis (i.e. a ROC curve with an *AUC* = .5).

4.3.3. Modelling the *trial-type* D_{IRAP}s in terms of Tobacco Addiction Criteria

Preliminary Correlation Analyses

Of the four IRAP trial-type effects, the *Need-Neg* D_{IRAP} correlated to the greatest degree with the six tobacco addiction criteria – not only did it correlate with all six criteria, it did so to a large degree in each case, average r = .61 (see upper panel of Table 5.1). Granted, the other three IRAP trial-type effects also correlated with all six criteria – but overall, they did so moderately rather than largely: *Enjoy-Pos* D_{IRAP}, average r = .32; *Enjoy-Neg* D_{IRAP}, average r = .39; *Need-Pos* D_{IRAP}, average r = .36 (Table 5.1). Indeed, confirming this, correlation Z-tests among the averaged criterion correlations for each trial-type effect indicated that *Need-Neg* D_{IRAP} achieved moderately higher criterion correlations on average than *Enjoy-Pos* D_{IRAP}, *Enjoy-Neg* D_{IRAP}, and also *Need-Pos* D_{IRAP}, Z = 1.44, 1.13, 1.26; ps = .07, .13, .10; ds = .52, .41, .45 (i.e. rs ≈ .25, .20, .22).

Table 5.1

A zero-order correlation matrix of the four IRAP trial-type effects and six tobacco addiction criteria: YS, CPD, mFTQ, HONC, AIS and TC.

	The Four IRAP Trial-type Effects & The Five Tobacco Addiction Criteria					
	YS	***CPD***	***mFTQ***	***HONC***	***AIS***	***TC***
Enjoy-Pos D_{IRAP}	.34*	.29*	.33*	.39*	.27#	.29#
Enjoy-Neg D_{IRAP}	.37*	.38*	.37*	.47**	.40*	.34*
Need-Pos D_{IRAP}	.35*	.39*	.37*	.39*	.32*~	.35*
Need-Neg D_{IRAP}	.59***	.58***	.66****	.58***	.62****	.65****
YS	--	.92****	.94****	.75****	.75****	.92****
CPD		--	.90****	.69****	.85****~	.90****
mFTQ			--	.75****	.97****~	.98****
HONC				--	.69****	.71****
AIS					--	.87***

#$p \leq .1$, *$p \leq .05$, **$p \leq .01$, *** $p \leq .001$, **** $p \leq .0001$ (all ps one-tailed). N = 32 except where ~ indicated N = 31. *Note.* All ps are uncorrected for familywise error, but note Bonferroni corrected p < .01 should be applied to each sextuple of correlations between each *trial-type* D_{IRAP}s and the six tobacco dependence criteria because each constitutes a different domain of variability (Howell, 2012a).

Causal Path Analyses

We used the Hayes (2012) '*PROCESS*' software for bootstrapped casual path analysis to estimate the extent to which the four IRAP trial-type effects measured implicit evaluating that could have been integral to tobacco addiction intensity. In particular, we tested CPD, mFTQ, HONC and AIS *separately* as candidate mediators between YS and each of the D_{IRAP}s because of the high degree of collinearity and conceptual reciprocity among them, .69 < *r*s < .98; all *p*s < .0001 (see lower panel of Table 5.1; see also DiFranza, Ursprung, & Biller, 2012; O'Loughlin et al., 2002, *p*. 359; Wellman et al., 2006).

Overall, *Need-Neg* D_{IRAP} was the only IRAP trial-type effect in the current study to have been substantially mediated by any of the four tobacco addiction intensity criteria. More specifically, we estimated that up to 10.2%, 12.6%, 6.7% and 10.5% of *Need-Neg* D_{IRAP} could have developed integrally with CPD. mFTQ, HONC and AIS, respectively. In each case we calculated the relevant estimation by multiplying the κ^2 for the relevant candidate mediator (i.e. κ^2s = .35, .36, .19, .31, *p*s ≤ .05, respectively for CPD, mFTQ, HONC and AIS) by the percentage of shared zero-order variance observed between YS and the *Need-Neg* D_{IRAP} for each of the respective mediation models (i.e. $F[1, 28] = 11.52$, $R^2 = .29$, for the CPD model; $F[1, 30] = 14.81$, $R^2 = .35$, for the mFTQ and HONC models; $F[1, 29] = 12.45$, $R^2 = .34$, for the AIS model; for more detailed statistics see Appendix 14). Thus, for example, approximately (κ^2 =) 35% of the 29% shared variance between YS and *Need-Neg* D_{IRAP} could, in principle, have developed integrally with mFTQ; which amounts to 10.2% of *Need-Neg* D_{IRAP} overall.

In contrast, the respective relationships between YS and each of the three other trial-type effects were not sufficient, $3.86 \leq F(1, 30)\text{s} \leq 4.68$, $.07 \leq R^2\text{s} \leq .15$, for any of the four addiction intensity criteria to have an overall impact as candidate mediators on any of these trial-type effects (i.e. overall percentage of relevant trial-type effects mediated ≈ 0.4-3.6%; see Appendix 14). Furthermore, not only was *Need-Neg* D_{IRAP} unique among the three other trial-type effects with regard to being integral to tobacco addiction intensity, the former was also more extensively related to these criteria collaterally than the latter. Namely, CPD, mFTQ, HONC and AIS, respectively, explained 9%, 9%, 4%, and 8% of *Need-Neg* D_{IRAP} in addition to YS (i.e. *F*-changes = 3.94, 4.85, 1.99, 4.12; *p*s = .06, 04, .17, .05; R^2-changes = .09, .09, .04, .08).[51] And in contrast, the remaining three trial-type effects did not exhibit distinct collateral paths from any of the tobacco addiction intensity criteria, .03 ≤ *F*-changes ≤ 1.10 , .87 ≥ *p*s ≥

[51] In principle, the additional variance in *Need-Neg* D_{IRAP} explained by each of the tobacco addiction criteria in addition to YS may have reflected some recent causal influence of implicit evaluating upon tobacco addiction (i.e. within the previous year). However, our participants were specifically sampled for stable patterns of smoking during the preceding year, and therefore it seems likely that the majority of the relevant partial correlations instead reflected the causal influence of tobacco addiction (as per CPD, mFTQ, HONC and/or AIS) upon *Need-Neg* D_{IRAP}.

.30, 0.001 ≤ R^2-changes ≤ .035, except for all of them with HONC, 1.32 ≤ F-changes ≤ 3.38, .26 ≥ ps ≥ .08, .04 ≤ R^2-changes ≤ .09. Overall, therefore, *Need-Neg* D_{IRAP} appeared to be 7-13% integral and 4-9% collateral to CPD, mFTQ, HONC and AIS, but the remaining three trial-type effects each only exhibited distinct collateral paths with HONC (4-9%) and each lacked integral relationships with any of the four intensity criteria.

5.3.4. Validity with Respect to Explicit Evaluations

All eight explicit measures were rescored so that their polarity concurred with those of the four IRAP trial-type effects (i.e. positive ≡ mood-consistent, and negative ≡ mood-inconsistent), and so that zero ratings purported to be neutral. The resulting semantic differentials all exhibited extremely high internal reliability with *Cronbach alphas* ranging from .980 to .995. In broad terms, as per Table 5.2, the smokers explicitly evaluated in a highly pro-smoking (and mood-consistent) fashion that smoking is enjoyable when one is feeling good and needed when one is feeling bad, $t(15)$s ≥ 4.57, one-tailed ps ≤ .0002, η^2 ≥ .58.

By contrast, however, smokers were more equivocal in their explicit evaluating of smoking as enjoyable when one is feeling bad, or needed when one is feeling good. Specifically, on the relevant semantic differentials they evaluated smoking in a moderately pro-smoking (and mood-inconsistent) fashion, $t(15)$s ≥ -1.32, one-tailed ps ≥ .90, η^2 ≥ .10 (i.e. that smoking *is enjoyable* when one is feeling bad, and *is needed* when one is feeling good), but by contrast on the corresponding feeling thermometers they evaluated smoking in a moderately anti-smoking (and mood-consistent) manner, $t(15)$s ≥ 1.35, one-tailed ps ≤ .10, η^2 ≥ .11 (i.e. that smoking is *not enjoyable* when one is feeling bad, and *not needed* when one is feeling good).[52] The non-smokers explicitly evaluated smoking in a broadly opposite manner to the smokers: they tended to explicitly evaluate smoking as being *not enjoyable* and *not needed* whether one is feeling good or bad, $t(15)$s ≥ 6.03, one-tailed ps ≤ .0001, η^2 ≥ .71.

As per Table 5.3 *Enjoy-Pos* D_{IRAP} and *Need-Pos* D_{IRAP} both failed to correlate with any of the eight measures of explicit evaluating. In contrast, both *Enjoy-Neg* D_{IRAP} and *Need-Neg* D_{IRAP} correlated to a moderate-to-large positive degree with explicit evaluating of smoking as enjoyable when one is feeling good, and with explicit evaluating of smoking as needed when one is feeling bad, .37 ≤ rs ≤ .58, ps ≤ .05. Moreover, conversely, both *Enjoy-Neg* D_{IRAP} and *Need-Neg* D_{IRAP} correlated to

[52] Note that here we interpreted the degree of (dis)favour in relation to the respective Likert-type scales rather than in relation to the size of the relevant statistical effect.

moderate-to-large negative degree with explicit evaluating of smoking as enjoyable when one is feeling bad, and with explicit evaluating of smoking as needed when one is feeling good, $-.61 \leq r$s $\leq -.32$, ps $\leq .10$.

Table 5.2

The means and bracketed standard deviations of the four semantic differential scores, and the four feeling thermometer scores split by smoking-status.

	Semantic Differentials				Feeling Thermometers			
	Enjoy-Pos	*Enjoy-Neg*	*Need-Pos*	*Need-Neg*	*Enjoy-Pos*	*Enjoy-Neg*	*Need-Pos*	*Need-Neg*
Smoker	1.7 (1.4)	-0.5 (1.0)	-0.3 (1.0)	2.1 (1.4)	25 (22)	23 (27)	15 (16)	35 (24)
Non-smoker	-2.3 (1.5)	2.4 (1.2)	2.5 (1.2)	-2.3 (1.6)	-41 (27)	41 (24)	45 (15)	-41 (27)

Note. Thermometer scores ranged from -50 to +50, and semantic differential scores ranged from -3 to +3. Positive scores indicated mood-consistent evaluating of smoking, negative scores indicated mood-inconsistent evaluating of smoking, and near-zero scores purported neutral evaluating of smoking.

Table 5.3

The correlation matrix of the four IRAP trial-type effects crossed with the eight explicit evaluation scores.

	Semantic Differentials				Feeling Thermometers			
	Enjoy-Pos	*Enjoy-Neg*	*Need-Pos*	*Need-Neg*	*Enjoy-Pos*	*Enjoy-Neg*	*Need-Pos*	*Need-Neg*
Enjoy-Pos D_{IRAP}	.14	-.19	-.17	.16	.11	-.12	.15	.14
Enjoy-Neg D_{IRAP}	.37*	-.38*	-.38*	.40*	.38*	-.37*	-.32#	.40*
Need-Pos D_{IRAP}	.03	.05	.05	-.001	-.03	.07	.08	-.002
Need-Neg D_{IRAP}	.57***	-.61***	-.59***	.56***	.57***	-.47**	-.51**	.58***

$p \leq .10$, * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$, **** $p \leq .0001$, with all ps one-tailed. ($N = 32$).

5.3.5. Validity in Relation to Internal Reliability

The smokers exhibited relatively high internal reliability on all four of the IRAP trial-type effects as per the upper part of Table 5.4, $.47 \leq r_{sb}$s $\leq .87$; and particularly when we compensated our estimates for the typically fourfold greater test-length held by IATs, $.78 \leq$ compensated-r_{sb}s $\leq .96$. Similarly, the non-smokers exhibited acceptable internal reliability on *Enjoy-Pos* D_{IRAP}, *Need-Pos* D_{IRAP}, and *Neg-Pos* D_{IRAP}, $.52 \leq r_{sb}$s $\leq .64$; $.81 \leq$ compensated-r_{sb}s $\leq .88$. Thus, apart from the non-smokers' *Enjoy-Neg* IRAP effects, which almost completely lacked internal consistency, it appeared as though both the smokers and non-smokers responded consistently to each of the four IRAP trial-types.

Table 5.4

The Spearman-Brown split-half reliabilities (r_{sb}) for smokers' versus non-smokers' IRAP trial-type effects. 'Compensated D_{IRAP} r_{sb}s' approximated what r_{sb}s the various D_{IRAP}s would have if, with all else equal, they were comprised of the same number of trials as an IAT effect (for algorithm see Appendix 10).

	D_{IRAP} r_{sb}	Compensated D_{IRAP} r_{sb}
Smokers		
Enjoy-Pos D_{IRAP} [a]	.87****	96****
Enjoy-Neg D_{IRAP} [a]	.62**	.87****
Need-Pos D_{IRAP} [a]	.47*	.78***
Need-Neg D_{IRAP} [b]	.66**	.89****
Non-Smokers		
Enjoy-Pos D_{IRAP} [a]	.52*	.81****
Enjoy-Neg D_{IRAP} [a]	.13	.37[#]
Need-Pos D_{IRAP} [a]	.64**	.88****
Need-Neg D_{IRAP} [a]	.55**	.83****
Smokers & Non-Smokers		
Enjoy-Pos D_{IRAP} [c]	.73****	.92****
Enjoy-Neg D_{IRAP} [c]	--	--
Need-Pos D_{IRAP} [c]	.62****	.87****
Need-Neg D_{IRAP} [c]	.75****	.92****

[a] $n = 16$; [b] $n = 14$, [c] $n = 32$. [#] $p \leq .10$, * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$; **** $p \leq .0001$; all ps for r_{sb} one-tailed.

Table 5.5

Zero-order Pearson correlations among the four trial-type D_{IRAP}s.

	Enjoy-Pos D_{IRAP}	*Enjoy-Neg* D_{IRAP}	*Need-Pos* D_{IRAP}	*Need-Neg* D_{IRAP}
***Enjoy-Pos* D_{IRAP}**	--	.61 [a]****	.45 [a]**	.54 [a]***
***Enjoy-Neg* D_{IRAP}**		--	.50 [b]**	.67 [a]****
***Need-Pos* D_{IRAP}**			--	.43 [b]**
***Need-Neg* D_{IRAP}**				--

[a] $n = 32$; [b] $n = 31$. [#] $p \leq .10$, * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$; **** $p \leq .0001$; all ps for r_{sb} one-tailed.

It therefore seemed appropriate, as per the bottom panel of Table 5.4, to calculate internal reliabilities for the *Enjoy-Pos*, *Need-Pos*, and *Neg-Pos* trial-type effects across both smokers and non-smokers. Crucially, this indicated that the overall internal reliability of all three of these trial-type effects was relatively high, $.62 \leq r_{sb}\text{s} \leq$

.75, particularly when we compensated our estimates for the fourfold greater test-length typically held by IATs, $.87 \leq r_{sb}$s $\leq .92$. In addition, as illustrated in Table 5.5, the four *trial-type* D_{IRAP}s were inter-correlated to a large degree with each other thus indicating that they were highly consistent not just internally but also with each other.

5.4. DISCUSSION

Overall, the current IRAP appeared to improve upon the precision with which our first study's IRAP captured implicit evaluating that was integral to tobacco addiction. In particular, the current IRAP allowed us to determine that even when phrased in terms of stereotypical reasons for smoking, only certain types of implicit evaluating were integral to tobacco addiction. And that by contrast, other very similar types of implicit evaluating nevertheless appeared to be related to tobacco addiction in a mainly collateral fashion. We proceed now to review the various implications of these findings for the literature on smoking-related implicit cognition.

Smoking for Relief from Negative Affect as a Primary Motivator of Tobacco Addiction

All four IRAP trial-type effects confirmed our key known-groups prediction that smokers would be more mood-consistent than non-smokers in their implicit evaluating of reward- and relief-focused reasons for smoking (i.e. endorsing smoking for emotional reward during positive but not negative craving-related moods, and endorsing smoking for emotional relief during corresponding negative but not positive craving-related moods). Moreover, both smokers and non-smokers were decisive in this pattern of implicit evaluating insofar as both groups exhibited relatively high internal reliability, overall, across the current IRAP trial-types. In fact, the only trial-type effect to exhibit low internal reliability for either group was the non-smokers' *Enjoy-Neg* effect, and all this indicated was that the current non-smokers were relatively conflicted about denying that they enjoy smoking when feeling bad. Indeed, in any case, all four trial-type effects correlated at least moderately with all six of the continuous tobacco addiction criteria (i.e. YS, CPD, mFTQ, HONC, AIS and TC); and not only were the four *trial-type* D_{IRAP}s internally consistent for the most part, they were also highly consistent with each other insofar as all *trial-type* D_{IRAP} correlated to a large degree with every other one. Therefore, all four of the current IRAP trial-types appeared to be at least moderately related to tobacco addiction, and moreover, in a highly systematic fashion that cohered different (implicit) reasons for smoking depending upon positive versus negative craving-related moods.

Interestingly, the *Need-Neg* effect exhibited known-groups smoking-status differences that were far greater than any of the other three IRAP trial-types. This indicated that implicitly affirming one's need to smoke for relief from negative affect was a much more important aspect of tobacco addiction intensity than the other three types of implicit evaluating. Furthermore, the *Need-Neg* effect consistently correlated more strongly with tobacco addiction criteria than the three other trial-type effects. This was impressive given that the three relevant trial-types each correlated at least moderately with all six of the tobacco addiction criteria. Moreover, *Need-Neg* was the only trial-type to diagnose smoking-status with a high degree of sensitivity and specificity – and in particular, unlike its counterparts, *Enjoy-Pos* failed to diagnose smoking-status at all. Crucially, this suggested that *Need-Neg* was the only one of the current trial-types that could have been integral to tobacco addiction (i.e. insofar as it is not possible, by definition, for any measure to be integral to tobacco addiction without first being able to diagnose addicted smokers from among never-smokers).[53] Indeed, bearing this out, our mediational analyses indicated that *Need-Neg* was the only one of the current trial-types to exhibit an integral relationship with trait tobacco addiction intensity; and moreover, it did so to a large degree with all four trait criteria (i.e. CPD, mFTQ, HONC and AIS).

Overall, therefore, the current findings consistently supported the idea that implicitly evaluating smoking as being needed during negative craving-related moods was a primary motivator of tobacco addiction. And that, by contrast, implicitly affirming one's need to smoke while already experiencing craved positive moods, or implicitly affirming smoking as being enjoyable when one is feeling positive or negative craving-related moods, did not appear to motivate tobacco addiction to the same degree. Importantly, this pattern of findings is highly consistent with the traditional theoretical view that tobacco addiction gradually intensifies as a primary function of how much one evaluates smoking as being necessary to regulate negative moods resulting from tobacco withdrawal (i.e. rather than as a function of how much one evaluates smoking as being emotionally rewarding; see Baker et al., 2004; DiFranza, 2015; D'Souza, & Markou, 2011, p. 5; McCallion & Zvolensky, 2015).

[53] Note that in order for any measure to obtain a high degree of smoking-status sensitivity and specificity it is not enough for it to share a high degree of variance with smoking-status as per correlation. Rather it also requires that changes in the relevant measure are associated with a relatively high degree of change in smoking-status (*cf.* linear regression for the difference between non-standardized regression coefficients and *r* statistics). Crucially, this explains why *Enjoy-Pos*, *Enjoy-Neg* and *Need-Pos* correlated at a consistently moderate-to-large degree with tobacco addiction criteria but yet performed poorly in diagnosing smoking-status.

Reward-focused Implicit Evaluating as a Secondary Motivator of Tobacco Addiction

At first glance, the failure of the current reward-focused trial-types to exhibit integral relationships with tobacco addiction might appear to contradict the fact that the reward-focused *Smoking-Pos* effect from Study 1 was moderately integral to addiction intensity (i.e. CPD, mFTQ and HONC). However, the current trial-types incorporated a theoretically important conditionality that was not a part of the IRAP from Study 1. Namely, the previous study asked how smoking (or the smoking-ban) makes one feel, but in contrast the current IRAP was phrased in terms of asking under what (positive versus negative) craving-related mood conditions one finds smoking to be enjoyable versus needed. Crucially, IRAP research in other domains has shown that even very particular changes to the stimuli presented in an IRAP can impact quite dramatically and systematically upon the extent to which specific trial-types correlate with criterion variables (Nicholson & Barnes-Holmes, 2012; Remue, Hughes, De Houwer, & De Raedt, 2014).

Therefore, rather than contradicting our findings in Study 1, the current reward-focused trial-type findings served to qualify those earlier findings in potentially useful ways. Specifically, tobacco addiction may be implicitly motivated by smoking to increase positive affect as per our *Smoking-Pos* findings in Study 1, but crucially, the current IRAP data suggested that this likely only holds when one is not already experiencing particularly pleasing or displeasing moods. In other words, reward-focused implicit evaluating of reasons for smoking are perhaps most likely to be integral to tobacco addiction when smokers are going about routine aspects of their daily business and are not currently experiencing strong craving-related moods. Interestingly, this pattern of findings corroborated the fact that researchers have been relatively unsuccessful in inducing tobacco cravings with positive mood inductions (Heckman et al., 2013; Wray, Gass, & Tiffany, 2013). And moreover, it also corroborated the fact that smokers tend to explicitly cite reward-focused reasons for smoking only when not nicotine deprived (Veilleux et al., 2013), and/or in the context of performing mundane daily routines (Bancroft et al., 2003; Cook et al., 2004; Copeland et al., 1995; McEwen, West, & McRobbie, 2008; Vidrine, Vidrine, Costello, Mazas, Cofta-Woerpel, Mejia, & Wetter, 2009). Overall, therefore, the findings thus far suggest that smokers' implicit evaluating appears to motivate tobacco addiction in a highly systematic manner depending on the presence of positive versus negative craving-related moods.

Further Clarifications on the Involvement of Implicit Evaluating in Tobacco Addiction

Reward-focused Implicit Evaluating during Positive Mood as a Risk Factor for Initiating Tobacco Smoking

Reward-focused implicit evaluating may not be integral to tobacco addiction intensity during craving-related positive moods, but most theories of tobacco addiction strongly implicate reward-focused (implicit) evaluating during positive moods as being a primary motivator for non-smokers initiating smoking. For example, a classic explanation of why young smokers continue to smoke even before they are addicted is to gain popularity among their peers (Vahey et al., 2010). Indeed, more broadly, non-smoking adolescents' are characteristically prone to describe smoking as being an inherently celebratory activity (i.e. rewarding, discretionary, and as not ever having the potential to be needed for emotional relief; see Chassin et al., 2007; Fitz et al., 2015; Glautier, 2004; O'Connor et al., 2007; Vahey et al., 2010).

Interestingly, the current non-smokers' IRAP trial-type effects comported very closely with this complex pattern of characteristic evaluating. Namely, they implicitly evaluated smoking as being an enjoyable adjunct to positive moods (i.e. as per *Enjoy-Pos*), *and* did not implicitly evaluate it as being enjoyable or needed otherwise (i.e. as per the other three trial-types). And moreover, this was despite the fact that these non-smokers had not smoked within the past 12 months, or on more than one occasion previously. Therefore, the findings from all four of the current trial-types appeared to systematically converge on the idea that contexts involving craving-related positive affect may be a uniquely important risk factor for young non-smokers initiating smoking – and particularly, given that all four trial-type effects were at least moderate risk factors for smoking (i.e. as per criterion correlations).[54] Overall, therefore, the current IRAP findings were systematic in corroborating at an implicit level the prevailing non-implicit explanation for why young people begin smoking – namely that young (non-)smokers tend to view smoking as being an inherently celebratory activity. In fact, to our knowledge, this is the first time in the literature that any implicit measure has specifically corroborated this standard account at an implicit level (e.g., Larsen, Kong, Becker, Cousijn, Boendermaker, Cavallo, Krishnan-Sarin, & Wiers, 2014; see also Chassin et al., 2007; Glautier, 2004; Fitz et al., 2015; Vahey et al., 2010).[55]

[54] Note that we are not saying, for example, that *Enjoy-Pos* is necessarily integral to young non-smokers initiating smoking. Rather, *Enjoy-Pos* might be relatively collateral to young non-smokers initiating smoking and yet still be a risk factor for this process.

[55] In Study 1, the non-smokers' were equally able to sustain pro- versus anti-smoking perspectives on *Smoking-Pos* in the pro- and anti-smoking-first block order conditions, respectively – however, unlike the current findings, this did not identify anything about the circumstances under which non-smokers tend to implicitly evaluate from a pro-smoking perspective.

Implicit versus Explicit Evaluating of Reward- and Relief-focused Smoking

Known-groups Conflicts between Implicit versus Explicit Evaluating

We confirmed multiple known-groups conflicts and agreements between each groups' implicit versus explicit evaluating of the four trial-type topics. For instance, the non-smokers explicitly evaluated all four trial-type topics in an anti-smoking manner on both the feeling thermometers and the semantic differentials, but only implicitly evaluated two of the IRAP trial-types in an anti-smoking manner. More specifically, they implicitly evaluated *Enjoy-Neg* and *Need-Pos* in an anti-smoking manner, but were implicitly pro-smoking with respect to *Enjoy-Pos* and implicitly indifferent with respect to *Need-Neg* (i.e. passively indecisive). Crucially, we anticipated this pattern of non-smoker findings on the basis that to qualify for participation in the current study, the current non-smokers had to explicitly identify themselves as not having smoked during the previous 12 months or on more than one occasion before that. As such, to do anything thereafter but explicitly deny the topics dealt with by *Enjoy-Pos* and/or *Need-Neg* would have been socially stigmatized for the current non-smokers (i.e. regardless of their characteristic tendency to otherwise explicitly evaluate smoking during celebrations as being enjoyable; Chassin et al., 2007; Fitz et al., 2015; Glautier, 2004; O'Connor et al., 2007; Vahey et al., 2010). Moreover, we expected that the non-smokers would explicitly deny the topics dealt with by *Enjoy-Pos* and/or *Need-Pos* on the basis that it not only comported with their own characteristic mood-consistent evaluating of smoking (see above), but also directly comported with their having explicitly identified themselves as being committed non-smokers at the outset. Thus, overall, the current IRAP confirmed its ability to reveal pro-smoking aspects of non-smokers' evaluating that were obscured by the impact of social desirability bias on equivalent measures of explicit evaluating.

Furthermore, the current smokers explicitly evaluated the four trial-type topics in a broadly similar mood-consistent fashion as they implicitly evaluated those topics on the IRAP. In particular, we anticipated that this would be the case on the basis that smokers are not generally stigmatized, but rather are more likely to be socially accepted for offering the relevant reward- and/or relief-focused reasons for their smoking (see Chassin et al., 2007; Fitz et al., 2015; Vahey et al., 2010). Granted, two of the eight measures of the smokers' explicit evaluating indicated mood-inconsistent evaluating contrary to our expectations – namely, that the smokers explicitly affirmed both the *Enjoy-Neg* and *Need-Pos* topics on their respective semantic differentials. It was not clear exactly why the smokers explicitly endorsed smoking conditionally on the feeling thermometers but unconditionally on the semantic differentials. Crucially, however, this finding appeared to contradict not just our corresponding feeling thermometer findings, but also the tobacco addiction literature more broadly (see above). On balance, therefore, whatever the explanation for this discrepant subset of explicit evaluating,[56] it appeared to further underscore the idea that the IRAP is less prone to distorted measurement than corresponding questionnaire-based methods. Thus, overall, the current IRAP was successful in confirming multiple known-groups conflicts and/or agreements between each group's implicit versus explicit evaluating.

Exploring the Involvement of Implicit Evaluating in Explicit Evaluating

Two of the IRAP trial-type effects, *Enjoy-Neg* and *Need-Neg*, correlated with all eight of the explicit measures; but that the other two trial-type effects, *Enjoy-Pos* and *Need-Pos*, correlated with none of these eight explicit measures. In particular, *Enjoy-Neg* and *Need-Neg* consistently correlated with explicitly endorsing smoking as being both unconditionally rewarding and unconditionally relieving.[57] That is, the *Enjoy-Neg* and *Need-Neg* effects not only correlated positively with explicitly affirming the *Enjoy-Pos* and *Need-Neg* topics, but they each also correlated negatively with explicitly denying the *Enjoy-Neg* and *Need-Pos* topics. In other words, crucially, the current participants' (reward- and relief-focused) explicit evaluating of smoking appeared to be driven rather exclusively by implicit evaluating that smoking is needed (as per *Need-Neg*) and not enjoyed (as per *Enjoy-Neg*) during negative craving-related moods. Notably, such findings further emphasize the possibility of identifying what types of implicit evaluating are particularly likely to interfere with (or strengthen) a smoker's ongoing explicit evaluating about whether to continue smoking or not. And thus, the current pattern of implicit-explicit correlations provided even further corroboration of the idea that tobacco addiction gradually intensifies as a primary function of how much

[56] One possible explanation was that the current smokers responded in this way as a spurious response heuristic to simplify and thus expedite their completion of the twelve semantic differentials. Indeed, by the time the smokers were completing the twelve semantic differentials they had already completed at least 192 very similarly formatted IRAP trials. As such, the smokers may have been reluctant to fully attend to the semantic differentials because of fatigue with respect to such tasks formatted like IRAP trials (e.g. including response options that randomly alternated their left-right positioning). Certainly, this interpretation would explain why the smokers' feeling thermometers were not similarly affected – only four feeling thermometers were presented (directly after the IRAP); and they had a novel, simpler response format as compared to the IRAP trials (and the corresponding semantic differentials).

[57] Note that this further supports the idea put forward in the previous sub-section that the current smokers were merely employing a post hoc heuristic when they explicitly evaluated in an unconditionally pro-smoking fashion on the semantic differentials.

one (implicitly) evaluates smoking as being necessary to regulate negative (and not positive) craving-related moods.[58]

The IRAP Block Order Variable as a Measure of Perspective-switching Tendencies

The *Enjoy-Pos* effect was the only one of the current trial-types to exhibit an interaction between smoking-status and block order, and it was an almost perfect cross-over interaction. Specifically, whereas the non-smokers' *Enjoy-Pos* effects were influenced in line with block order (i.e. more mood-consistent in the mood-consistent-first condition than in the mood-inconsistent-first condition), interestingly, block order influenced the smokers' *Enjoy-Pos* effects in the opposite direction. Crucially, as follows, this pattern of block order effects comported closely with the characteristically opposing patterns of perspective-switching that both groups were known to be characteristically accustomed to.

As per our discussion of our findings in Study 1, participants should only respond in line with block order on a given trial-type to the extent that they are similarly (un)accustomed to adopting each of the two opposing evaluative responses it targeted in separate respective contexts (e.g. 'I ENJOY SMOKING WHEN I'm Relaxed – *True* versus *False*'). In particular, it is well known fact that young non-smokers tend to adopt perspectives affirming that smoking is enjoyable when they are feeling good (e.g. as when celebrating with pro-smoking peers), but that they are also at least somewhat accustomed to switching to perspectives that deny this *as required* by other contexts (e.g. as when celebrating with anti-smoking peers or authority figures; see Chassin et al., 2007; Dal Cin et al., 2007; Fitz et al., 2015, p. 441; O'Connor et al., 2007; Pfizer Ireland, 2007; Piontek, Buehler, Rudolph, Metz, Kroeger, et al., 2008). Therefore, it was closely in line with the current non-smokers' known-groups tendency for perspective-switching that they exhibited a mood-consistent (i.e. pro-smoking) *Enjoy-Pos* effect in both block order conditions, but less so in the mood-inconsistent-first condition.[59]

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[58] Indeed, bearing this interpretation out even further, post hoc analyses tentatively suggested that *Enjoy-Neg* might have correlated with explicit evaluating only as a function of *Need-Neg* – the one trial-type here that directly addressed the relevant process of tobacco addiction, smoking for relief from negative moods. In particular, *Need-Neg* correlated with all eight explicit measures to a consistently larger degree than *Enjoy-Neg* did; and moreover, the correlation between *Enjoy-Neg* and *Need-Neg* was strong, and indeed stronger than that between any other pairing of the current trial-types. In other words, in lieu of a more systematic experimental treatment, it tentatively appeared as though *Need-Neg* was simultaneously driving both *Enjoy-Neg* and the eight explicit measures so that they correlated with each other but did not necessarily interact with each other.

[59] Incidentally, the non-smokers' *Enjoy-Pos* effect was just as pro-smoking as the smokers' large *Enjoy-Pos* effect in the mood-consistent-first block order condition, if not more so, but far less pro-smoking than the smokers on all other combinations of the current trial-types with block order. Crucially, this pattern of

By contrast, the current smokers smoked daily and had not even contemplated restricting their smoking within the previous 12 months – and as such, they were not only characteristically inclined to adopt perspectives that affirm smoking as being enjoyable when one is feeling good (see Cook et al., 2004; Copeland et al., 1995; DiFranza et al., 2012; DiFranza, 2015; Fitz et al., 2015; Glautier, 2004; McEwen et al., 2008; Pfizer Ireland, 2007; Vidrine et al., 2009), but were also characteristically resistant to adopting any perspectives denying this (McCallion & Zvolensky, 2015; Moss, Erskine, Albery, Allen, & Georgiou, 2015; Rhodes, Roskos-Ewoldsen, Edison, & Bradford, 2008; Sayers & Sayette, 2013; Schueller, Pérez-Stable, & Muñoz, 2013). In principle, therefore, the current smokers were not only pre-inclined to respond to *Enjoy-Pos* in line with the mood-consistent-first instructions, but also pre-inclined to (re)actively resist the mood-inconsistent-first (i.e. anti-smoking) instructions on this trial-type in particular. And crucially, this is just what we found. Namely, the smokers' *Enjoy-Pos* effect was more pro-smoking in the mood-inconsistent-first condition than the mood-consistent condition, even though the former encouraged anti-smoking perspectives and the latter encouraged pro-smoking perspectives. Overall, therefore, the cross-over interaction between smoking-status and block order on *Enjoy-Pos* supported the idea that it was measuring perspective-switching tendencies.

Furthermore, as follows, the fact that the remaining three trial-types did not exhibit any interactions between block order and smoking-status was also in line with the idea that block order was measuring perspective-switching tendencies. Firstly, smokers and (young) non-smokers are well known for having mood-consistent beliefs about enjoying smoking such that they would deny *Enjoy-Neg* (see above), but crucially, it is not a characteristic of either group that they should ever switch perspectives to affirm *Enjoy-Neg* in a sustained fashion. Indeed, insofar as enjoyment is generally incompatible with feeling bad, it is hard to imagine any situation in which either group would maintain a perspective that involved affirming smoking (or anything else) as being enjoyable when one is feeling bad.[60] Moreover, neither group was

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block order effects further corroborates the previous section's findings that non-smokers' implicit evaluating is particularly likely to put them at risk of initiating smoking while they are celebrating.

[60] Smokers may sometimes report enjoying euphoric relief when they smoke while experiencing unusually severe cravings (see Cook et al., 2004; Glautier, 2004; Henningfield, Miyasato, & Jasinski, 1985; Pillitteri, Kozlowski, Sweeney, & Heatherton, 1997; Toll, Schepis, O'Malley, McKee, & Krishnan-Sarin, 2007). However, by definition, such situations are unusual, fleeting and moreover markedly different from the current measurement context (i.e. the current smokers were not nicotine deprived; also see Bancroft et al., 2003). It was therefore rather unlikely that they (or non-smokers) were prepared to sustain any perspective on *Enjoy-Neg* that was in line with the mood-inconsistent-first instructions. Granted, the non-smokers were actively indecisive on *Enjoy-Neg* (i.e. low internal reliability) such that their *dominant* tendency to deny *Enjoy-Neg* (i.e. an anti-smoking perspective) was intermittently

characteristically accustomed to resist affirming *Enjoy-Neg*, thus ruling out any likelihood of block order rebound effects. As such, the current pattern of block order effects on *Enjoy-Neg* was consistent with both groups' respective known-groups perspective-switching capacities.

In addition, we have already outlined that smokers are well-known for evaluating that they strictly only need to smoke during certain negative moods and not when they are already experiencing positive moods. As such, the current smokers were not only characteristically pre-inclined to respond to *Need-Pos* from mood-consistent perspectives (i.e. denying it), but also characteristically unaccustomed to switching from perspectives denying *Need-Pos* to any affirming it. Crucially, therefore, the current smokers were well prepared to respond to *Need-Pos* from perspectives in line with the mood-consistent-first instructions, but not from perspectives in line with the mood-inconsistent-first instructions. Moreover, the current smokers were probably unaccustomed to rationally defending their tendency to deny *Need-Pos* (i.e. given that their non-smoker peers tend to view smoking as being relatively discretionary) – and as such, they were unlikely to exhibit any rebound effect on *Need-Pos* during the mood-inconsistent-first condition. The fact that block order did not moderate either the smokers' or the non-smokers' *Need-Pos* effects was therefore highly consistent with the idea that the IRAP block order variable measures perspective-switching tendencies.

Furthermore, (young) non-smokers are characteristically pre-inclined to evaluate smoking as being both discretionary and enjoyable in celebratory contexts (i.e. even if they are otherwise disinclined to recommend smoking; see above). And as such, the current non-smokers were accustomed to adopting perspectives that deny *Need-Pos*, and unaccustomed to adopting perspectives that either affirm affirming *Need-Pos*. Moreover, insofar as it is unusual for anyone (smokers or non-smokers) to affirm *Need-Pos*, it follows that non-smokers are characteristically unlikely to ever be confronted with having to rationally justify their tendency to deny *Need-Pos* (i.e. as means of resisting others' perspectives affirming *Need-Pos*). Thus, overall, the fact that the current non-smokers' exhibited similarly mood-consistent *Need-Pos* effects in both block order conditions closely confirmed the current rationale.

Likewise, in relation to the *Need-Neg* effects, the only reason that (young) non-smokers typically say that they might smoke is to enhance positive mood during

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interfered with by some opposing *recessive* perspective(s) affirming *Enjoy-Neg*. However, if anything, this further corroborated the current non-smokers' difficulty sustaining switches between the perspectives dealt with by *Enjoy-Neg* (i.e. insofar as they were not even able to reliably sustain their dominant perspective to deny *Enjoy-Neg*, much less switch to any recessive perspective affirming *Enjoy-Neg*).

(certain) celebrations – and as such, the current non-smokers were characteristically unaccustomed to either affirming or denying reasons that they might smoke during negative moods (see Chassin et al., 2007; DiFranza et al., 2012; DiFranza, 2015; Fitz et al., 2015; Glautier, 2004; O'Connor et al., 2007; Vahey et al., 2010). And crucially, the current findings comported very closely with this known-groups criterion insofar as the current non-smokers exhibited a passively indecisive *Need-Neg* effect (i.e. internally reliable, in the neighbourhood of zero, and not moderated by block order), indicating that they were indifferent to this topic.

However, as daily smokers who had not restricted their smoking within the previous 12 months, the current smokers were not only characteristically inclined to affirm *Need-Neg* but probably also well accustomed to rationally defending this perspective. In particular, it is common for smokers to be confronted by non-smokers who typically dispute one's need to smoke (i.e. because they view smoking as discretionary; Chassin et al., 2007; DiFranza et al., 2012; DiFranza, 2015; Fitz et al., 2015; Glautier, 2004; O'Connor et al., 2007; Vahey et al., 2010). Therefore, the current smokers should have been characteristically pre-inclined not only to affirm *Need-Neg*, but perhaps also to actively resist any perspectives denying *Need-Neg* (i.e. as per the current mood-inconsistent-first instructions). Confirming this, the current smokers produced a positive *Need-Neg* in both block order conditions, and exhibited a strong trend towards *Need-Neg* being more positive in the mood-inconsistent-first condition. Indeed, the relevant trend was sufficiently large that it generated a main effect for block across smoking-status even though the non-smokers' *Need-Neg* was clearly unaffected by block order (see Figure 5.1).

To complicate matters, however, the impact of block order on the smokers' *Need-Neg* was not sufficiently large to generate a smoking-status by block order interaction (i.e. particularly given that the non-smokers did not exhibit an opposing block order effect like they did with *Enjoy-Pos*; see Figure 5.1). Thus, technically, we were disqualified from acknowledging the large simple effect of block order on the current smokers' *Need-Neg* scores (see Figure 5.1). And yet, it would have been equally inappropriate to interpret the current lack of a legitimate block order rebound effect on the smokers' *Need-Neg* as being evidence against the idea that IRAP block order measures perspective-switching. In particular, that would be to ignore the highly consistent pattern of known-groups perspective-switching effects we have reported so far despite relatively small block order condition sample sizes in both studies (ns = 8-12; i.e. which would have underestimated rather than inflated our ability to detect such a consistent pattern of known-groups patterns; see Bachetti, 2013; LeBel & Paunonen, 2011; Shoukri, Asyali, & Donner , 2004; Sidman, 1960; Quinlan, 2013; Wainer & Thissen, 2001; Webb et al., 2007, p. 87).

Overall, therefore, the smokers' ironic *Need-Neg* block order effect provided tentative additional support for the idea that the IRAP block order variable measures one's perspective-switching tendency. And furthermore, insofar as this was the case, the smokers' ironic *Enjoy-Pos* and *Need-Neg* block order effects may have succeeded in directly measuring an instance of *repressive coping* for the first time. Repressive coping is broadly conceptualised as being a relatively automatic counterpart to thought suppression, whereby individuals strategically contradict and/or dissociate from evaluative perspectives that they typically perceive as being aversive (e.g. as when a smoker experiences tobacco cravings during abstinence, or an anti-smoking message while smoking; see Myers 2010; Moss et al., 2015). There are decades of research implicating repressive coping (and thought suppression) as playing central roles in (tobacco) addiction, and indeed in psychopathology more broadly (e.g. Myers, 2010). In particular, repressive coping (and thought suppression) is likely to hamper adaptive behavioural change to the extent that it makes one less sensitive, and thus less adaptive to changing contingencies in one's environment (see Chawla, N., & Ostafin, 2007; Geraerts, Merckelbach, Jelicic, & Smeets, 2006; Hayes, 1993; Hayes, Wilson, Gifford, Follette, & Strosahl, 1996; Moss et al., 2015; Törneke, Luciano, & Valdivia-Salas, 2008; for related ethical issues see also Kjærsgaard, 2015). Indeed, confirming this repressive coping (and thought suppression) predict both poor mortality and long-term health outcomes (Geraerts et al, 2006; Myers, 2010).

Crucially, however, this literature has relied exclusively upon anomalies in questionnaire-based measures, and as such it has provided only indirect evidence of repressive coping. Thus, repressive coping has remained a relatively nebulous even if well-established concept, with little if any research distinguishing between what particular automatic processes might be involved in it. Certainly, the literature has yet to examine repressive coping in terms of implicit evaluating[61] – with the most likely reason being that measures of implicit evaluating were not equipped to distinguish one type of complex implicit evaluating from another, nor thus one aspect of repressive coping from another, until the advent of the IRAP. The IRAP block order variable, as a

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[61] Granted, some studies of repressive coping have employed rudimentary measures of psycho-physiological arousal – however, these measures were not interpreted as instances of repressive coping but rather as evidence of whatever evaluating was supposedly being automatically repressed from corresponding questionnaires (see Myers, 2010; Moss et al., 2015).

measure of perspective switching tendencies, may therefore be a promising means of revealing key processes in addiction and psychopathology more broadly by revealing key processes involved in repressive coping and thought suppression. In particular, there is emerging evidence that repressive coping is a more advanced form of thought suppression that is more effective in eliminating unwanted (smoking-related) evaluative perspectives from an individual's ongoing experiences (see Geraerts et al., 2006; McGreevy, Bonanno, & D'Andrea, 2015; Moss et al., 2015; Myers, 2010; see also Brandt, Bakhshaie, Garey, Schmidt, Leventhal, & Zvolensky, 2015).

On balance, however, it is important to acknowledge that the current study was not specifically designed to reveal the aforementioned known-group differences in the extent to which trial block order interacted with smoking-status among the four IRAP trial-types. Rather as one of the first smoking-related IRAP studies, it was primarily concerned with establishing whether the absolute size of its trial-type effects would covary with various criterion variables for tobacco addiction (e.g. such as smoking-status, CPD or years smoking). Therefore, regardless of how systematic the foregoing IRAP trial block order effects appeared to be, it is important to remain sceptical about whether they measured one's tendency to perspective switch on each trial-type's evaluative topic – particularly given the relatively modest sample size of the current study and the rather borderline smoking-status by block order interaction obtained on *Need-Neg* D_{IRAP}. In other words, we must specifically sample and/or experimentally manipulate variables related to perspective switching on a given trial-type topic, before we can firmly establish whether IRAP block order measures a person's tendency to perspective switch (or thus repress) their implicit response to that trial-type topic.

Conclusions

The foregoing network of findings is perhaps most valuable from the point of view that it revealed major systematic differences between the mood contexts likely to occasion non-smokers to begin smoking versus those that implicitly motivate tobacco addiction thereafter. In summary, we found that non-smokers are likely to be rather uniquely at risk of initiating smoking in contexts involving the enjoyment of positive craving-related moods, but that in contrast, such contexts do not appear to motivate an intensification of tobacco addiction (i.e. such contexts already involve the positive moods that addicted smokers stereotypically crave). And that in contrast, tobacco addiction is likely to be primarily motivated by contexts involving some implicit need for relief from craving-related negative affect; and that in the absence of both positive and negative moods, tobacco addiction is likely to be secondarily motivated by contexts that involve implicitly evaluating smoking as a means of achieving stereotypically-craved positive moods. Crucially, the key point here is that with just two IRAPs, we were able to reveal that smokers and non-smokers both exhibited highly coherent and distinctive networks of implicit evaluating with respect to smoking for mood regulation. Namely, that the mood contexts which are likely to implicitly motivate tobacco addiction among established smokers are characteristically different from those which are likely to be instrumental in implicitly motivating young people to begin smoking in the first place.

In addition, these findings offered a plausible means of reconciling multiple contradictory points of view in the tobacco addiction literature. Unlike most dominant (implicit) theories of tobacco addiction which argue about whether tobacco addiction is driven by reward- versus relief-focused (implicit) evaluating (see Conklin et al., 2004; Everitt & Robbins, 2005; Farris et al., 2015; Robinson & Berridge, 2008; Stacy & Wiers, 2010), the current findings systematically allowed for both possibilities depending upon mood context. Namely, the current findings alerted us to the possibility that tobacco addiction is alternately motivated by *both* reward- and relief-focused in different respective contexts. Indeed, whatever about such findings being at the fringe of the questionnaire-based tobacco addiction literature (DiFranza et al., 2012; DiFranza, 2015; Köpetz et al., 2013; Fitz et al., 2015; Hendricks & Brandon, 2008; Hendricks, Wood, Baker, Delucchi, & Hall, 2011; Spada et al., 2015; Wetter et al., 1999; Wray et al., 2013), they are almost entirely unprecedented in the literature dealing with smoking-related implicit cognition (for the only other example we are aware of see McKee et al., 2003; and for concordant reviews see Hendricks & Brandon, 2008; Jajodia & Earleywine, 2003; Rooke et al., 2008; Tibboel et al., 2007; Watson et al., 2012; Stacy & Wiers, 2010).

As such, overall, it therefore appeared as though the two IRAP's we have used so far were a precise means of determining how implicit evaluating motivates tobacco addiction. And yet, even though the current research modelled the impact of YS on various gold-standard criteria for tobacco addiction the fact still remains that our findings were cross-sectional with respect to existing criteria for tobacco addiction. Crucially, in order to discover more precise understandings of tobacco addiction with regard to its treatment there is no substitute for experimental research that systematically develops new criteria by qualifying old criteria ever more precisely (e.g. DiFranza, 2010; Gifford & Humphreys, 2007, pp. 356-358; Tønnesen, 2009, p. S22; Tiffany, 2008; Tiffany et al., 2004; Wiers et al., 2010; Wray et al., 2013). Thus, for the remainder of the current thesis we focused upon conducting experimental research that would provide an ever more precise understanding of how implicit evaluating differentially motivates tobacco addiction depending upon context. In particular, we sought to determine not just how the relevant implicit evaluating perspectives vary depending upon context, but also how one might change these patterns via learning processes as a means of treating tobacco addiction (i.e. as we outlined at the end of Chapter 3; see also De Houwer, Barnes-Holmes, & Moors, 2013; Hayes & Brownstein, 1986; Whelan & Barnes-Holmes, 2010, pp. 347-350).

CHAPTER 6: A First Systematic Test of Thought Suppression as a Means of Bringing Problematic Implicit Evaluating Under Self-control (Studies 3 & 4)

6.1. GENERAL INTRODUCTION: CHAPTER 6

Studies one and two were primarily concerned with providing a first test of the IRAP as a means of distinguishing between smoking-related implicit evaluating that was correlated integrally with tobacco addiction versus not. One of our most important findings from these preliminary studies was that tobacco addiction appeared to be motivated in a very systematic context-dependent manner by implicit evaluating. Not only did we find cross-sectional evidence that smokers (and non-smokers) implicitly evaluate smoking differently depending upon their perceived mood. We also found preliminary experimental evidence, in the form of IRAP trial block order effects, that smokers (and non-smokers) implicitly evaluate smoking differently depending upon whether they are cued to relate from the overarching perspective of a smoker versus a non-smoker. In particular, we found that non-smokers were generally inclined to implicitly evaluate smoking (and the Irish smoking-ban) in line with whichever perspective they were cued to adopt by block order; but that smokers were not. In fact, the smokers even appeared to repress (i.e. automatically suppress) those block order instructions that most directly contradicted implicit evaluating integral to tobacco addiction (i.e. during the mood-inconsistent-first instructions on the *Enjoy-Pos* and *Need-Neg* trial-types).

Thus, insofar as smokers were disinclined to switch to perspectives contradicting any type of implicit evaluating that was integral to tobacco addiction, it followed by definition that these smokers were unlikely to be able to deliberately suppress any such integral implicit evaluating for long. In particular, the very fact that smokers were disinclined to switch perspectives on any of the relevant IRAP trial-types implied that they would be relatively disinclined to continue (deriving) the relevant (implicit) anti-smoking perspectives once the typically temporary cues for doing so had passed (see Geraerts et al., 2006; Moss et al., 2015; Hughes, Barnes-Holmes, & Vahey, 2012, pp. 24-34; and particularly among young smokers; Wiers, Boelema, Nikolaou, & Gladwin, 2015). Crucially, this tallies well with the fact that there is an extensive questionnaire-based evidence demonstrating that smokers' attempts at thought suppression of pro-smoking perspectives not only fail to persist (particularly under cognitive load), but that they often lead to ironic increases in tobacco cravings and even smoking (Erskine, Georgiou, & Kvavilashvili, 2010; Erskine, Ussher, Cropley, Elgindi, Zaman, & Corlett, 2012; Moss et al., 2015, pp. 66-67; Sayers & Sayette, 2013; Schueller et al., 2013). Moreover, those who smoke as a means of suppressing aversive aspects of their ongoing evaluating tend to be more addicted to smoking than those who tend not to (see Erskine et al., 2010; Erskine et al., 2012; Farris et al., 2015; Lee, An, Levin, & Twohig, 2015; McCallion & Zvolensky, 2015; Moss et al., 2015; Toll, Sobell, Wagner, & Sobell, 2001). Indeed, more broadly, in his final review of the thought suppression literature, its originator and main protagonist Daniel Wegner concluded that "The apparent inevitability of thought suppression failure suggests that it is useful to explore strategies people could use…to escape their unwanted thoughts without recourse to thought suppression" (Wegner, 2011, p. 672). And accordingly, many of the alternative strategies that Wegner (2011) reviewed, such as Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 2011), are fundamentally designed to teach individuals how to disengage from unhelpful patterns of thought suppression broadly termed *experiential avoidance* (see Chawla & Ostafin, 2007; Hayes et al., 1996; McCallion & Zvolensky, 2015; Moss et al., 2015, pp. 67-68; Strosahl et al., 2004; Wolgast, Lundh, & Viborg, 2013).

Crucially, however, some of the relevant authors have raised the possibility, at least in passing, that thought suppression strategies might sometimes be useful in certain limited situations as long as they do not conflict with behaving in the ways one values (see Geraerts et al., 2006; Hayes, Strosahl, & Wilson, 2011, pp. 73-74; Kashdan, Barrios, Forsyth, & Steger, 2006, pp. 1301-1302; Van Dijk, 2009, pp. 102-104; Wegner, 2011; Wilson & DuFrene, 2009, p. 49; but see Hooper, Stewart, Duffy, Freegard, & McHugh, 2012). For example, thought suppression may sometimes be useful to abstinent smokers during particularly acute tobacco cravings as a means of temporarily resisting smoking until he/she can sufficiently remind themselves of the core reasons why they quit smoking in the first place (cf. Bloom, Matsko, & Cimino, 2014; Brown et al., 2005; Cox et al., 2006; Dawkins, Powell, Pickering, Powell, & West, 2009; Whelan, Conrod, Poline, Lourdusamy, Banaschewski, Barker, et al., 2012; McCallion & Zvolensky, 2015; Moss et al., 2015). Indeed, the literature on repressive coping suggests that with practise any such inhibitory strategies are likely to become more efficient in serving protective functions (i.e. requiring less deliberation and thus less likely to provoke rebound effects; see Bonanno, Papa, Lalande, Westphal, & Coifman, 2004; Coifman, Bonanno, Ray, & Gross, 2007; Litvin, Kovacs, Hayes, & Brandon, 2012;

Moss et al., 2015; Myers, 2010; also cf. Robusto, Cristante, & Vianello, 2008, pp. 955, 959; Verschuere, Spruyt, Meijer, & Otgaar, 2011).

And yet, apart from citing exceptional cases where thought suppression strategies might in principle be helpful, to date there has been disappointingly little questionnaire-based research examining the relative efficacy of different thought suppression strategies by context (see Aldao, 2013; Aldao & Nolen-Hoeksema, 2012, pp. 3-4; Bonanno & Burton, 2013; Sensibaugh, 2014; Wegner, 2011, p. 672; Wenzlaff & Wegner, 2000, pp. 73-74; Wolgast et al., 2013; see also Gross, 2015; Myers, 2010, pp. 6, 9; Najmi et al., 2007, p. 1963; Sheeran et al., 2013; Webb, Miles, & Sheeran, 2012; Webb, Sheeran & Pepper, 2012). Moreover, to our knowledge, only two studies have examined the impact of thought suppression on implicit evaluating, and neither was related to (tobacco) addiction (i.e. Hooper et al., 2010; Ritzert, Forsyth, Berghoff, Barnes-Holmes, & Nicholson, 2015; also cf. Liefooghe & De Houwer, 2015; Moss et al., 2015). In fact, the first of these two studies did not specify what thought suppression strategy its participants should use, and crucially, nor did it measure any implicit evaluating related to the content that participants were instructed to suppress evaluating about (Hooper et al., 2010). And although the other study, by Ritzert et al. (2015), did coordinate the target of their thought suppression instructions with what type of implicit evaluating they measured, the relevant thought suppression strategy was specifically designed not to change that implicit evaluating (see p. 114). Namely, rather than instructing participants to challenge the content of the relevant intrusive evaluating, Ritzert et al. instructed participants to temporarily distract themselves from it with an *unrelated* task delivered well before, rather than during measurement of the relevant implicit evaluating (i.e. unlike the timeframe within which thought suppression effects are typically captured; see Wenzlaff & Wegner, 2000; see also Wegner, 2011, pp. 672-673).[62]

[62] There are of course many studies that measure the efficacy with which individuals can inhibit established biases in their evaluative responding, using (confounded) methods such as the Addiction Stroop test (Cox et al., 2006; see Chapter 1), or the Sheffield lie test (Debey, Ridderinkhof, De Houwer, De Schryver, & Verschuere, 2015). Crucially, however, this literature has not examined the impact of thought suppression on implicit evaluative biases, but rather it has focused upon measuring how long it takes an individual to inhibit the expression of pre-established implicit evaluative biases (e.g. see Whelan, et al., 2012; Liefooghe & De Houwer, 2015). In addition, apart from that literature, we are aware of only four other studies that have incidentally examined concepts related to thought suppression using measures of implicit evaluating. However, none of these studies involved instructing participants to engage in thought suppression. In fact, two of these studies were specifically designed to circumvent the relevant deliberative processes by using cognitive training techniques to induce novel implicit evaluating perspectives as a means of automatically contradicting some problematic aspect of implicit evaluating (Deutsch, Gawronski, & Strack, 2006, p. 400; Gawronski, Deutsch, Mbirkou, Seibt, & Strack, 2008; see also *repressive coping*; Moss et al., 2015). Finally, the remaining study, by Krank et al. (2010), may have

Granted, the thought suppression literature has always focused upon implicit aspects of evaluating insofar as it sought to examine how successfully individuals could suppress *intrusive* aspects of their own evaluating like smokers' tobacco cravings (i.e. that enter one's awareness unintentionally, somewhat uncontrollably, and to an aversive extent). However, in all such cases, the relevant implicit evaluating was inferred not from measures of implicit evaluating per se, but instead based exclusively upon participants' explicit self-reports of how much they noticed themselves evaluating in an inadvertent and/or uncontrollable fashion about some topic (for reviews see Moss et al., 2015; Myers, 2010; Wenzlaff & Wegner, 2000). Indeed, the literature on thought suppression has yet to the examine the impact of specific thought suppression strategies as distinct from each other, preferring instead to simply instruct research participants to suppress intrusive aspects of their evaluating in whatever way they prefer on that occasion (see Erskine et al., 2015, pp. 1-3; Sensibaugh, 2014; Wenzlaff & Wegner, 2000, pp. 73-74; see also Myers, 2010, p. 6; Najmi et al., 2007, p. 1963).

This lack of systematic research is all the more remarkable when one considers that thought suppression is traditionally the most commonly proposed strategy offered for treating (tobacco) addiction in the literature on addiction-related implicit cognition, and also within the wider (neuro-)cognitive literatures addressing addiction (i.e. including via medication; see Abrams et al., 2003; Brandon et al., 2004; Fujita, 2011; Gifford & Humphreys, 2007; Hayes et al., 1996; Heatherton & Wagner, 2011; Heyman, 2009; Hughes, 2008; Moss et al., 2015; Stead et al., 2012; Volkow, Fowler, & Wang, 2004; Wiers, Gladwin et al., 2013; Wiers, Houben et al., 2010; Wiers & Stacy, 2006a). Indeed, more specifically, one of the most popular professional tactics for treating tobacco addiction is to advise smokers to deliberate about scenarios that make them feel like a non-smoker in order to "resist" and/or "overcome" their cravings by direct contradiction (Abrams et al., 2003; Brandon et al., 2004; Fujita, 2011; Friese et al., 2011; Heatherton & Wagner, 2011; Hofman et al., 2012; O'Connell et al., 2006; Roefs et al., 2011; Wiers et al., 2010). Likewise, most cognitive theories view (tobacco) addiction as being primarily an issue of not being able to sustain pro-abstinence/anti-addiction rumination extensively enough to permanently contradict, and thus prevent pro-smoking/anti-abstinence implicit evaluating from interjecting to motivate addictive

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engaged deliberative processes by confronting participants with written statements that labelled their usual alcohol-related expectancies as myths. However, in doing so, Krank et al. made no attempt to experimentally determine what those deliberative processes were. Thus, not only was it unclear how these three studies related to thought suppression, insofar as all three studies used confounded measures of implicit evaluating it was also unclear how these three studies related to implicit evaluating (as per our reviews of the relevant implicit measures in Chapters 1-3).

behaviour (see concepts such as *ego/cognitive depletion*, *cognitive efficiency*, and *dual processes* of cognitive control; Baumeister & Vonasch, 2015; Friese et al., 2011, p. 337; Fujita, 2011; Hoffman, Vohs & Baumeister, 2012; Wiers, Houben et al., 2010; Spada et al., 2015; Stacy & Wiers, 2010; Wiers, Gladwin et al., 2013; Tiffany, 1990, 2008). Even putting this aside, contradiction-based thought suppression is one of the primary methods used by smokers who attempt to quit smoking unaided (Baumeister, Heatherton, & Tice, 1994; Farris et al., 2015; Lee et al., 2015; McCallion & Zvolensky, 2015; O'Connell, Gerkovich, Bott, Cook, & Shiffman, 2002; O'Connell, Hosein, Schwartz, & Leibowitz, 2007, p. 79; Najmi, Reese, Wilhelm, Fama, Beck, & Wegner, 2010; O'Connell, Hosein, & Schwartz, 2006; Salkovskis & Reynolds, 1994; Sayers & Sayette, 2013).

In sum, therefore, we deemed it a major priority to examine how thought suppression instructions specifically designed to contradict implicit evaluating integral to tobacco addiction, would impact that implicit evaluating. Crucially, insofar as different treatments for tobacco addiction involve different thought suppression strategies, this type of component process research is a fundamental prerequisite for systematically refining any such treatments (e.g. see Abrams et al., 2003, pp. 21-23; De Houwer, 2011; De Houwer, Barnes-Holmes & Barnes-Holmes, 2015; Kapson et al., 2012; Kazdin, 2007; Gifford & Humphreys, 2007; Levin et al., 2011; Liefooghe & De Houwer, 2015; Murphy et al., 2009; Roefs et al., 2011, pp. 185-186; Vahey & Whelan, 2015). To this end, the current chapter contains two experiments, testing thought suppression instructions specifically designed to contradict the addiction-integral implicit evaluating observed in Study 1 on the reward-focused *Smoking-Pos* trial-type; and the other testing thought suppression instructions designed to contradict the addiction-integral implicit evaluating observed in Study 2 on the relief-focused *Need-Neg* trial-type. As such, unlike the extant literature, the current research examined the impact of thought suppression on its intended target in a much more precise way than the extant literature – which has typically instructed its participants merely to "avoid thinking" and/or "stop thinking" about some broad topic of concern such as smoking without measuring (any particular aspect of) implicit evaluating (e.g. Erskine et al., 2015; Wegner, Schneider, Carter, & White, 1987, p. 6; Wenzlaff & Wegner, 2000). And in effect, therefore, the current research sought to provide a first systematic examination of how thought suppression impacts any problematic aspects of (smoking-related) implicit evaluating it contradicts (see Hooper et al., 2010; Hooper et al., 2010, pp. 234, 239; Moss et al., 2015; Myers, 2010; Najmi et al., 2007, p. 1963; Rhodes et al., 2008; Spada et al., 2015; Wenzlaff & Wegner, 2000).

6.2. INTRODUCTION (STUDY 3)

Testing Thought Suppression as a Means for Smokers under Cognitive Load to Deliberately Control Implicit Evaluating Integral to Tobacco Addiction

In brief, the first experiment, Study 3, involved instructing smokers to continually deliberate, throughout the IRAP described in Study 1 (i.e. the *unconditional-feelings-IRAP*), about any familiar scenarios that helped them to maintain the stereotypically contrary perspective of a lifelong non-smoker. This strategy was designed to contradict the strongly pro-smoking *Smoking-Pos* D_{IRAP} exhibited by smokers in Study 1, in the broadest possible terms. Crucially, our primary aim in doing so was to ensure that we did not inadvertently rule out smokers' favourite, and thus perhaps most personally effective, strategies for contradicting their reward-focused tobacco cravings (i.e. particularly given that this was the first research of its kind; for relevant theoretical concerns see Aldao, 2013; Aldao & Nolen-Hoeksema, 2012, p. 4; Bonanno & Burton, 2013; Erskine et al., 2015, pp. 1-3; Kazdin, 2007; Myers, 2010, pp. 5-6; Sensibaugh, 2014). Indeed, in order to find out more about smokers' preferred strategies for deliberately suppressing craving-related evaluating by contradiction, we required participants to summarize their preferred strategies for doing so both before and after the IRAP.

In addition, we were also particularly interested in providing first estimates of how immediately, persistently and indeed consistently contradiction-based thought suppression would impact intrusive aspects of (pro-smoking) implicit evaluating. This was important to examine from the point of view that thought suppression strategies tend to have only temporary (ironic) effects on questionnaire-based measures of whatever intrusive evaluating they contradict (e.g. Erskine et al., 2015; Wegner & Erber, 1992, p. 905; Wegner et al., 1987, p. 6). Indeed, judging from the response times garnered in Studies One and Two, the extant thought suppression literature suggested that any dynamics in implicit evaluating due to thought suppression were likely to occur in a shorter timeframe than it would take to complete an entire set of IRAP practise and test trials (i.e. approximately 20 minutes on average). To account for this, we therefore modified the standard D_{IRAP} scoring algorithm to allow an analysis of each *trial-type* D_{IRAP} from one successive pair of IRAP trial blocks to the next, beginning with the final pair of IRAP practise blocks (i.e. which, by definition, satisfied the same response latency and accuracy criteria as those in the IRAP test phase; for the details of the relevant *extended D_{IRAP} algorithm* see Appendix 15). Again, judging from the response times garnered in Studies One and Two, we estimated that this approach would provide us with valid IRAP effect data for four successive measurement intervals lasting approximately two minutes each on average for each IRAP implementation.

Furthermore, one of the most important findings from the literature on thought suppression is that this strategy becomes harder to maintain, and thus less effective, the more one is required to engage in secondary tasks that also require relatively high levels of deliberation (i.e. requiring relatively high levels of relational responding low in *derivedness* as per Hughes, Barnes-Holmes, & Vahey, 2012, pp. 24-34; for examples see Erskine et al., 2015; Moss et al., 2015; Wenzlaff & Wegner, 2000). This issue is particularly relevant to tobacco addiction insofar (abstaining) smokers are classically at most risk of smoking when they are under cognitive load during stressful situations (see Baker et al., 2004; Brandon et al., 2004; Erskine et al., 2015; Farris et al., 2015; Heatherton & Wagner, 2011; Heckman et al., 2013; McCallion & Zvolensky, 2015; Shiffman & Waters, 2004). Thus, during the very times when thought suppression would in principle be most needed, it seems likely to be least effective in relation to problematic implicit evaluating. In order to model this process in terms of implicit evaluating we therefore examined the impact of the current thought suppression instructions when they were issued at two different stages of the IRAP from Study 1, each imposing a different degree of cognitive load.

Specifically, an IRAP practise phase typically involves learning how to master a given stimulus set according to a given set of challenging response latency and accuracy criteria (usually over multiple practise blocks); but crucially, an IRAP test phase does not. And as such, it follows that even though an IRAP test phase is designed to be cognitively challenging (i.e. as per the strict response criteria it imposes; see Vahey et al., 2010), the IRAP practise phase preceding it will always place a heavier cognitive (i.e. deliberative) load on participants. Indeed, if anything, this is particularly likely to be the case for those who have not previously completed an IRAP (i.e. it would require one to learn how to respond not just to a given IRAP stimulus set, but also to the more general features shared among all IRAPs). And confirming this, it is now well established in the IAT literature that the more unfamiliar the IAT responses required (i.e. those in so-called *inconsistent IAT blocks*) the more they tend to speed up over one's first two encounters with an IAT (see Greenwald et al., 2003, p. 211; Robusto et al., 2008, pp. 955, 959; and indeed, for related practise effects on the impact of IAT block order see Nosek et al., 2005).

We therefore recruited the current smokers, as in Study 1, so that they had absolutely no experience completing an IRAP; and we randomly assigned these recruits between the following two experimental groups designed to vary the amount of cognitive load they experienced during the IRAP test phase. Namely, one group received the relevant thought suppression instructions before the IRAP practise phase (i.e. the *pre-practise group*); and the other received those same instructions just before the IRAP test phase (i.e. the *post-practise group*). As such, by the time the pre-practise group reached the IRAP test phase, in principle, they should have been experiencing more cognitive load on the thought suppression task than the post-practise group owing to fatigue. That is, unlike the post-practise smokers, by the time the pre-practise smokers reached the IRAP test phase they would have already been continually deriving the relevant non-smoker scenarios for 5-10 minutes under the relatively high cognitive load of learning to master the IRAP's stringent response criteria.

In addition, we specifically recruited the current smokers according to the same criteria as those used in study one, and this allowed us to incorporate the smokers' IRAP data from study one as a quasi-experimental control for the pre- and post-practise groups' implicit evaluating. This control group was particularly important given that the current study was focused upon the immediacy and persistency with which the thought suppression instructions impacted smokers' implicit evaluating. In particular, given the well known instability of response latency measures, it was possible that each respective *trial-type* D_{IRAP} might have varied from block pair to block pair even in the absence of treatment (e.g., see Greenwald et al., 2003, p. 211; LeBel & Paunonen, 2011; Nosek et al., 2005; Robusto, Cristante, & Vianello, 2008, pp. 955, 959). To mitigate this possibility, it was important that we therefore judged all changes in the perspective switchers' *trial-type* D_{IRAP}s from block pair to block pair against the control group's corresponding *block pair trial-type* D_{IRAP}s.

Moreover, another key benefit of this approach was that, in principle, it maximised the timeframe across which we could examine the developmental trajectory of thought suppression on implicit evaluating. Crucially, whereas the three successive pairs of post-practise IRAP test blocks measured the immediate impact of thought suppression on implicit evaluating, the four pre-practise IRAP trial blocks from the criterion practise blocks onwards extended this timeframe by up to another four pairs of trial blocks (i.e. depending upon the average number of practise blocks it took the pre-practise group to reach the criterion practise blocks). In other words, the pre- and post-practise groups together had the potential to measure the impact of thought suppression on implicit evaluating across a range of up to seven pairs of IRAP trial blocks (i.e. if participants completed three pairs of practise blocks before reaching the criterion practise blocks). Indeed, even with some overlap between the thought suppression ranges examined by the three post-practise IRAP test blocks, and the four qualifying pre-practise IRAP trial blocks, it would provide an important opportunity to check for time-lagged replication of thought suppression effects between the two groups *block-pair trial-type* D_{IRAP}s.

Of course, any comparisons with a quasi-experimental control group must be viewed with a certain degree of scepticism owing to the lack of random assignment across such comparisons. On balance, however, we designed the current study to mitigate such concerns in multiple ways. Not only did we employ the same strict sampling criteria for all three groups (i.e. a form of experimental control in its own right), but the post-practise group's IRAP data also provided supplementary control data against which to gauge how well the smokers from study one matched the current smokers at baseline on the current IRAP. Namely, the fact that the post-practise group had completed the IRAP practise phase before being instructed to engage in thought suppression, allowed us to compute a supplementary baseline control in the form of *trial-type* D_{IRAP}s derived from the final pair of IRAP practise blocks in each case. Crucially, not only did the resulting *trial-type* D_{IRAP}s satisfy the same response criteria as conventionally imposed upon IRAP test data, but these *criterion practise trial-type* D_{IRAP}s were also randomly assigned with respect to the other thought suppression group.[63] And therefore, the post-practise group's *criterion practise trial-type* D_{IRAP}s provided us with a strong experimental safeguard on using the smokers' IRAP data from Study One as a quasi-experimental control group here.

Moreover, given the relative rarity of regular smokers in the general population at any given time who have not recently planned or attempted to quit smoking, it seemed to us particular wasteful of our limited resources in the current project to collect a fully randomised control group for the current study (i.e. less than one quarter of the Irish population report smoking at least once per week, and contrary to our sampling

[63] Note, however, that although the post-practise group's *criterion practise trial-type* D_{IRAP}s provided an experimental control for the pre-practise group's *criterion practise trial-type* D_{IRAP}s they did not necessarily provide a stable baseline, or thus an experimental control, for any *trial-type* D_{IRAP}s computed from IRAP test phase data (i.e. whether computed on a block pair by block pair basis across the entire IRAP test phase; see above).

criteria, approximately 20-57% of these smokers were typically transitioning towards smoking-cessation at any given time; see Borland, Partos, Yong, Cummings, & Hyland, 2012; Irish Cancer Society, 2014; Morgan et al., 2008, p. 76-77). Critically, the current thesis sought to provide a first systematic exploration of the IRAP as a means of understanding tobacco addiction. And therefore, rather than essentially replicating the relatively particular IRAP findings we obtained for smokers in Study 1, we instead deemed it a higher priority to dedicate our limited resources to exploring tobacco addiction more broadly as per the remainder of the current thesis. This strategy was mainly born from the simple idea that notwithstanding our firm commitment to replicating theoretically important findings, one must first conduct research across multiple aspects of a given topic before it becomes possible to judge what theoretical priority particular findings should have for (ever more precise) replication (for reviews of related issues see Cumming, 2013; Field & Gillett, 2010, pp. 666-668; Francis, 2012; Rosenthal & DiMatteo, 2001, pp. 64-70).

Overall, therefore, we made only tentative predictions about the extent to which the current thought suppression instructions might succeed in suppressing the pattern of pro-smoking implicit evaluating we observed among smokers in Study 1. As we reviewed earlier, the extant literature on thought suppression was based upon explicit evaluating of implicit evaluating, and thus it provided us with little basis to predict how the current thought suppression strategy would impact any implicit evaluating that it contradicted; much less how immediately and persistently this would occur. Nonetheless, on the basis that high cognitive load is likely to interfere with any thought suppression strategy, and given that the IRAP is cognitively demanding, we predicted that the current thought suppression instructions would only temporarily reduce those aspects of smokers' pro-smoking implicit evaluating that it contradicted. That is, we expected these instructions to only temporarily reduce smokers' coherent tendency to affirm the *Smoking-Pos* and *Ban-Neg* topics, and also their tendency to deny the *Smoking-Neg* topic. Indeed, further along these lines, we also predicted that such reductions would be less in the pre-practise group than in the post-practise group; and particularly for any pre-practise group IRAP effects occurring more than three trial blocks after the thought suppression instructions were introduced (i.e. outside the thought suppression timeframe covered by the pre-practise group's IRAP test phase).

In addition, thought suppression rebound effects typically only occur on explicit measures (of implicit evaluating) once participants (a) report having been engaged in thought suppression for protracted periods of time under cognitive load, and (b) they are reporting a precipitous decrease in their attempts to do so (e.g. due to fatigue; see Erskine et al., 2015; Wenzlaff & Wegner, 2000). On this basis, therefore, we also tentatively predicted that if the current thought suppression instructions were to provoke rebound effects it would only be on those aspects of the smokers' implicit evaluating that they contradicted; and only after a delay of at least one IRAP block pair following their introduction. And furthermore, we also expected the current thought suppression instructions would temporarily increase the smokers' tendency to affirm the *Ban-Pos* IRAP trial-type without provoking any rebound effects, because these instructions ostensibly encouraged rather than contradicted the smokers' implicit tendency to affirm the *Ban-Pos* topic.

Finally, as a secondary aim of the current research we also sought to explore how the thought suppression instructions would interact with the IRAP trial block order variable. Critically, the current thought suppression instructions specifically directed participants to interpret all IRAP instructions, including those relevant to block order, from the perspective of a lifelong non-smoker (i.e. thus incorporating the vague block order instructions within the thought suppression instructions). And therefore, we were interested in examining how the thought suppression instructions interacted with IRAP block order sequencing, rather than with the minimal and rather ambiguous block order instructions we described in discussing Study 1. However, we had little or no basis upon which to predict the extent to which our thought suppression instructions would operate by interfering with any implicit evaluating it contradicted versus by facilitating any complementary implicit evaluating that it (therefore) affirmed. And as such, without any knowledge of how it would operate upon implicit evaluating, we had no basis for predicting how the relevant thought suppression processes would interact with the current IRAP's trial block order sequencing.

Nonetheless, it is well-known that individuals typically engage in thought suppression less over time, and particularly under cognitive load. Therefore, given that the IRAP is designed to impose a relatively high cognitive load on participants, we tentatively predicted that the current smokers would be somewhat less inclined to adhere to the thought suppression instructions in any given IRAP trial block pair whenever the first trial block in each pair rendered those instructions redundant. For example, having received the current thought suppression instructions those completing the anti-smoking-first IRAP would initially have to maintain thought suppression during an anti-smoking trial block where it would apparently not be needed, before they reached a pro-smoking trial block where it would apparently be needed. Crucially, given that thought suppression typically diminishes over time, with such a delay in relevancy it seemed likely that the current smokers would engage less with thought suppression across any given IRAP block pair (i.e. the basic unit of all IRAP effects); and indeed, particularly for IRAP block pairs delivered further after the relevant thought suppression instructions. Therefore, we expected that the thought suppression instructions would be somewhat more successful in reducing the smokers' pro-smoking implicit evaluating during the pro-smoking-first block order than during the anti-smoking-first block order; and particularly under the additional cognitive load experienced by the pre-practise group.

6.3. METHOD (STUDY 3)

Participants

We recruited 43 smokers to the current study using strictly the same criteria for recruiting smokers as in Study 1, and of these participants we excluded five for failure to progress to the IRAP's test phase within four pairs of practise blocks. The remaining 38 smokers (19 female) were each randomly assigned without replacement between the two thought suppression conditions. In summary, 18 (9 female) were instructed to adopt the perspective of a lifelong non-smoker throughout both the practise and test phases of the IRAP (i.e. the *pre-practise group*); and 20 (9 female) were instructed to adopt that perspective throughout the IRAP test phase but not during its practise phase (i.e. the *post-practise group*). In addition, as usual, all participants were randomly assigned between the two levels of the IRAP block order variable for counterbalancing.

Crucially, the 38 smokers in question were well matched to those who completed the IRAP in Study 1 (i.e. the *smoker control group* within the current study). On average, they were 25 years of age (SD = 6.4; *range* = 18-40), had been smoking regularly for 8.9 years (SD = 7.1; *range* = 0.5-30), smoked on 96% of the previous 30 days, and consumed 15.2 CPD on those days (SD = 5.3, *range* = 4-25). Thus, not only were there no differences between the smoker controls and the thought suppression groups with respect to age, $t(57) = .48$; $p = .63$ (i.e. $r \approx .06$), this was also the case with respect to YS, $t(57) = -.05$; $p = .46$ (i.e. $r \approx .01$), CPD, $t(57) = -.03$; $p = .97$ (i.e. $r \approx .004$), and the percentage of the previous 30 days each participant had smoked, $t(57) = -.82$; $p = .42$ (i.e. $r \approx .11$). Also, as in Study 1, all 38 of the thought suppression smokers were naive to the IRAP, and had been similarly exposed to the Irish Smoking-ban (i.e. continuously resident in Ireland throughout the approximately 3 years since its introduction at the time of the current study). In addition, none had recently committed, or attempted to restrict their smoking – indeed, 60% had never attempted to quit smoking before (as compared to 75% in Study 1), and it had been at least ten months since the remainder had made any such attempt. Furthermore, we found no differences in the foregoing criterion variables when we compared the pre- versus post-practise thought suppression groups with each other and with the smoker controls taken from Study 1, $F(2, 56)\text{s} \leq 1.34$, $p\text{s} \geq .27$ (i.e. $r\text{s} \leq .15$).

Apparatus

We employed the same unconditional-feelings-IRAP, and also the entire set of questionnaire measures used in Study 1, but with some important differences. Unlike the smoker controls from Study 1, the thought suppression smokers were formally instructed to adopt the perspective of a lifelong non-smoker either throughout the IRAP (i.e. the pre-practise group) or during just the IRAP test phase (i.e. the post-practise group). The specific wording of these instructions were as follows (see Appendix 16 for specific formatting):

> In this study we wish to investigate the extent to which smokers are able to pretend they are non-smokers.
>
> Accordingly, I would like you to do your very best to **imagine yourself as a lifelong NON-SMOKER** while you are completing the following tasks. Remember, however, that to complete the following tasks properly your response-times must be as accurate and fast as possible (i.e., go fast and avoid the red X).
>
> **In other words, think of yourself as a non-smoker and avoid the appearance of the red X's while also responding quickly.**
>
> **In your own words,** what do you think you are now required to do and what strategy do you intend to use to accomplish it (if you don't know inform the researcher):

In addition as a manipulation check, both thought suppression groups were also instructed to complete all of the Study 1 questionnaires from the perspective of a life-long non-smoker, but with the exception of the DBHQ (i.e. it would have confounded the current known-groups agenda for participants to have pretended being a lifelong non-smoker while answering the DBHQ). For details of these instructions see Appendix 17.

Crucially, both sets of instructions required participants to describe in writing what specific strategy they intended to use in order to respond from the perspective of a lifelong non-smoker. Moreover, in those relatively few cases (not recorded) where a written account failed to describe at least one concrete scenario related to being a non-smoker, the researcher prompted the relevant participant to conversationally describe some such scenario that reminded them of how they felt about smoking at a time before they first started smoking . Indeed, along similar lines, the researcher also took particular care to ensure that each participant's written account (and/or conversational description) of this perspective switching task also emphasized the need to respond quickly while avoiding the onscreen appearance of red X's during IRAP trials. Furthermore, once the researcher had verified the foregoing for each participant, he instructed all thought suppression smokers to make a special effort to keep their chosen scenario(s) 'in mind during all of the following computerized tasks whenever it does not prevent you from achieving at least 80% accuracy and at most 3000*ms* median response speed on those tasks.' Indeed, the researcher advised participants that they would find this combined task more manageable if they deliberately set aside 30 seconds in between each 'block of the computerized tasks' to concentrate upon visualising their chosen non-smoker scenario(s) in preparation for each successive block.

In addition, as further manipulation checks, both thought suppression groups also completed one set of three questions specifically relating to the IRAP-based perspective switching task (see Appendix 18), and another set of matched questions relating to the questionnaire-based perspective switching task (see Appendix 19). In particular, both sets of manipulation check questionnaires began by asking participants to rate 'from their own perspective': (a) how successful they reckoned they had been at pretending to be a lifelong non-smoker during the relevant perspective switching task; and (b) how motivated they felt in doing so (i.e. on six-point Likert-type scales ranging from 'O [Not At All]' to 3 [Moderately]' to '6 [Very Much]'; see Appendices 18 & 19). Lastly, both sets of manipulation check questionnaires concluded by asking participants to retrospectively summarize (again from their 'own perspective') how they had attempted to adopt the perspective of lifelong non-smoker on the relevant IRAP/questionnaires.

Procedure

The current study employed the same procedure as Study 1 except for the following additions. The pre-practise group were formally instructed before the IRAP practise phase to adopt the perspective of a lifelong non-smoker 'throughout the following set of computerized tasks'; and the post-practise group received no such instruction until the interval between the IRAP practise and test phases (for details of the relevant instruction sets see the Apparatus section above). Thus, whereas the pre-practise group were instructed to respond from the perspective of a lifelong smoker during both the IRAP practise and test phases, the post-practise group were not instructed to do so until after the IRAP practise phase. After completing the IRAP test phase both perspective switching groups were asked to answer the above-described manipulation check questionnaire relating to the preceding IRAP-based perspective switching task, followed by being formally instructed to again adopt the perspective of a lifelong non-smoker while answering all of the questionnaires from Study 1 (i.e. barring the DBHQ). Having done so, both thought suppression groups then completed the above-described manipulation check questionnaire relating to the preceding questionnaire-based perspective switching task. Lastly, the researcher interviewed all of the current participants using the DBHQ in the same semi-structured manner as described for Study 1.

6.4. RESULTS (STUDY 3)

6.4.1. Scoring and Analysing the IRAP Data

Of the 38 thought suppression smokers who succeeded in progressing to the IRAP test phase, we excluded four from further analyses on the basis that they did not maintain an average response accuracy of at least 80% and/or a response speed of at most 3000*ms* across its trials. We scored the remaining 16 pre-practise and 18 post-practise groups' IRAP data using both the standard D_{IRAP}-algorithm (see Appendix 4), and also an 'extended' D_{IRAP}-algorithm (see Appendix 15).

6.4.2 Manipulation Checks for the Thought suppression Instructions

The core aim of the current study was to examine how attempting to continually maintain the perspective of a lifelong non-smoker would impact committed smokers' implicit evaluating of the current trial-type topics. Therefore, in order to verify the degree to which the current thought suppression instructions were effective in encouraging such attempts, we examined multiple manipulation checks related to the manner in which participants adopted the perspective of a lifelong non-smoker.

Self-reported Perceptions of Success and Motivation

The thought suppression groups differed to a moderate statistical degree, in terms of the success they reported in responding from the perspective of a lifelong smoker throughout the IRAP, $t(32) = 1.45$; $p = .16$; $\eta^2 = .06$ (i.e. $r \approx .25$); but not with respect to the questionnaire-based thought suppression task, $t(32) = -.09$; $p = .92$; η^2 =.0003 (i.e. $r \approx .02$). In practical terms, however, the relevant differences were relatively minor insofar as these groups reported being successful to a similarly moderate degree in adopting the perspective of a lifelong non-smoker during the IRAP (i.e. respectively, *M*s = 3.3, 3.9; *SD*s = 1.5, 1.1); and to a similarly moderate-to-large degree during the relevant questionnaires (i.e. respectively, *M*s = 4.1, 4.1; *SD*s = 1.7, 1.4). Moreover, there was no statistical difference between the pre- and post-practise groups in terms of how motivated they reported feeling to complete the IRAP-based thought suppression task, $t(32) = -.09$; $p = .93$; $\eta^2 = .0003$ (i.e. $r \approx .02$), or its questionnaire-based counterpart, $t(32) = -.05$, $p = .96$, η^2 =.0001 (i.e. $r \approx .01$). Rather, both groups reported feeling a moderate-to-large degree motivated both during the relevant IRAP-based (i.e. respectively, *M*s = 3.4, 4.1; *SD*s = 1.5, 1.3), and questionnaire-based tasks (i.e. respectively, *M*s = 4.3, 4.3; *SD*s = 1.3, 1.6).

Furthermore, given the extensive concordance between participants' retrospective perceptions of success in completing the thought suppression tasks and their subsequent self-reported motivation in doing so, we examined the extent to which these two variables were correlated with each other. This resulted in large correlations between these measures both in relation to the IRAP- and questionnaire-based thoughts suppression tasks (i.e. respectively, $r(32)$s = .64, .65; ps < .0001). Crucially, this might explain why the pre-practise group, $t(15) = -1.74$, $p = .10$, $\eta^2 = .17$ (i.e. $r \approx .41$), but not the post-practise group, $t(17) = -.34$, $p = .74$, $\eta^2 = .007$ (i.e. $r \approx .08$), reported less motivation in completing the IRAP-based perspective switching task than its questionnaire-based counterpart (see relevant descriptive statistics above). Namely, as a result of the pre-practised group having first retrospectively reported less success in completing the IRAP-based task relative to the questionnaire-based task, $t(15) = -.32$, $p = .21$, $\eta^2 = .10$ (i.e. $r \approx .32$)*, and* the post-practise group exhibiting no such difference, $t(17) = -.57$, $p = .58$, $\eta^2 = .02$ (i.e. $r \approx .14$).

Content Analyses of the Various Thought Suppression Strategies Adopted in Practise

The thought suppression instructions did not prescribe specific scenarios for participants to imagine the perspective of a lifelong non-smoker. Therefore, in order to determine in broad terms what types of perspective switching strategies participants adopted we first performed exploratory thematic analyses of the pre- and post-practise groups' open-ended descriptions of how they attempted to adopt these perspectives in practise. This resulted in four basic themes (see below) which we then used to perform the following content analyses of these open-ended descriptions. Crucially, the thought suppression groups did not differ from each other with respect to any of these four content analysis themes, $\chi^2(1, 34)$s ≤ .95, *ps* ≤ .33, *Cramer's Vs* ≤ .17.

Instead, with regard to the first theme, every participant in both thought suppression groups reported imagining an anti-smoking scenario in order to respond from the perspective of a lifelong non-smoker both during the IRAP- and questionnaire-based perspective switching tasks. And moreover, with regard to the second theme, a similarly high proportion of both thought suppression groups did not mention imagining scenarios that typified the benefits of abstinence (or smoking-bans) during either the IRAP- or the questionnaire-based perspective switching tasks (i.e. respectively, 87% and 94% during the IRAP-based task, and 100% and 89% during the questionnaire-based task).

In addition, with regard to the third theme, just over half of both groups reported difficulty maintaining the perspective of a lifelong non-smoker during the IRAP (i.e. respectively, 63% and 56%), and in contrast no participant in either group reported difficulty maintaining the perspective of a lifelong non-smoker while answering the relevant questionnaires. Indeed, with regard to the fourth and final theme, just under half of both groups reported abandoning their efforts to perspective switch before completing the IRAP (i.e. respectively, 44% and 38%), but none of either group reported doing so during the relevant questionnaires.

The Questionnaire-based Thought Suppression Task

The thought suppression groups explicitly evaluated in a very similarly anti-smoking/pro-ban manner to each other on both the semantic differentials and feeling thermometers, *t(32)s* ≤ |.89|, *ps* ≥ .38, η^2*s* ≤ .03 (i.e. *rs* ≤ .16). Namely, the pre- and post-practise groups both explicitly evaluated smoking with strong disfavour on the smoking-related semantic differentials (i.e. respectively, Ms = -2.1, -2.1; SDs = 0.9, 1.1), and feeling thermometers (i.e. respectively, Ms = -40.6, -34.5; SDs = 13.3, 24.1). And moreover, both groups explicitly evaluated the smoking-ban with strong favour on the ban-related semantic differentials (i.e. respectively, Ms = -2.2, -2.2; SDs = 0.8, 1.0), and also on the ban-related feeling thermometers (i.e. respectively, Ms = -39.9, -39.4; SDs = 15.7, 14.7). Crucially, this was in strong contrast to the control group, *t(32)s* ≥

|3.20|, *ps* $\leq$ *.003,* η^2*s* $\geq$ *.22 (i.e. rs* $\geq$ *.47)*, who explicitly evaluated the same semantic differentials and feeling thermometers in both a moderately pro-smoking (i.e. respectively, Ms = 1.2, 24.8; SDs = 1.2, 17.0), and moderately pro-ban manner (i.e. respectively, Ms = -.5, -7.6; SDs = 1.4, 28.1).

Likewise, when it came to the mFTQ, HONC and tobacco craving (TC) measures both thought suppression groups were similarly effective in portraying themselves as being absent of the symptoms of tobacco addiction. Namely, the pre- and post-practise groups both exhibited mFTQ scores that incorrectly indicated that these groups were comprised of infrequent- and/or non-smokers (i.e. respectively, Ms = 1.0, 0.5; SDs = 1.3, 0.9); as did both groups' HONC scores (i.e. respectively, Ms = 0.6, 0.3; SDs = 2.2, 1.0), and indeed their TC scores (i.e. respectively, Ms = 0.3, 0.3; SDs = 1.0, 0.8). This was in contrast to the control group whose mFTQ, HONC and TC scores consistently indicated moderate levels of nicotine dependence (i.e. respectively, Ms = 3.5, 7.0, 3.2; SDs = 1.1, 2.5, 2.0), even though these smokers' smoking histories closely matched those of both thought suppression groups (see Method).[64] Overall, therefore, it appeared as though the current cohort of committed smokers consistently imagined non-smokers' perspectives in terms of being strongly against smoking (i.e. rather than indifferent to it), and as being characteristically free from the symptoms of tobacco addiction.

6.4.3. IRAP trial-type by Thought Suppression by Block Sequence Analyses

A second major aim of the current study was to use the IRAP to explore how immediately, persistently and indeed consistently that the thought suppression instructions would impact committed smokers' implicit evaluating of each of the current trial-type topics. We therefore entered the 'extended' *block-pair* D_{IRAP} data into a 3x2x2x4 mixed ANOVA, which crossed the thought suppression variable (i.e. smoker controls versus the pre-practise smokers and post-practise smokers), with the two IRAP trial-type variables, and *IRAP trial block sequence* (i.e. four levels with one for each successive pair of IRAP trial blocks from the criterion practise blocks onwards; as per the extended D_{IRAP} algorithm; see Appendix 15). This resulted in a moderately-sized interaction among all four variables, $F(6, 159) = 3.72$, $p = .08$, $\eta_p^2 = .07$ (i.e. $r \approx .26$),

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[64] Indeed, the relevant differences between the control group and each perspective switching group was statistically very large for each of these three measures, *t*s $\geq$ 5.30, *p*s $\leq$ .0001, η^2s $\geq$ .44 (i.e. *r*s $\geq$ .66). In addition, even though there was statistically moderate difference between the two perspective switching groups on mFTQ, t(32) = 1.41, *p* = .17, η^2 = .06 (i.e. *r* = .24), it was very small in practical terms (Prokhorov et al., 1998, p. 42); and moreover, there were no such statistical differences in terms of HONC or TC, *t*(32)s $\leq$ .60, *p*s $\geq$ .55, η^2s $\geq$ .01 (i.e. *r*s $\geq$ .11).

which qualified multiple lower-level interactions and main effects.[65] In broad terms, this indicated that thought suppression had a different impact on each of the four trial-type effects across IRAP block sequence. In order to unpack this four-way interaction, we conducted the following four follow-up ANOVAs, each examining the impact of thought suppression on one of the four trial-type effects across block sequence.

Thought Suppression by Block Sequence on Smoking-Pos D_{IRAP}

There was no main effect of block sequence on *Smoking-Pos* D_{IRAP}, $F(3, 159) = 1.61$, $p = .19$, $\eta_p^2 = .03$ (i.e. $r \approx .17$), but there was a moderate main effect of thought suppression, $F(2, 53) = 1.93$, $p = .16$, $\eta_p^2 = .07$ (i.e. $r \approx .26$); and indeed crucially, thought suppression and block sequence interacted with each other to a moderate degree on *Smoking-Pos* D_{IRAP}, $F(6, 159) = 2.30$, $p = .04$, $\eta_p^2 = .08$ (i.e. $r \approx .28$). To unpack the nature of this interaction, illustrated in Figure 6.1, we conducted twelve planned comparison *t*-tests to respectively compare *Smoking-Pos* D_{IRAP} between each pairing of the thought suppression groups with the control group on each level of block sequence.

There was no difference between the control group and the post-practise group on *Smoking-Pos* D_{IRAP} during the criterion practise blocks, $t(38) = -.345$, $p = .73$, $\eta^2 = .003$ (i.e. $r \approx .06$). Crucially, this indicated that both thought suppression groups were well matched at baseline to the control group on *Smoking-Pos* D_{IRAP} from Study 1 (i.e. given random assignment of participants between the thought suppression groups). Thereafter, upon introducing the thought suppression instructions, there was little difference between the post-practise and control groups on *Smoking-Pos* D_{IRAP} during the first two pairs of IRAP test blocks (i.e. respectively, $t(38)$s = .86, .05; *p*s = .39, .96; η^2s = .02, .00006; *r*s ≈ .14, .008). However, during the final pair of test blocks the post-practise group did exhibit a *Smoking-Pos* D_{IRAP} that was a large degree less pro-smoking than the smoker control group's *Smoking-Pos* D_{IRAP}, $t(38) = 2.74$, $p = .01$, $\eta^2 = .17$ (i.e. $r \approx .41$; see Figure 6.1).

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[65] Namely, there were main effects observed for both IRAP concept label, $F(1, 53) = 13.62$, $p = .001$, $\eta_p^2 = .20$ (i.e. $r \approx .45$), and IRAP attribute stimulus class, $F(1, 53) = 6.67$, $p = .01$, $\eta_p^2 = .11$ (i.e. $r \approx .33$). And in addition to the foregoing interaction among all four variables, these two main effects were qualified by a large interaction between the trial-type variables, $F(1, 53) = 53.45$, $p < .0001$, $\eta_p^2 = .50$ (i.e. $r \approx .71$); a moderate interaction between thought suppression and the trial-type variables, $F(2, 53) = 2.29$, $p = .11$, $\eta_p^2 = .08$ (i.e. $r \approx .28$); and also moderate three-way interactions between block sequence, thought suppression and both IRAP concept label, $F(6, 159) = 1.14$, $p = .34$, $\eta_p^2 = .04$ (i.e. $r \approx .20$), and IRAP attribute stimulus class, $F(6, 159) = 1.05$, $p = .39$, $\eta_p^2 = .04$ (i.e. $r \approx .20$). Furthermore, there were also moderate two-way interactions between thought suppression and both block sequence, $F(6, 159) = 1.76$, $p = .11$, $\eta_p^2 = .06$ (i.e. $r \approx .25$), and IRAP attribute stimulus class, $F(2, 53) = 1.83$, $p = .17$, $\eta_p^2 = .06$ (i.e. $r \approx .25$). All remaining main and interaction effects were null, *F*s ≤ .79, *p*s ≥ .38, η_p^2s ≤ .03 (i.e. *r*s ≤ .17).

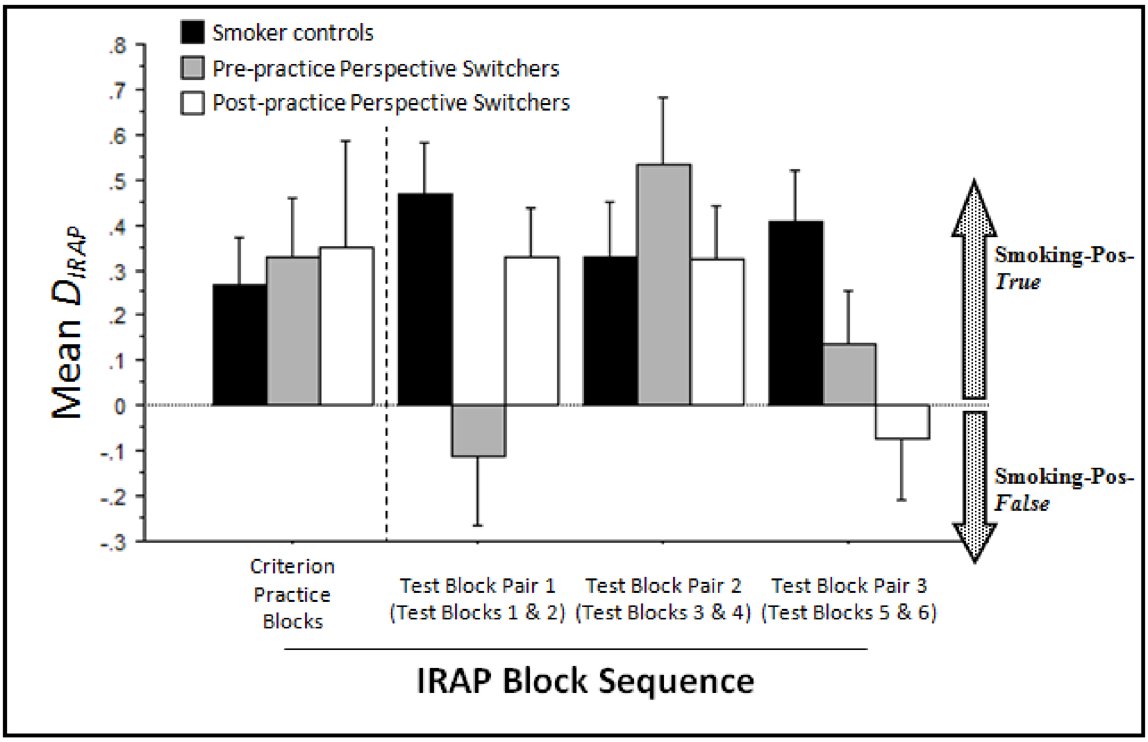

Figure 6.1. The two-way interaction between thought suppression and IRAP trial block sequence on *Smoking-Pos* D_{IRAP} (with standard error bars).

Crucially, planned *F*-test contrasts revealed that the relevant *Smoking-Pos* D_{IRAP} interaction between the control group and the post-practise group from criterion practise to test pair 1 was null, $F(1, 38) = .67$, $p = .42$, $\eta_p^2 = .02$ (i.e. $r \approx .13$); as was the interaction between test pairs 1 and 2, $F(1, 38) = .26$, $p = .62$, $\eta_p^2 = .007$ (i.e. $r \approx .08$); but in contrast, that between test pairs 2 and 3 was large, $F(1, 38) = 4.78$, $p = .04$, $\eta_p^2 = .11$ (i.e. $r \approx .33$). Thus, in summary, it appeared as though the thought suppression instructions had a relatively delayed, but eventually large impact on *Smoking-Pos* D_{IRAP} (see Figure 6.1). Indeed, bearing this out, the post-practise group's *Smoking-Pos* D_{IRAP} was positive to a similar degree throughout the criterion practise blocks and test pairs 1 and 2 (i.e. respectively, $t[17]$s = 1.49, 3.00, 2.66; *p*s = .16, .008, .02; η^2s = .11, .35, .29; *r*s = .34, .59, .54), but then reduced to the neighbourhood of zero during test block pair 3, $t(17) = -.55$, $p = .60$, $\eta^2 = .02$, $r = .13$.[66]

The pre-practise group, by contrast, who had received the thought suppression instructions before the criterion practise blocks, nevertheless did not differ from either the control group or the post-practise group on the *criterion practise Smoking-Pos*

[66] We highlighted the changing size of the post-practise group's *Smoking-Pos* D_{IRAP}s relative to zero to provide a preliminary indication of the *qualitative* impact of the perspective switching instructions on *Smoking-Pos* D_{IRAP} (i.e. whether these instructions succeeded in removing smokers' implicit preference for responding True rather than False to the *Smoking-Pos* trial-type topic). It is important to note, however, that this heuristic approach would not have been interpretable had the size of the control group's *block-pair Smoking-Pos* D_{IRAP}s not been at least somewhat stable in relation to each other, $2.55 \leq t(21)\text{s} \leq 4.05$; $p\text{s} \leq .01$; $.24 \leq \eta^2\text{s} \leq .44$; $.49 \leq r\text{s} \leq .66$ (see Figure 5.4.1).

D_{IRAP}, respectively, *t*s = -.37, .08; *p*s = .71, .94; η^2s = .0004, .0002 (i.e. *r*s ≈ .06, .01). This provided us with double confirmation that the thought suppression from pre-practise had a no impact on smokers' *Smoking-Pos* D_{IRAP}s during the criterion practise blocks. And yet, crucially, during the IRAP test phase, the pre-practise group oscillated between being less than versus broadly similar to the control group on *Smoking-Pos* D_{IRAP}. Specifically, as illustrated in Figure 6.1, the pre-practise group's *Smoking-Pos* D_{IRAP} were less than the smoker control group's *Smoking-Pos* D_{IRAP} on test block pairs 1 and 3 (i.e. respectively, *t*(36)s = 3.07, 1.62; *p*s = .004, .11, η^2s = .21, .07; *r*s ≈ .46, .26), but broadly similar to (if not more than) the smoker control's *Smoking-Pos* D_{IRAP} on test block pair 2, *t*(36) = -1.08, *p* = .29, η^2 = .031 (i.e. *r* ≈ .18).

Confirming this pattern, planned *F*-tests revealed that the *Smoking-Pos* D_{IRAP} interaction between the control group and the pre-practise group from criterion practise to test pair 1 was large and negative, *F*(1, 36) = 7.37, *p* = .01, η_p^2 = .17 (i.e. *r* ≈ .41); but that the corresponding *Smoking-Pos* D_{IRAP} interaction between test pairs 1 and 2 was in the opposite direction to a similar extent, *F*(1, 36) = 8.75, *p* = .005, η_p^2 = .20 (i.e. *r* ≈ .44); and moreover, that that the corresponding *Smoking-Pos* D_{IRAP} interaction between test pairs 2 and 3 again reversed direction and was moderately-sized, *F*(1, 36) = 3.29, *p* = .08, η_p^2 = .08 (i.e. *r* ≈ .29).

In addition, we observed aconcordant pattern of effects between the pre- and post-practise groups' respective *block-pair Smoking-Pos* D_{IRAP}s. Namely, both groups exhibited broadly similar *Smoking-Pos* D_{IRAP}s to each other during the criterion practise blocks, *t*(32) = -.08, *p* = .94, η^2 = .0002 (i.e. *r* ≈ .01), and thereafter they oscillated between being greater versus less than each other on *Smoking-Pos* D_{IRAP}. Indeed, more precisely, the pre-practise group exhibited *Smoking-Pos* D_{IRAP}s that were largely less pro-smoking than the post-practise group during the 1st test block pair, *t*(32) = -2.37, *p* = .02, η^2 = .15 (i.e. *r* ≈ .39), but during the 2nd and 3rd test block pairs it was the other way around to a moderate-to-small degree (i.e. respectively, *t*(32)s = 1.11, 1.89; *p*s = .28, .24; η^2 = .04, .04; *r*s ≈ .20, .21).

Thus, in summary, it appeared as though the pre-practise thought suppression instructions had no impact on *Smoking-Pos* D_{IRAP} during the criterion practise blocks, but had a large impact on *Smoking-Pos* D_{IRAP} during test block pair 1, which disappeared during test block pair 2, and then reappeared during test block pair 3 (see Figure 6.1). Indeed, bearing this out, the pre-practise group's *Smoking-Pos* D_{IRAP} was largely positive to begin with during the criterion practise block pair, *t*(15) = 2.51, *p* =

.02, $\eta^2 = .29$, $r = .54$, but decreased to the neighbourhood of zero during test block pair 1, $t(15) = -.73$, $p = .48$, $\eta^2 = .03$, $r = .18$; and then became largely positive again during test block pair 2, $t(15) = 3.56$, $p = .003$, $\eta^2 = .46$, $r = .68$, before becoming moderately positive during test block pair 3, $t(15) = 1.21$, $p = .24$, $\eta^2 = .09$, $r = .30$. And furthermore,

Overall, therefore, it appeared as though the thought suppression instructions had broadly the same, relatively delayed impact on the size of both the pre- and post-practise groups' *block-pair Smoking-Pos* D_{IRAP}s. In particular, as illustrated in Figure 6.1, the thought suppression instructions first impacted the pre-practise group's *Smoking-Pos* D_{IRAP} (i.e. relative to the control group's *Smoking-Pos* D_{IRAP}) in test block pair 1, in a very similar manner as they first impacted the post-practise group's *Smoking-Pos* D_{IRAP} in test block pair 3. Namely, in test block pairs 1 and 3 the control group's *Smoking-Pos* D_{IRAP}s were largely pro-smoking, $t(21)$s ≥ 3.56, *ps* $\leq .002$, η^2s $\geq .38$ (i.e. *r*s $\geq .61$), but in contrast the pre-practise group's *Smoking-Pos* D_{IRAP} in test pair 1, and the post-practise group's *Smoking-Pos* D_{IRAP} in test pair 3 were both at least trending in an anti-smoking direction (i.e. respectively, $t(21)$s = -.73, -.54; *ps* = .48, .59; η^2s = .05, .02; *r*s = .22, .13).

Moreover, not only did the thought suppression instructions have a similar first impact on *Smoking-Pos* D_{IRAP} for both the pre- and post-practise groups, but they also occurred after a broadly similar delay for both groups. Namely, the post-practise group completed two pairs of IRAP test blocks before the thought suppression instructions first impacted their *Smoking-Pos* D_{IRAP} during IRAP test block pair 3 (see Figure 6.1). And similarly, the pre-practise group had completed an average of three pairs of IRAP practise blocks before the relevant instructions first impacted their *block-pair Smoking-Pos* D_{IRAP}s on test block pair 1. Crucially, the additional delay of one block pair for pre-practise group may be explained by the differing cognitive demands placed on these groups during the IRAP practise phase.

Namely, unlike the control group or the post-practise group, the pre-practise group had to focus upon perspective switching while learning how to master the IRAP practise trials (for the first time), and this likely interfered with achieving the relevant response latency and accuracy criteria. And bearing this out, it took the pre-practise group almost one extra pair of practise blocks before they progressed to the IRAP test phase (i.e. 0.7 on average relative to the other two groups; $F(1, 50) = 5.96$, $p = .02$, η_p^2

= .11 (i.e. $r \approx .33$).[67] And moreover, thereafter, both the pre- and post-practise groups exhibited similar response accuracies and latencies during the IRAP test blocks[68] – thus suggesting that mastering the IRAP delayed, rather than modified, the successive impact of the perspective taking instructions on the pre-practise group's *block-pair trial-type* D_{IRAP}s. On balance, therefore, it appeared as though the thought suppression instructions had broadly the same (delayed and oscillatory) progression on *Smoking-Pos* D_{IRAP} in both thought suppression groups.

Thought suppression by Block Sequence on Smoking-Neg D_{IRAP}

There was no main effect of block sequence on *Smoking-Neg* D_{IRAP}, $F(3, 159) = .36$, $p = .78$, $\eta_p^2 = .007$ (i.e. $r \approx .08$), but there was a moderate main effect of thought suppression, $F(2, 53) = 1.00$, $p = .37$, $\eta_p^2 = .036$ (i.e. $r \approx .19$); and indeed crucially, both thought suppression and block sequence interacted with each other to a moderate degree on *Smoking-Neg* D_{IRAP}, $F(6, 159) = 1.08$, $p = .37$, $\eta_p^2 = .04$ (i.e. $r \approx .20$). To unpack the nature of this interaction, we conducted the following planned comparisons to respectively compare *Smoking-Neg* D_{IRAP} between each pairing of the thought suppression groups with the control group on each level of block sequence.

There was no difference between the control group and the post-practise group on *Smoking-Neg* D_{IRAP} during the criterion practise blocks, $t(38) = .287$, $p = .78$, $\eta^2 = .002$ (i.e. $r \approx .05$). Thus, crucially, both thought suppression groups were well matched at baseline to the control group on *Smoking-Neg* D_{IRAP} from Study 1. Thereafter, upon introducing the thought suppression instructions, there was little difference between the post-practise and control groups on *Smoking-Pos* D_{IRAP} during the first two pairs of IRAP test blocks (i.e. respectively, $t(38)$s = 1.03, .19; ps = .31, .85; η^2s = .026, .001; rs ≈ .16, .03). However, contrary to the thought suppression instructions, the post-practise group was a moderate degree more pro-smoking than the control group on *Smoking-Neg* D_{IRAP} during the final pair of test blocks, $t(38) = -1.87$, $p = .07$, $\eta^2 = .08$ (i.e. $r \approx .29$; see Figure 6.2).

[67] Specifically, whereas the pre-practise group completed an average of 3.2 practise block pairs before progress to the IRAP test phase, this took the control group an average of 2.5 practise block pairs, and the post-practise group an average of 2.4 practise block pairs.

[68] Namely, the 16 pre-practise switchers completed the pro- and anti-smoking test blocks with average respective accuracies of 89% and 95%, and average respective latencies of 2622*ms* and 2500*ms*. And likewise, the post-practise switchers completed the pro- and anti-smoking test blocks with average respective accuracies of 85% and 93%, and average respective latencies of 2699*ms* and 2610*ms*.

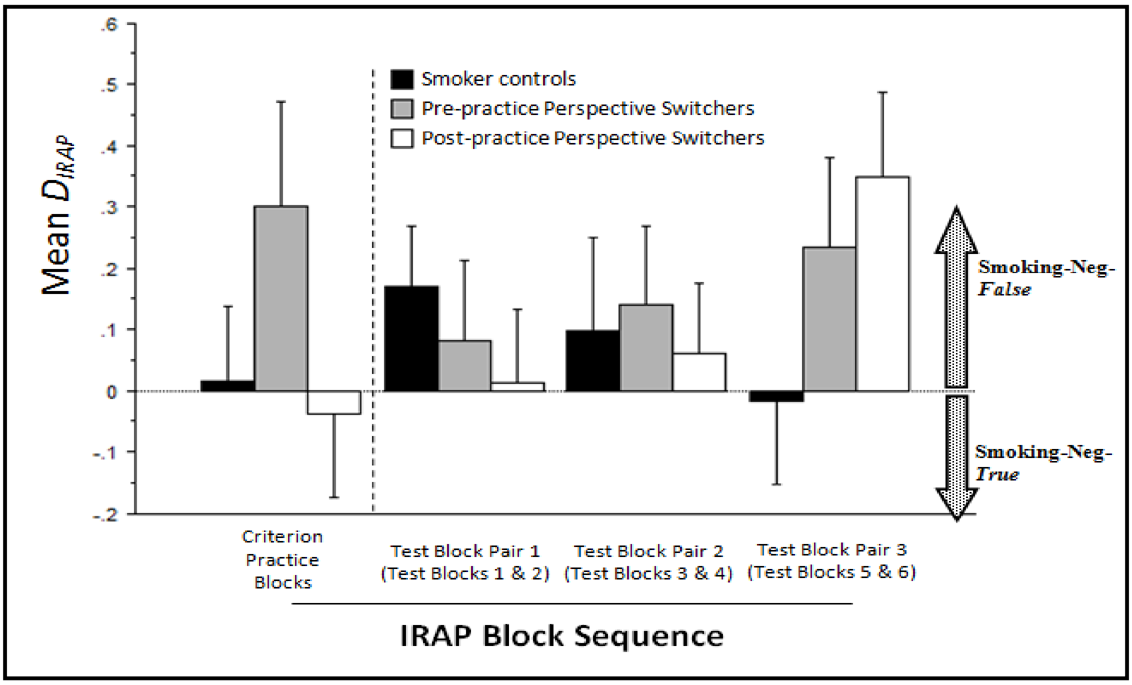

Figure 6.2. The two-way interaction between thought suppression and IRAP trial block sequence on *Smoking-Neg* D_{IRAP} (with standard error bars).

Confirming this pattern, planned *F*-test contrasts revealed that the relevant *Smoking-Neg* D_{IRAP} interaction between the control group and the post-practise group from criterion practise to test pair 1 was null, $F(1, 38) = .21$, $p = .65$, $\eta_p^2 = .005$ (i.e. $r \approx .07$); as was the interaction between test pairs 1 and 2, $F(1, 38) = .21$, $p = .65$, $\eta_p^2 = .005$ (i.e. $r \approx .07$); but in contrast, there was a moderately-sized interaction between test pairs 2 and 3, $F(1, 38) = 2.02$, $p = .16$, $\eta_p^2 = .05$ (i.e. $r \approx .22$). Thus, in summary, it appeared as though the thought suppression instructions had a relatively delayed, moderately sized ironic impact on *Smoking-Neg* D_{IRAP} (see Figure 6.2). Indeed, bearing this out, the post-practise group's *Smoking-Neg* D_{IRAP} was in the neighbourhood of zero throughout the criterion practise blocks and test pairs 1 and 2 (i.e. respectively, $t[17]$s = -.27, .12, .53; ps $\geq .60$; η^2s $\leq .02$; rs $\leq .13$), but then became largely pro-smoking during test block pair 3, $t(17) = 2.55$, $p = .02$, $\eta^2 = .28$, $r = .53$.[69]

The pre-practise group, by contrast, were a similarly large degree greater than the control group on the *Smoking-Neg* D_{IRAP} during the criterion practise blocks, $t(36) = -1.41$, $p = .17$, $\eta^2 = .05$, $r \approx .23$, as the post-practise group was during test block pair 3 (i.e. when the thought suppression instructions first impacted the post-practise group's *Smoking-Neg* D_{IRAP}; see above). And indeed, confirming this, the pre-practise group

[69] And crucially, the control group's *Smoking-Neg* D_{IRAP} was in the neighborhood of zero during test block pair 3, $t(21) = -.12$, $p = .91$, $\eta^2 = .0006$, $r \approx .03$, as it had been in broad terms during the preceding three trial block pairs; respectively, $t(21)$s = .13, 1.78, .65; ps = .90, .09, .52; η^2s = .0008, .13, .02; rs $\approx$ .03, .36, .14 (see Figure 5.4.2).

was also a similarly moderate degree more pro-smoking on *criterion practise Smoking-Neg* D_{IRAP} than the supplementary control provided by pre-practise group on *criterion practise Need-Neg* D_{IRAP}, $t(32) = 1.57$, $p = .13$, $\eta^2 = .07$, $r \approx .27$. And moreover, both differences corresponded closely with the estimated size of the initial impact of these thought suppression instructions on the post-practise group's *block pair Smoking-Neg* D_{IRAP}s (i.e. see the relevant interaction above between the test block pairs 2 and 3).

The pre-practise group, by contrast, were moderately more pro-smoking on *Smoking-Neg* D_{IRAP} than the control group during both the criterion practise blocks and test block pair 3 (i.e. respectively, $t(36)$s = -1.407, -1.24; ps = .17, .22, η^2s = .05, .04; rs ≈ .23, .20). And, confirming this, the pre-practise group was a similarly more pro-smoking on *criterion practise Smoking-Neg* D_{IRAP} than the supplementary control provided by pre-practise group on *criterion practise Smoking-Neg* D_{IRAP}, $t(32) = 1.57$, $p = .13$, $\eta^2 = .07$, $r \approx .27$. Indeed, whereas the pre-practise group's *Smoking-Neg* D_{IRAP} was largely pro-smoking during the criterion practise blocks and test block pair 3 (i.e. respectively, $t(15)$s = 1.79, 1.62; ps = .09, .13; η^2s = .18, .15; rs ≈ .42, .39), during test pairs 1 and 2 it was much like for both other groups; namely, bordering on being pro-smoking (i.e. respectively, $t(15)$s = .64, 1.10; ps = .53, .29; η^2s = .03, .07; rs ≈ .16, .27). Thus, as illustrated in Figure 6.2, all three groups were similar to each other on *Smoking-Neg* D_{IRAP} during test block pairs 1 and 2 (i.e. $|t|$s ≤ 1.03 ; ps ≥ .58; η^2s ≤ .03; rs ≤ .16); as were the two thought suppression groups during test block pair 3, $t(32) = -.57$; $p = .57$; $\eta^2 = .01$, $r \approx .10$.

Crucially, planned F-test contrasts confirmed that the relevant *Smoking-Neg* D_{IRAP} interaction between the control group and the pre-practise group from criterion practise to test pair 1 was moderately positive, $F(1, 36) = 2.03$, $p = .16$, $\eta_p^2 = .05$, $r \approx .23$); that between test pairs 1 and 2 was null, $F(1, 38) = .23$, $p = .64$, $\eta_p^2 = .006$ (i.e. $r \approx .08$); and in contrast, that between test pairs 2 and 3 was moderately negative, $F(1, 38) = .47$, $p = .50$, $\eta_p^2 = .01$ (i.e. $r \approx .11$). Overall, therefore, it appeared as though the thought suppression instructions had broadly the same, relatively delayed ironic impact on the size of both the pre- and post-practise groups' *block-pair Smoking-Neg* D_{IRAP}s (i.e. especially given that on average the pre-practise group completed three pairs of practise blocks). Namely, the thought suppression instructions appeared to take two pairs of IRAP trial blocks before first (ironically) impacting the smokers' *Smoking-Neg* D_{IRAP}s; and thereafter, they appeared to be ineffective for two successive pairs of IRAP trial blocks before inducing a second rebound-type effect on the next *block-pair Smoking-Neg* D_{IRAP} (see Figure 6.2).

Thought suppression by Block Sequence on Ban-Pos D_{IRAP}

There was no main effect of block sequence on *Ban-Pos* D_{IRAP}, $F(3, 159) = .39$, $p = .76$, $\eta_p^2 = .007$ (i.e. $r \approx .09$), but there was a moderate main effect of thought suppression, $F(2, 53) = 1.24$, $p = .30$, $\eta_p^2 = .05$ (i.e. $r \approx .21$); and indeed crucially, both thought suppression and block sequence interacted with each other to a moderate degree on *Ban-Pos* D_{IRAP}, $F(6, 159) = 1.38$, $p = .23$, $\eta_p^2 = .05$ (i.e. $r \approx .22$). To unpack the nature of this interaction, illustrated in Figure 6.3, we conducted the following planned comparisons to respectively compare *Ban-Pos* D_{IRAP} between each pairing of the thought suppression groups with the control group on each level of block sequence.

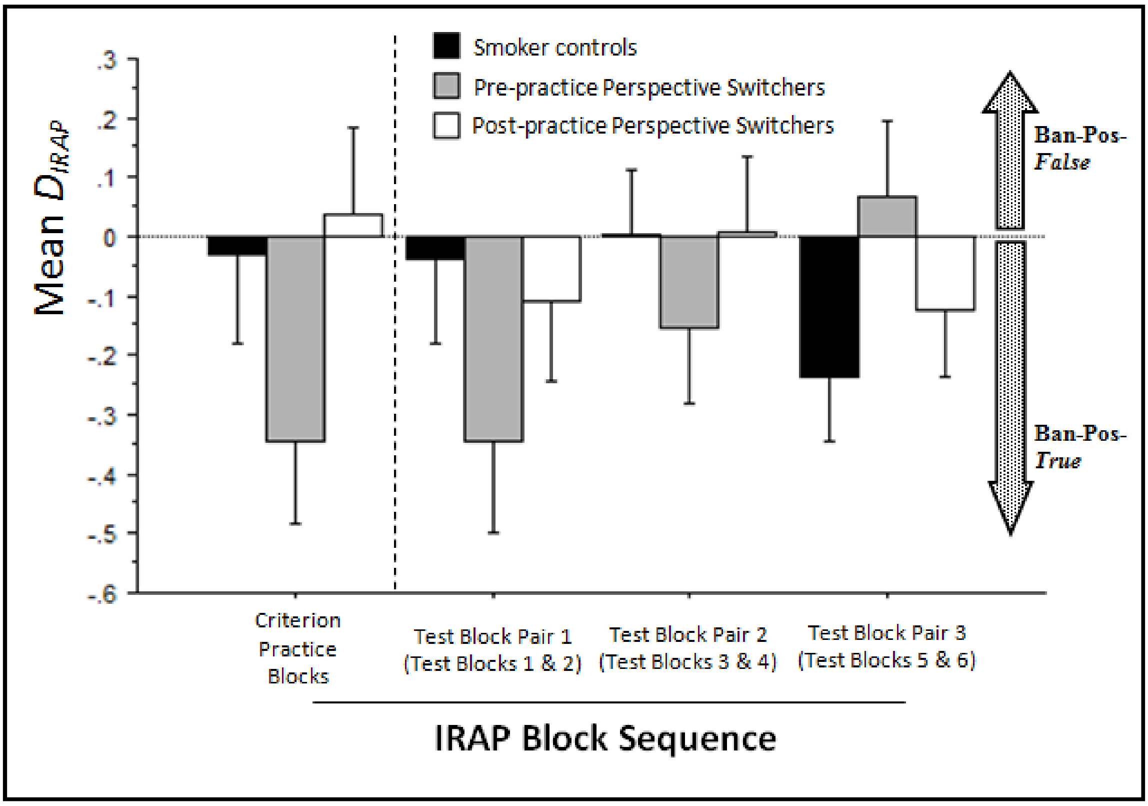

Figure 6.3. The two-way interaction between thought suppression and IRAP trial block sequence on *Ban-Pos* D_{IRAP} (with standard error bars).

The post-practise group was closely matched to the control group on *criterion practise Ban-Pos* D_{IRAP}s, $t(38) = -.32$, $p = .75$, $\eta^2 = .003$, $r \approx .05$. Thus, both thought suppression groups appeared to be well matched at baseline to the control group on *Smoking-Pos* D_{IRAP} from Study 1 (i.e. given random assignment of participants between the thought suppression groups). Moreover, curiously, the post-practise group and the controls did not differ on *Ban-Pos* D_{IRAP}s during any of the three test block pairs, $t(38)$s

≤ |.72|, *ps* ≥ .48, η^2s ≤ .01 (i.e. *r*s ≤ .12). Confirming this, planned *F*-tests revealed that there were no *Ban-Pos* D_{IRAP} interactions between successive block pairs for these two groups, *F*(1, 38) = .32, *p* = .58, η_p^2 = .008, *r* ≈ .09. Indeed, both the post-practise group and the control group each remained in the broad neighbourhood of zero on *Ban-Pos* D_{IRAP} during the criterion practise blocks, |*t*|s ≤ .25; *p*s ≥ .80, η^2s ≤ .004; *r*s ≤ .06, and test pairs 1 and 2, |*t*|s ≤ .82 ; *p*s ≥ .42; η^2s ≤ .03; *r*s ≤ .19; only becoming negative during test pair 3 (i.e. respectively, *t*(38)s = -2.22, -1.11; *p* = .04, .28; η^2 = .19, .07; *r* ≈ .44, .26; see Figure 6.3).

By contrast, the pre-practise thought suppression did appear to move *Ban-Pos* D_{IRAP} in an anti-smoking direction. Namely, the pre-practise group were more pro-ban than the control group with respect to both the *criterion practise* and *test block pair 1 Ban-Pos* D_{IRAP}s; respectively, *t*(36)s = 1.49, 1.47; *p*s = .14, .15; η^2s = .06, .06; *r*s ≈ .24, .24. And indeed, the same was true of the pre-practise group as compared to the post-practise group, respectively, *t*(32)s = -1.09, -1.17; *p*s = .07, .25; η^2s = .10, .04; *r*s ≈ .32, .20. However, when it came to test block pair 2 there was no difference between the pre-practise group and either of the other two groups on *Ban-Pos* D_{IRAP}s, |*t*|s ≤ .95 ; *p*s ≥ .35; η^2s ≤ .02; *r*s ≤ .16; and indeed, during test block pair 3 pre-practise thought suppression appeared to have a counterproductive influence on *Ban-Pos* D_{IRAP} relative to both the control group and the post-practise group, *t*s = -1.86, -1.15; *p*s = .07, .25; η^2s = .09, .04; *r*s ≈ .30, .20.

Confirming this pattern, planned *F*-tests indicated that the *Ban-Pos* D_{IRAP} interaction between the control group and the pre-practise group from criterion practise to test pair 1 was null, *F*(1, 36) = .0005, *p* = .98, η_p^2 = .00001, *r* ≈ .004; but those between test pairs 1 and 2, *F*(1, 36) = .33, *p* = .57, η_p^2 = .01, *r* ≈ .10, and between test pairs 2 and 3 together became gradually moderately negative, *F*(1, 36) = 3.62, *p* = .07, η_p^2 = .09, *r* ≈ .30. Indeed, the pre-practise group was largely and similarly negative on *Ban-Pos* D_{IRAP} during both the criterion practise pair and test pair 1 (i.e. respectively, *t*[15]s = -2.53, 2.28; *p*s = .02, .03; η^2s = .30, .26; *r*s ≈ .55, .51); but during test block pair 2 they were only moderately negative on *Ban-Pos* D_{IRAP}, *t*(15) = -1.23, *p* = .24, η^2 = .09, *r* ≈ .30, and indeed trending positive on *Ban-Pos* D_{IRAP} during test block 3, *t*(15) = .55, *p* = .59, η^2 = .02, *r* ≈ .14. Overall, therefore, it appeared as though the thought suppression instructions did not impact the post-practise group's *block-pair Ban-Pos* D_{IRAP}s, but did initially impact the pre-practise group *block-pair Ban-Pos* D_{IRAP}s in a large pro-ban direction until it diminished in test block pair 2, and then became ironic in test block pair 3 (see Figure 6.3).

Thought suppression by Block Sequence on Ban-Neg D_{IRAP}

There was no main effect of block sequence on *Ban-Neg* D_{IRAP}, $F(3, 159) = .62$, $p = .60$, $\eta_p^2 = .01$ (i.e. $r \approx .11$), but there was a moderate main effect of thought suppression, $F(2, 53) = 1.09$, $p = .34$, $\eta_p^2 = .04$ (i.e. $r \approx .20$); and indeed crucially, both thought suppression and block sequence interacted with each other to a moderate degree on *Ban-Pos* D_{IRAP}, $F(6, 159) = 1.06$, $p = .23$, $\eta_p^2 = .04$ (i.e. $r \approx .20$). To unpack the nature of this interaction, illustrated in Figure 6.4, we conducted the following planned comparisons to respectively compare *Ban-Pos* D_{IRAP} between each pairing of the thought suppression groups with the control group on each level of block sequence.

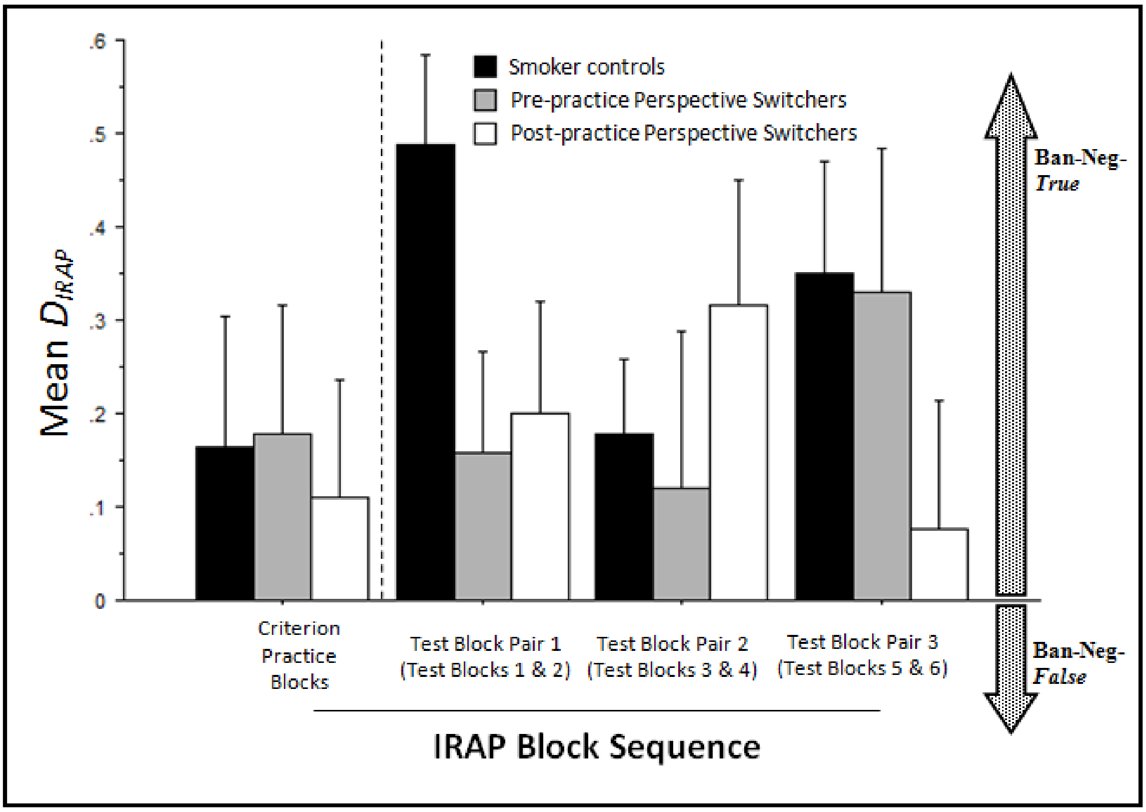

Figure 6.4. The two-way interaction between thought suppression and IRAP trial block sequence on *Ban-Neg* D_{IRAP} (with standard error bars).

The post-practise group was closely matched to the control group on *criterion practise Ban-Neg* D_{IRAP}, $t(38) = .28$; $p = .78$; $\eta^2 = .002$, $r \approx .04$. However, thereafter, once the thought suppression instructions had been introduced, the post-practise group became moderately less anti-ban on *Ban-Neg* D_{IRAP} than the control group during test block pair 1, $t(38) = 1.92$; $p = .06$; $\eta^2 = .09$ (i.e. $r \approx .30$); before oscillating between being similar during test block pair 2, $t(38) = -.91$; $p =.38$; $\eta^2 = .02$, $r \approx .15$, and moderately less anti-ban during test block 3, $t(38) = 1.51$; $p = .14$; $\eta^2 = .06$, $r \approx .24$, than the control group on *Ban-Neg* D_{IRAP}. Confirming this pattern in broad terms, the *Ban-Neg* D_{IRAP} interaction between the control group and the post-practise group from criterion practise to test pair 1 may have been null, $F(1, 38) = 1.13$, $p = .29$, $\eta_p^2 = .03$ (i.e. $r \approx .17$); but the interaction between test pairs 1 and 2 was moderately negative, $F(1, 38) = 5.62$, $p = .02$, $\eta_p^2 = .13$ (i.e. $r \approx .36$); and in contrast, there was a moderately-sized positive interaction between test pairs 2 and 3, $F(1, 38) = 2.68$, $p = .11$, $\eta_p^2 = .07$ (i.e. $r \approx .26$). Thus, on balance, it appeared as though the thought suppression instructions had a relatively immediate, moderately-sized impact on *Ban-Neg* D_{IRAP} in the pro-ban direction (see Figure 6.4).

The pre-practise group, by contrast, was similar to both the control group and the post-practise group on *criterion practise Ban-Neg* D_{IRAP}; respectively, $t(38)$s = -.08, .37; p = .94, .72; η^2 = .0002, .004; $r \approx$ .01, .06 (i.e. even though they had already received the thought suppression instructions). In fact, the only block pair during which the pre-practise group differed from the control group on *Ban-Neg* D_{IRAP} was test block pair 1, such that the pre-practise group was moderately less anti-ban than the control group, $t(36) = 2.29$; $p = .03$; $\eta^2 = .13$, $r \approx .36$. And as such, there was also no difference between the pre-practise group and the control group on *Ban-Neg* D_{IRAP} during test blocks 2 and 3; respectively, $t(38)$s = .34, .11; p = .74, .91; η^2 = .003, .003; $r \approx$ .06, .02. Confirming this pattern, the *Ban-Neg* D_{IRAP} interaction between the control group and the pre-practise group from criterion practise to test pair 1 was moderately negative, $F(1, 36) = 2.31$, $p = .14$, $\eta_p^2 = .06$, $r \approx .25$; but the corresponding interaction between test pairs 1 and 2 was moderately positive, $F(1, 36) = 2.24$, $p = .14$, $\eta_p^2 = .06$ (i.e. $r \approx .25$); and in contrast, that between test pairs 2 and 3 was null, $F(1, 36) = .02$, $p = .90$, $\eta_p^2 = .0005$, $r \approx .02$. Overall, therefore, the thought suppression instructions appeared to initially oscillate twice between effective versus ineffective from one block pair to the next on *Ban-Neg* D_{IRAP} (i.e. as per the post-practise group's *block-pair Ban-Neg* D_{IRAP}s across the IRAP test phase); before thereafter, deteriorating rapidly into ineffectiveness (i.e. as per the pre-practise group's full sequence of *block-pair Ban-Neg* D_{IRAP}s).[70]

6.4.4 Analyses of Thought suppression by IRAP Block Order by IRAP trial-type

[70] Note also, that the impact of thought suppression appeared to progress similarly in the pre- and post-practise groups. Namely, it impacted the post-practise group's *Ban-Neg* D_{IRAP} during test pair 3 very similarly to how it impacted the pre-practise group's *Ban-Neg* D_{IRAP} during test pair 1 and in each case the relevant effect occurred approximately three IRAP block pairs after the introduction of the relevant perspective switching instructions.

A secondary aim of the current study was to examine how the thought suppression instructions would interact with IRAP trial block order to affect committed smokers' implicit evaluating of the current trial-type topics. We therefore entered the *standard trial-type* D_{IRAP} data into a 3x2x2x2 mixed-repeated measures ANOVA, which crossed the two between-groups variables *thought suppression* (i.e. smoker controls versus pre-practise perspective switchers versus post-practise perspective switchers) and *trial block order* (i.e. pro-smoking-first versus anti-smoking-first), with the two repeated measures IRAP 'trial-type' variables, *Concept Label* (i.e. "Smoking makes Me Feel" versus "The Smoking Ban makes Me Feel") and *Attribute Stimulus Class* (i.e. positive versus negative craving-related moods).[71]

We obtained no main effect for block order, $F(1, 50) = .42$, $p = .52$, $\eta_p^2 = .008$ (i.e. $r \approx .09$), but did for the thought suppression variable, $F(2, 50) = 1.30$, $p = .28$, $\eta_p^2 = .05$ (i.e. $r \approx .22$), concept label, $F(1, 50) = 7.54$, $p = .009$, $\eta_p^2 = .13$ (i.e. $r \approx .36$), and for attribute stimulus class, $F(1, 50) = 7.16$, $p = .01$, $\eta_p^2 = .13$ (i.e. $r \approx .35$). Critically, however, these main effects were qualified by a moderately sized three-way interaction effect with each other, as illustrated in Figure 6.5, $F(2, 50) = 2.76$, $p = .07$, $\eta_p^2 = .10$ (i.e. $r \approx .32$); and by an large two-way interaction effect between the two trial-type variables, $F(1, 50) = 32.43$, $p < .0001$, $\eta_p^2 = .39$ (i.e. $r \approx .62$). In broad terms, this indicated that all four *trial-type* D_{IRAP} scores functioned differently from each other with respect to the thought suppression variable. To explore the nature of this three-way interaction, we therefore conducted a one-way follow-up ANOVA of thought suppression on each *trial-type* D_{IRAP}.

The *Smoking-Pos* and *Ban-Neg* both exhibited moderately sized main effects for thought suppression (i.e. respectively, $F[2, 53]$s = 2.74, 1.47; ps = .07, .24; η_p^2s = .10, .05; rs ≈ .32, .22), but the *Smoking-Neg* and *Ban-Pos* effects did not (i.e. respectively, F[2, 53]s = .33, .23; ps = .72, .80; η_p^2s = .01, .009; rs ≈ .11, .09). Furthermore, follow-up *t*-tests confirmed that as per Figure 6.5 the pre-practise perspective switchers

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[71] Note that we did not include IRAP block order in an analysis with IRAP block sequence because the current study did not incorporate enough participants per block order condition to make it feasible to analyse stable block order effects at the level of *block-pair trial-type* D_{IRAP}s. In particular, each *block-pair trial-type* D_{IRAP} incorporated a minimal number of pairs of IRAP response latencies (i.e. six), and the current study incorporated only the minimum number of participants per IRAP block order condition to make parametric statistical comparisons of difference legitimate (i.e. *n*s = 7-12; see see Lane et al., 2007; pp. 88-90; VanVoorhis & Morgan, 2007, p. 48). Indeed, based on the fact that we have already observed stable patterns of block order effects at the level of *standard trial-type* D_{IRAP}s during studies one and two, we estimated that the current study would have required at least three times as many participants as it did in order to observe stable block order effects at the level of *block-pair trial-type* D_{IRAP}s (i.e. assuming that three times as many participants would compensate for the three times as many response latencies comprising a *standard* versus *block-pair trial-type* D_{IRAP}).

exhibited moderately reduced *Smoking-Pos* and *Ban-Neg* effects relative to the control group, respectively, $t(36)$s = 2.09, 1.38, one-tailed ps = .02, .10, η^2s = .11, .05 (i.e. rs ≈ .33, .22). And that likewise, the post-practise perspective switchers also exhibited moderately reduced *Smoking-Pos* and *Ban-Neg* effects relative to the control group, respectively, $t(38)$s = 2.04, 1.45, one-tailed ps = .02, .08, η^2s = .10, .05 (i.e. rs ≈ .31, .23). Thus, crucially, it appeared as though both the *Smoking-Pos* and *Ban-Neg* effects, but not the *Smoking-Neg* and *Ban-Pos* effects, were moderately influenced in line with the thought suppression variable.

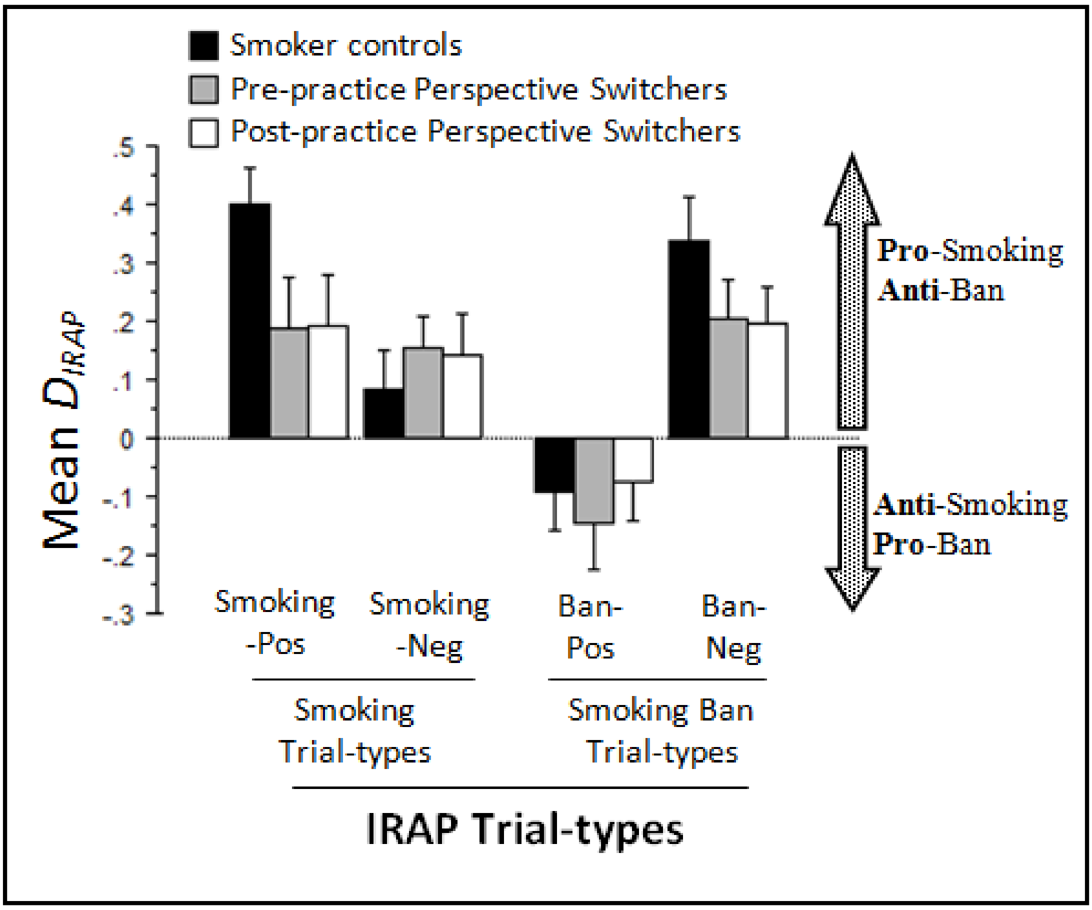

Figure 6.5. The three-way interaction between thought suppression and the two IRAP trial-type variables in terms of mean *standard trial-type* D_{IRAP} scores (with standard error bars).

Follow-up analyses indicated that the thought suppression instructions made their impact on the *Smoking-Pos* and *Ban-Neg* effects by delaying (i.e. interfering with) pro-smoking responses more than anti-smoking IRAP trial responses (for details see Appendix 20). Crucially, this indicated that the thought suppression instructions made *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} less pro-smoking by interfering with pro-smoking responding (i.e. with smokers' pre-existing implicit evaluating perspectives), rather than by facilitating anti-smoking responding (i.e. facilitating smokers' pre-existing anti-smoking implicit evaluating perspectives).

All of the interactions between block order and the two trial-type variables were null, Fs ≤ .72, ps ≥ .40, η_p^2s ≤ .01; rs ≤ .12. However, as illustrated in Figure 6.6, the foregoing three-way interaction between the thought suppression variable and the two IRAP trial-type variables, was accompanied by a moderate two-way interaction between thought suppression and block order across the four trial-type effects, $F(2, 50) = 1.67$, $p = .20$, $\eta_p^2 = .06$, $r \approx .24$. Crucially, follow-up F-test contrasts indicated that pre- and post-practise thought suppression both had an overall impact on the current *trial-type* D_{IRAP}s in the pro-smoking-first block order (i.e. respectively, Fs = 6.76, 2.47, ps = .02, .13, η^2s = .26, .11; rs ≈ .51, .33), but not in the anti-smoking-first block order (i.e. respectively, Fs = .09, .04, ps = .77, .84, η^2s = .006, .002; rs ≈ .08, .05).

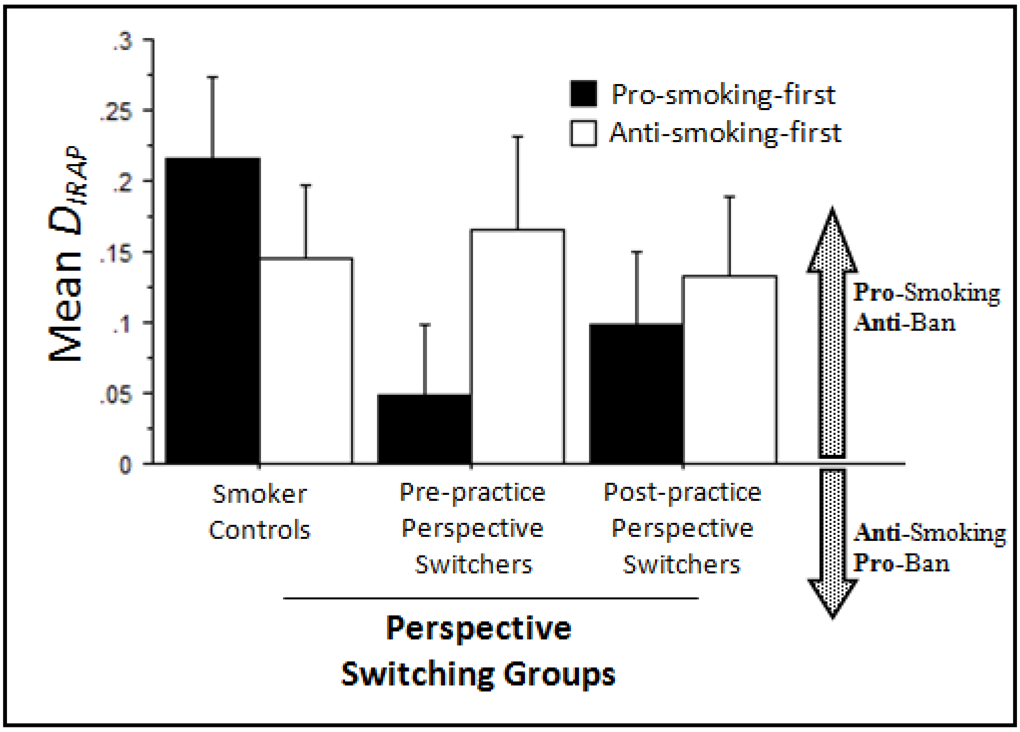

Figure 6.6. The two-way interaction between thought suppression and IRAP trial block order across the four *trial-type* D_{IRAP}s (with standard error bars).

As a result, the pre-practise group exhibited a small-to-moderate main effect of block order across all four *trial-type* D_{IRAP}s, $F(1, 62) = 2.12$, $p = .15$, $\eta_p^2 = .03$, $r = .18$, but the control group did not, $F(1, 86) = .56$, $p = .46$, $\eta^2 = .001$, $r \approx .08$, and nor did the post-practise group, $F(1, 70) = .20$, $p = .65$, $\eta^2 = .002$, $r \approx .05$ (see Figure 6.6). Moreover, using an extension of the method used to determine that thought suppression influenced the current *trial-type* D_{IRAP}s mainly by interfering with pro-smoking IRAP responding, it appeared that block order mainly facilitated thought suppression by facilitating anti-smoking responding. Namely, pro-smoking-first block order helped make the pre-practise group's *trial-type* D_{IRAP}s less pro-smoking mainly by speeding up their anti-smoking IRAP responding, $t(14) = -1.52$, $p = .15$; $\eta^2 = .14$, $r \approx .38$, rather than by interfering with their pro-smoking IRAP responding which were actually moderately faster than in the anti-smoking-first pre-practise group, $t(14) = -.90$, $p = .38$; $\eta^2 = .06$, $r \approx .23$ (i.e. the relevant interaction was moderately-sized, $F(1, 14) = .92$, $p = .35$, $\eta_p^2 = .06$, $r = .25$). In other words, the thought suppression instructions appeared to operate synergistically with the pro-smoking-first condition, and relatively independently of the anti-smoking-first condition.

6.4.5. The Impact of the Thought suppression Instructions on *trial-type* D_{IRAP} Internal Reliability

The thought suppression groups both exhibited a similar lack of internal reliability on all four standard *trial-type* D_{IRAP}s, $-.54 \leq r_{sb}\text{s} \leq .31$, one-tailed ps $\geq .12$. Crucially, this was in contrast to the control group who exhibited good internal reliability on the standard *Smoking-Pos* D_{IRAP}, $r_{sb}\text{s} = .68$, one-tailed ps $< .0001$, but no internal reliability on the remaining three standard *trial-type* D_{IRAP}s addressing anti-smoking topics, $r_{sb}\text{s} \leq .08$, one-tailed ps $> .10$.[72]

6.5 DISCUSSION (STUDY 3)

Manipulation checks confirmed that the current smokers experienced high cognitive load during the IRAP-based thought suppression tasks, with approximately 60% of the smokers from both the pre- and post-practise groups offering unsolicited reports that they were unable to avoid adhering less and less to the thought suppression instructions as the IRAP progressed. Moreover, approximately 41% of both thought suppression groups even offered unsolicited reports that they had found the thought suppression instructions so difficult to adhere to during the IRAP that they ceased attempting to do so well before completing it.

In addition, manipulation checks also indicated the current smokers were remarkably consistent in viewing the perspective of a lifelong non-smoker as being primarily anti-smoking rather than pro-abstinence. That is, whereas all of the smokers in both thought suppression groups reported deriving anti-smoking scenarios as a means of responding from the perspective of a lifelong non-smoker during the IRAP, only 0-13% of these participants mentioned deriving any scenarios on the benefits of abstinence

[72] Note that it would have been possible to calculate *Spearman-Brown corrected* split-half reliabilities for each group's *extended trial-type* D_{IRAP}s. However, the pre-practise group was instructed to thought suppress during the criterion practise blocks but the post-practise and control groups were not, and therefore it would have confounded the thought suppression instructions (with time since their delivery) to have compared these three groups with respect to the internal reliabilities of their respective *extended trial-type* D_{IRAP}s.

and/or smoking-bans in order to do so. Indeed, these smokers consistently responded to the questionnaire-based tobacco addiction scales and explicit evaluating scales from Study 1 as if non-smokers' were strongly against smoking (i.e. rather than indifferent to it), and characteristically free from the symptoms of tobacco addiction. As such, it appeared as though the current committed smokers' experience of non-smokers was stigmatizing rather than socially supportive in ways that are likely to foster abstinence (see Gifford & Humphreys, 2007, *p.* 359).

Critically, this stigmatizing approach to thought suppression was relatively ineffective as a means for smokers to eliminate, or even just control, their pro-smoking implicit evaluating. Namely, it had only an unstable and often counterproductive impact on smokers' implicit evaluating of both pro- and anti-smoking topics alike. For example, there was a delay of 2-3 pairs of IRAP trial blocks for both thought suppression groups before the relevant instructions first impacted smokers' *Smoking-Pos* D_{IRAP}s. And although that initial impact eliminated the smokers' positive *Smoking-Pos* D_{IRAP} to zero, this effect was unstable insofar as it oscillated in direction from trial block pair to trial block pair. More specifically, having initially eliminated the smokers' positive *Smoking-Pos* D_{IRAP} to zero for one trial block pair, the thought suppression instructions appeared to lead to a borderline rebound effect on the subsequent trial block pair (i.e. more pro-smoking than the control group); followed lastly by a second smaller reduction in the smokers' positive *Smoking-Pos* D_{IRAP}. In other words, relative to the control group who responded rather consistently to the *Smoking-Pos* trial-type throughout the IRAP trial block sequence, those receiving the thought suppression instructions did so in a rather conflicted, inconsistent and diminishing manner. Indeed, confirming this even further, whereas the control group exhibited good internal reliability on *Smoking-Pos* D_{IRAP}, both thought suppression groups exhibited poor internal reliability on *Smoking-Pos* D_{IRAP} (i.e. indicating conflicted implicit evaluating).

Admittedly, the thought suppression instructions were more immediate in eliminating the smokers' positive *Smoking-Neg* D_{IRAP} to zero than they did so for the smokers' positive *Smoking-Pos* D_{IRAP}. However, having done so during the first pair of IRAP trial blocks after the thought suppression instructions were introduced, this moderately-sized thought suppression effect disappeared for one trial block pair; before reversing direction to a moderate degree to result, crucially, in a moderately-sized rebound effect on the next trial block pair. Moreover, thereafter, following two more trial block pairs wherein there was no thought suppression effect another moderately-sized rebound effect appeared on the trial block pair furthest from those instructions.

And, again, accordingly the thought suppression groups both exhibited low internal reliability on *Smoking-Neg* D_{IRAP} (albeit that this was also originally the case for the control group).

Next, much like the impact of the current thought suppression instructions on the previous two trial-type effects, they also had a relatively delayed impact on the smokers' *Ban-Pos* effects which did not appear until 3-4 pairs of trial block pairs after the instructions were introduced. In fact, as such, the thought suppression instructions implemented post-practise did not appear to have enough time across the IRAP test phase to make an impact on any of the three *block-pair Ban-Pos* D_{IRAP}s collected across that phase. Rather, having initially made the pre-practise group's *Ban-Pos* D_{IRAP} a large degree more pro-smoking-ban than the control group for two successive pairs of trial block pairs, the impact of the thought suppression instructions disappeared for one trial block before becoming a moderately-sized rebound effect on the trial block pair furthest from those instructions. And again, accordingly, the thought suppression groups both exhibited low internal reliability on *Ban-Pos* D_{IRAP} (albeit that this was also originally the case for the control group).

Lastly, the thought suppression instructions affected an immediate but oscillating and temporary reduction on smokers' *Ban-Neg* D_{IRAP}s. Specifically, having initially brought about a moderate, incomplete reduction in the smokers' *Ban-Neg* D_{IRAP} during the first block pair after the instructions, the relevant reduction disappeared in the next trial block pair, before returning for one trial block, and disappearing for the remaining two trial block pairs. And, again, accordingly the thought suppression groups both exhibited low internal reliability on *Ban-Neg* D_{IRAP} (albeit that this was also originally the case for the control group).

Overall, therefore, the current thought suppression instructions had a pervasive influence on smokers' implicit evaluating of both pro- and anti-smoking topics, but that influence was rather unstable and often counterproductive across successive pairs of IRAP trial blocks. Indeed, as anticipated, when the thought suppression instructions provoked rebound effects in smokers' implicit evaluating it was always after a delay of at least one IRAP block pair following their introduction. In fact, in line with the pre-practise group being subject to more prolonged cognitive load by the IRAP measurement phase than the post-practise group, the former's block-pair trial-type effects exhibited more instability than the latter's. And indeed, confirming this prediction further, the pre-practise group retrospectively reported being less successful in adhering to the IRAP-based thought suppression instructions than the post-practise group did (albeit to a relatively minor degree). Thus, even though the thought suppression instructions reduced the pre- and post-practise smokers' standard *Smoking-Pos* and *Ban-Neg* effects to a similarly moderate overall degree (i.e. but not their standard *Smoking-Neg* and *Ban-Pos* effects), it did so only sporadically and temporarily in both cases; and particularly so for the pre-practise group.

With this in mind, we also found a moderate interaction between thought suppression and IRAP trial block order confirming our tentative predictions that the thought suppression instructions would be more effective during the pro-smoking-first IRAP block order than during its anti-smoking-first counterpart. Indeed, in line with our tentative predictions, we found that the thought suppression instructions appeared to have broadly the same impact across all four of the smokers' standard trial-type effects in the pro-smoking-first condition, but none in the anti-smoking-first condition.

Finally, as a key part of our exploratory analyses of how thought suppression instructions operate upon any implicit evaluating they contradict, we found that the current ones almost exclusively made smokers' *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} less pro-smoking by means of interfering with pre-established aspects of their pro-smoking implicit evaluating perspectives (for details of analyses see Appendix 20). Nonetheless, in addition, we also found that these thought suppression effects were supplemented to a relatively small degree by alternative means in the pro-smoking-first IRAP block order. Namely, we found that relative to the anti-smoking-first block order, the pro-smoking-first block order consistently empowered the thought suppression instructions (see Figure 6.6) to facilitate (i.e. speed up) anti-smoking responses across all four trial-type to a similarly small degree (i.e. otherwise, the thought suppression instructions neither facilitated nor interfered with smokers' anti-smoking responses; for details see Appendix 21).

In hindsight, this pattern of block order effects on pro- versus anti-smoking IRAP response latencies could perhaps be explained by the fact that the relevant anti-smoking implicit evaluating is characteristically unfamiliar to committed smokers. And as such, the current smokers were relatively ill-equipped to *immediately* switch to an anti-smoking implicit evaluating perspective, as was required by the thought suppression instructions delivered during the anti-smoking-first IRAP. Rather, instead, it appeared that by engaging in the relevant perspective switching for one pro-smoking trial block before each anti-smoking trial block, such practise within each block pair increased the fluency with which smokers provided all four anti-smoking IRAP responses during the latter half of each such block pair (i.e. the key unit upon which all

IRAP effects are based). Indeed, comporting with the fact that the pre-practise group had been required engage in thought suppression for substantially longer than the post-practise group by the IRAP measurement phase, only the former group exhibited the relevant increase in anti-smoking response fluency.

In any case, at the very least, our IRAP block order analyses revealed that the current thought suppression instructions operated synergistically with the pro-smoking-first IRAP sequencing, particularly when issued pre-practise (i.e. under additional cognitive load); but relatively independently of the anti-smoking-first condition under either thought suppression condition. And moreover, our block order analyses also confirmed our tentative prediction that the thought suppression instructions would be more successful in reducing smokers' pro-smoking implicit evaluating during the pro-smoking-first block order than during the anti-smoking-first block order, and in proportion to the cognitive load imposed on them during the IRAP measurement phase. In other words, whatever fleeting success the current thought suppression instructions had in reducing any of the four standard trial-type effects measured here, in each case that reduction appeared to be somewhat amplified during the pro-smoking-first block order relative to its anti-smoking-first counterpart – and particularly, under the additional cognitive load imposed during the IRAP measurement phase by the pre-practise instructions.

6.6. INTRODUCTION (STUDY 4)

Testing Thought Suppression as a Means for Smokers under Cognitive Load to Deliberately Control Implicit Evaluating Integral to Tobacco Addiction Selectively

Our findings from Study 3 indicated that when committed smokers were instructed to respond from the perspective of a lifelong non-smoker they consistently did so in an anti-smoking manner that stigmatized pro-smoking implicit evaluating in general. And although this broad thought suppression strategy did initially reduce smokers' pro-smoking implicit evaluating pervasively across both pro- and anti-smoking topics, these reductions were short-lived, intermittent and indeed periodically oscillated into rebound effects. According to the most dominant theory of thought suppression effects, ironic process theory, the broader one's strategy for thought suppression the greater one's cognitive load in doing so, and crucially the greater the range of evaluative rebound effects one will inadvertently provoke (see Wenzlaff & Wegner, 2000, pp. 66-70; see also Wegner, 2011, pp. 672-673). As such, our findings from Study 3, though unprecedented in measuring the progression of thought suppression effects with respect to particular types of implicit evaluating, were nevertheless somewhat expected. Crucially, however, the literature on thought suppression and/or repressive coping has little to say theoretically, or empirically about whether a more focused approach to thought suppression would improve one's efficiency in controlling intrusive aspects of implicit evaluating (e.g. see Erskine et al., 2015; Moss et al., 2015; Wenzlaff & Wegner, 2000). In fact, apart from a limited selection of questionnaire-based findings suggesting that thought suppression by focused-distraction is more efficient than by unfocused-distraction, we are aware of no other research examining this topic; and particularly not in relation to contradiction-based thought suppression strategies (Wegner, 2011, pp. 672-673).

Therefore, in order to further test the potential of contradiction-based thought suppression for (temporarily) eliminating implicit evaluating integral to tobacco addiction, Study 4 employed the same range of experimental safeguards as Study 3 but if anything with greater precision. Namely, we instructed smokers to continually deliberate, throughout the IRAP from Study 2 (i.e. the *conditional-feelings-IRAP*), about familiar scenarios in which they would typically not evaluate any need to smoke as their primary means of maintaining the perspective of a lifelong non-smoker. And as such, these instructions directly contradicted the relief-focused *Need-Neg* (and *Need-Pos*) implicit evaluating exhibited by smokers in Study 2, but not their reward-focused *Enjoy-Pos* (or *Enjoy-Neg*) implicit evaluating which was found to be merely collateral to tobacco addiction.

In other words, rather than instructing smokers to continually derive scenarios that contradicted all aspects of pro-smoking (implicit) evaluating being measured as in Study 3, instead Study 4 involved specifically instructing smokers to engage in thought suppression of just that specific aspect of implicit evaluating that we had so far found to be most integral to tobacco addiction. Crucially, this approach provided the opportunity to examine whether more focused contradiction-based thought suppression strategies would indeed improve one's efficiency in controlling intrusive aspects of implicit evaluating involved in tobacco addiction (i.e. as appears to be the case for distraction-based thought suppression; see Wegner, 2011, pp. 672-673). In particular, we were keen to explore whether the current thought suppression instructions, focused upon contradicting the *Need-Neg* (and *Need-Pos*) trial-type, would perhaps better reduce smokers' pro-smoking *Need-Neg* trial-type effects (and increase their inclination to deny *Need-Pos*) having avoided impacting their pro-smoking *Enjoy-Pos* trial-type effects (or their inclination to deny *Enjoy-Neg*).

Lastly, although it was not a primary focus of the current exploratory research, the current thought suppression instructions were also designed so that they would have similar relevance for the current smokers during both the mood-consistent and mood-inconsistent trial blocks of the conditional-feelings-IRAP. Namely, in contrast to the thought suppression instructions in Study 3 which effectively required smokers to derive various unspecified anti-smoking scenarios on demand *as needed*, the current thought suppression instructions required smokers to focus upon deriving details about certain very particular familiar scenarios. Thus, crucially, the current thought suppression instructions were in principle no less relevant in the sense of being cued during one IRAP trial block order versus another. On this basis, therefore, we tentatively predicted that the current thought suppression instructions would minimize the likelihood of any interactions between thought suppression and IRAP block order sequencing on the conditional-feelings-IRAP. In other words, we predicted that unlike in Study 3, the current smokers would engage as much in thought suppression during suppression-consistent IRAP blocks (i.e. the mood-inconsistent trial blocks) as during the suppression-inconsistent IRAP blocks (i.e. the mood-consistent trial blocks).

6.7. METHOD (STUDY 4)

Participants

We recruited 34 smokers to the current study using the same sampling criteria as in Study 2 insofar as possible (for minor exceptions see below), and of these participants we excluded one participant for failure to progress to the IRAP's test phase within four pairs of practise blocks. The remaining 33 smokers (21 female) were each randomly assigned without replacement between the two relevant thought suppression conditions. Namely, 17 (11 female) pre-practise group smokers were instructed to thought suppress in the manner described below throughout both the practise and test phases of the IRAP; and 16 (10 female) post-practise group smokers were instructed to thought suppress in the same manner but just not until just before the IRAP test phase. In addition, as usual, all participants were randomly assigned between the two levels of the IRAP block order variable for counterbalancing.

Crucially, the current smokers in question were generally well matched demographically to the corresponding control group provided by Study 2. Namely, there were no differences between the control group and the thought suppression groups with respect to age, $t(46) = -1.22$, $p = .23$ (i.e. $r \approx .17$), or with respect to CPD, $t(46) = -.41$, $p = .68$ (i.e. $r \approx .06$). On average, the perspective switchers were 25 years of age (*SD* =

6.4; *range* = 18-40), and consumed 12.6 CPD during the previous 30 days (*SD* = 6.6, *range* = 3-28). Also, as in Study 2, all 33 of the thought suppression smokers were naive to the IRAP and none had recently committed, or attempted to restrict their smoking within the previous 30 days – indeed, 41% had never attempted to quit smoking before (as compared to 75% in Study 2).

There were however three relatively minor, but nonetheless inadvertent differences between the thought suppressors and the control group. First, the thought suppressors reported smoking an average of 1.8 days fewer of the previous 30 days than the controls (i.e. respectively, *M*s = 93%, 99%; *SD*s = 8.3, 4.1; *range*s = 83-100, 83-100; $t(46) = 2.64$, $p = .01$, $r \approx .36$). Second, none of the control group from Study 2 had attempted or even contemplated quitting smoking during the previous 12 months, but five of the 33 thought suppressors had attempted to quit smoking as recently as 2 months previously (i.e. the remainder had not attempted to quit smoking in the previous 12 months). As such, a small subset of the thought suppressors were potentially more ambivalent about their smoking than the control smokers provided by Study 2. Third, on average, the thought suppressors reported smoking regularly for 3 years more than the control group, $t(46) = -1.95$, $p = .06$ (i.e. $r \approx .28$; 7.6 versus 4.6 years, *SD*s = 6.5, 1.6; *range*s = 1.5-30, 2-7).

Apparatus

The current study employed the *conditional-feelings-IRAP* described in Study 2, and also the same entire set of questionnaire measures as used in Study 2 – but with some important differences. Unlike the smoker controls from Study 2, the thought suppression smokers were formally instructed to respond from the perspective of a lifelong non-smoker during various parts of the IRAP, and while completing all of the Study 2 questionnaires except the DBHQ (i.e. it would have confounded the current known-groups agenda for participants to have pretended being a lifelong non-smoker while answering the DBHQ). In particular, the current study used the same thought suppression instructions as used in Study 3 except for one crucial addition. Namely, when it came to participants formulating a strategy for imagining themselves as lifelong non-smokers the researcher suggested that:

> *In order to imagine yourself as a lifelong non-smoker you should focus upon visualizing any scenarios where you would personally not normally feel any need to smoke (e.g. while having a shower, exercising, or doing some other familiar activity at a time before you started smoking).*

In particular, based upon anecdotal accounts we obtained from smokers for the purposes of designing the current study, we chose the illustrative scenarios of having a shower or exercising as being scenarios that are typically viewed as being quintessentially incompatible with smoking.

Procedure

The current study modified the procedure for Study 2 in much the same way in which Study 3 modified the procedure for Study 1. Namely, it used the same sequencing of thought suppression instructions as was used in Study 3, but with the additional instruction to visualize scenarios where one would not personally feel any need to smoke (see above). Thus, the pre-practise group were instructed to imagine themselves as lifelong non-smokers throughout all phases of the IRAP and all subsequent questionnaires from Study 2 except the DBHQ; and the post-practise group were instructed to do the same but from the end of the IRAP practise phase onwards.

6.8. RESULTS (STUDY 4)

6.8.1. Scoring the IRAP Data

Of the 33 thought suppressors who succeeded in progressing to the IRAP test phase, we excluded one from further analyses on the basis that they did not maintain a minimum response accuracy of 80% and/or a maximum response speed of 3000*ms* on average across the IRAP test trials. As in Study 3, we scored the remaining 16 pre-practise and 16 post-practise thought suppressors' IRAP data using both the standard D_{IRAP}-algorithm (see Appendix 4), and the *extended* D_{IRAP}-algorithm (see Appendix 15).

6.8.2. Manipulation Checks for the Thought suppression Instructions

In order to verify the degree to which the current thought suppression instructions were effective, as follows, we examined multiple manipulation check variables focusing on determining what strategy(s) the current smokers used in practise to respond from the perspective of a lifelong non-smoker.

Self-reported Perceptions of Success and Motivation

The thought suppression groups differed to a statistically moderate degree with regard to how successful they reported they were in responding from the perspective of a lifelong smoker during the IRAP, $t(28) = 1.65; p = .11; \eta^2 = .09$ *(i.e.* $r \approx .30$*)*; and to a large degree with regard to doing so during the questionnaire-based thought suppression task, $t(24) = -2.39; p = .03; \eta^2 = .19$ *(i.e.* $r \approx .44$*)*. In practical terms, however, the relevant differences were relatively minor insofar as both groups reported being successful to a moderate-to-small degree in adopting the perspective of a lifelong non-smoker during the IRAP (i.e. respectively, *M*s = 3.2, 2.2; *SD*s = 1.8, 1.5); and to a moderate-to-large degree during the relevant questionnaires (i.e. respectively, *M*s = 3.7, 5.0; *SD*s = 1.5, 1.1). Concordantly, there was also a statistically moderate-to-large difference in the degree to which the two thought suppression groups reported feeling motivated to perform their IRAP-based thought suppression task, $t(28) = 2.20; p = .04; \eta^2 =.15$ *(i.e.* $r \approx .38$*),* that was relatively minor in practical terms. Namely, both groups reported feeling moderately motivated to perform the IRAP-based thought suppression task (i.e. respectively, *M*s = 3.8, 2.7; *SD*s = 1.4, 1.2). And likewise, both groups reported feeling highly motivated to similar degree to each other in performing the questionnaire-based thought suppression task *(i.e. respectively,* *M*s = 4.5, 4.6; *SD*s = 1.5, 1.3; $t(24) = -.15, p = .88, \eta^2 = .0009, r \approx .03$*).*

Furthermore, given the extensive concordance between participants' retrospective perceptions of success in completing the thought suppression tasks and their subsequence self-reported motivation in doing so, we examined the extent to which these two variables were correlated with each other. This resulted in a large correlation between self-report success and motivation in relation to the IRAP-based thought suppression task, $r(26) = .65$; $p < .0001$; and a moderate correlation between the corresponding measures relating to the questionnaire-based thought suppression task, $r(26) = .29$; $p = .15$. And given that in each case, participants reported how successful they felt in relation to thought suppression just before they reported how motivated they felt in this regard, it therefore seems likely that latter self-reports were largely a post-hoc function of the former self-reports. Crucially, this might explain why both groups not only reported less success in completing IRAP-based thought suppression task as compared to its questionnaire-based counterpart (i.e. respectively, ts = $-1.26, -6.68$; ps = $.23, .0001$; η^2s = $.11, .80$; rs ≈ $.33, .90$), but thereafter also reported feeling less motivated in completing the former relative to the latter task (i.e. respectively, ts = $-1.93, -3.36$; ps = $.08, .006$; η^2s =$.22, .51$; rs ≈ $.47, .71$).

Content Analyses of the Various Thought suppression Strategies Adopted in Practise

To further explore what types of contradiction-based thought suppression strategies participants adopted in practise, we performed exploratory thematic analyses of thought suppressors' open-ended descriptions of what strategies they employed to respond from the perspective of a lifelong non-smoker. This resulted in four basic themes which we then used to perform the following content analyses of these open-ended descriptions. First, the pre- and post-practise groups were both highly inclined to report imagining a familiar positive non-smoking scenario in order to adopt the perspective of a lifelong non-smoker during the IRAP (i.e. respectively, 73% and 93%); even if the post-practise group were somewhat less inclined than the pre-practised group to do so, $\chi^2(1, 30) = 2.16, p = .13$, *Cramer's V* $= .27$. However, only the post-practise group showed a broadly similar inclination during the questionnaire-based thought suppression task (i.e. respectively, 17% and 67%; $\chi^2[1, 27] = 6.75, p = .01$, *Cramer's V* $= .50$).

Second, and at least somewhat concordantly, the pre- and post-practise groups both exhibited a relatively small inclination to report imagining a familiar anti-smoking scenario in order to adopt the perspective of a lifelong non-smoker during the IRAP (i.e. respectively, 40% and 20%; $\chi^2(1, 30) = 1.43, p = .23$, *Cramer's V* $= .22$); but both exhibited a relatively large inclination to report such things in relation to the questionnaire-based thought suppression task (i.e. respectively, 83% and 60%; $\chi^2(1, 27) = 1.74, p = .19$, *Cramer's V* $= .25$).

Third, the pre-practise group were somewhat less inclined than the post-practise group to report difficulty maintaining the perspective of a lifelong non-smoker during the IRAP (i.e. respectively, 67% and 87%; $\chi^2(1, 30) = 1.68, p = .20$, *Cramer's V* $= .24$). Fourth, however, the pre-practise group were no less inclined than the post-practise group to report relenting in their efforts to adopt the perspective of a lifelong non-smoker during the IRAP (i.e. respectively, 47% and 60%; $\chi^2(1, 30) = .54, p = .46$, *Cramer's V* $= .13$). Finally, by contrast, both groups were highly disinclined to retrospectively report difficulty persisting with the questionnaire-based thought suppression task (i.e. respectively, 7% and 8%; $\chi^2(1, 30) =.03, ps = .87$, *Cramer's V* $= .03$), and none of either group reported abandoning this task before completion.

The Questionnaire-based Thought-suppression Task

The thought suppression groups explicitly evaluated in a very similarly anti-smoking manner to each other on all of the semantic differentials and feeling thermometers, $|t(28)|s \leq .76, ps \geq .45, \eta^2 s \leq .02$ (i.e. $rs \leq .14$), except the two relating to the *Need-Neg* topic where they differed only to a minor practical degree (i.e. respectively, $t(28)s = 2.07, 1.53, ps = .05, .14, \eta^2 s = .13, .08; rs \approx .36, .28$). Namely, the pre- and post-practise groups both explicitly evaluated smoking as not being enjoyable during positive moods (i.e. respectively the semantic differentials, *M*s = -1.5, -1.9; *SD*s = 1.6, 1.3; and the feeling thermometers, *M*s = -28.6, -33.2; *SD*s = 28.3, 31.4), or during negative moods (i.e. respectively, the semantic differentials, *M*s = 2.0, 2.1;

*SD*s = 1.1, 1.6; and the feeling thermometers, *M*s = 31.0, 37.7; *SD*s = 23.0, 24.5). And moreover, both groups explicitly evaluated smoking as not being needed during positive moods (i.e. the respective semantic differentials, *M*s = 2.0, 2.2; *SD*s = 1.2, 1.3; and the respective feeling thermometers, *M*s = 42.5, 44.7; *SD*s = 11.9, 13.3), or during negative moods (i.e. the respective semantic differentials, *M*s = -.90, -2.0; *SD*s = 1.8, 1.1; and the respective feeling thermometers, *M*s = -15.8, -35.1; *SD*s = 39.0, 29.0).

Likewise, when it came to the mFTQ, HONC and tobacco craving (TC) measures both thought suppression groups were similarly effective in portraying themselves as being absent of the symptoms of tobacco addiction. Namely, the pre- and post-practise groups both exhibited mFTQ scores that incorrectly indicated that these groups were comprised of infrequent- and/or non-smokers (i.e. respectively, *M*s = .93, 0.71; *SD*s = 1.7, 1.3); as did both groups' HONC scores (i.e. respectively, *M*s = 1.60, 0.53; *SD*s = 3.5, 1.8), and indeed their TC scores (i.e. respectively, *M*s = 1.07, 0.33; *SD*s = 1.67, 1.29). This was in contrast to the control group whose mFTQ, HONC and TC scores consistently indicated moderate levels of nicotine dependence (i.e. respectively, *M*s = 4.8, 3.3, 2.1; *SD*s = 1.1, 2.3, .57), even though these smokers' smoking histories extensively matched those of both thought suppression groups (see Method).[73]

6.8.3. IRAP trial-type by Thought suppression by Block Sequence Analyses

A major aim of the current study was to use the IRAP to explore how immediately, persistently and indeed consistently that the current thought suppression instructions would impact the smokers' *Need-Neg* and *Need-Pos* effects as opposed to their *Enjoy-Pos* and *Enjoy-Neg* effects. As in Study 3, we therefore entered the 'extended' *block-pair* D_{IRAP} data into a 3x2x2x4 mixed ANOVA (see Appendix 15), crossing the thought suppression variable, with the two IRAP trial-type variables, and IRAP trial block sequence. This resulted in a moderately-sized interaction among all four variables, $F(6, 135) = 1.38$, $p = .23$, $\eta_p^2 = .06$ (i.e. $r \approx .24$), which qualified multiple lower-level interactions and main effects.[74] In broad terms, this indicated that

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[73] Indeed, the relevant differences between the control group and each thought suppression group was consistent for each of these three measures, ts ≥ 1.57, ps $\leq .13$, η^2s $\geq .08$ (i.e. rs $\geq .28$). Even though there was statistically moderate difference between the two thought suppression groups on TC, $t(28) = 1.35$, $p = .19$, $\eta^2 = .06$ (i.e. $r = .25$), and on HONC $t(28) = 1.05$, $p = .30$, $\eta^2 = .04$ (i.e. $r = .20$), the relevant differences were very small in practical terms (Schuh & Stitzer, 1995; Wellman, Di Franza, Pbert, et al., 2005; Wellman, Savageau, et al. 2006); and moreover, there were no such statistical differences in terms of mFTQ, $t(28) = .41$, $p = .69$, $\eta^2 = .006$ (i.e. $r = .08$).

[74] There were main effects observed for both thought suppression, $F(2, 45) = 6.02$, $p = .005$, $\eta_p^2 = .21$ (i.e. $r \approx .46$), and IRAP concept label, $F(1, 45) = 4.93$, $p = .03$, $\eta_p^2 = .10$ (i.e. $r \approx .31$). In addition, these two main effects were also qualified by a large interaction between the trial-type variables, $F(1, 45) = 33.22$, $p < .0001$, $\eta_p^2 = .42$ (i.e. $r \approx .65$); a large two-way interaction between thought suppression and IRAP concept label, $F(2, 45) = 5.60$, $p = .007$, $\eta_p^2 = .20$ (i.e. $r \approx .45$); a moderate interaction between thought

thought suppression had a different impact on each of the four trial-type effects across IRAP block sequence. In order to unpack this four-way interaction, we conducted four follow-up mixed ANOVAs, each examining the impact of thought suppression on one of the four trial-type effects across block sequence. We unpacked the statistical implications of each of these ANOVAs in the following sub-sections.

Thought Suppression by Block Sequence on Need-Neg D_{IRAP}

There was a moderately-sized interaction between thought suppression and block sequence on *Need-Neg D_{IRAP}*, $F(6, 135) = 1.73$, $p = .12$, $\eta_p^2 = .07$ (i.e. $r \approx .27$), which qualified a large main effect of thought suppression, $F(2, 45) = 4.91$, $p = .01$, $\eta_p^2 = .18$ (i.e. $r \approx .42$), and a null main effect of block sequence, $F(3, 135) = .84$, $p = .45$, $\eta_p^2 = .02$ (i.e. $r \approx .14$). To unpack the nature of this interaction, illustrated in Figure 6.7, we conducted the following planned comparisons to respectively compare *Need-Neg D_{IRAP}* between each pairing of the thought suppression groups with the control group on each level of block sequence.

The post-practise group differed to a statistically moderate degree from the control group on the *criterion practise Need-Neg D_{IRAP}*, $t(30) = 1.44$, $p = .16$, $\eta^2 = .06$, $r = .25$ (i.e. even though they had not yet been introduced to thought suppression instructions at this stage). However, given that the control group and the post-practise group both exhibited large positive *criterion practise Need-Neg D_{IRAP}*s (i.e. respectively, $t(15)$s = 4.33, 2.65; ps ≤ .02; η^2s = .56, .32; rs = .56, .76), therefore, on balance, it appeared as though the thought suppression groups were reasonably well matched to the control group on *Need-Neg D_{IRAP}*. In any case, upon introducing the thought suppression instructions this moderate difference became large for test block pair 1, $t(30) = 2.62$, $p = .01$, $\eta^2 = .19$ (i.e. $r \approx .43$), and for test block pair 2, $t(30) = 2.30$, $p = .03$, $\eta^2 = .15$ (i.e. $r \approx .39$); before then gradually returning to the original moderate difference in test block pair 3, $t(30) = 1.16$, $p = .25$, $\eta^2 = .04$ (i.e. $r \approx .21$).

Indeed, confirming this pattern, planned F-test contrasts revealed that the relevant *Need-Neg D_{IRAP}* interaction between the control group and the post-practise group from criterion practise to test pair 1 was moderately-sized, $F(1, 30) = 1.52$, $p = .23$, $\eta_p^2 = .05$ (i.e. $r \approx .22$); and that in contrast the interaction between test pairs 1 and 2 was relatively small, $F(1, 30) = .06$, $p = .82$, $\eta_p^2 = .002$ (i.e. $r \approx .04$); as was that

suppression and the trial-type variables, $F(2, 45) = 1.49$, $p = .24$, $\eta_p^2 = .06$ (i.e. $r \approx .25$); a moderate three-way interaction between block sequence, thought suppression and IRAP concept label, $F(6, 135) = 1.41$, $p = .21$, $\eta_p^2 = .06$ (i.e. $r \approx .24$); and finally, also a moderate two-way interaction between thought suppression and block sequence, $F(6, 135) = 2.01$, $p = .07$, $\eta_p^2 = .08$ (i.e. $r \approx .29$). All remaining main and interaction effects were null, Fs ≤ 1.11, ps ≥ .35, η_p^2s ≤ .02, (i.e. rs ≤ .16).

between test pairs 2 and 3, $F(1, 30) = .50$, $p = .48$, $\eta_p^2 = .02$ (i.e. $r \approx .13$). Thus, in summary, it appeared as though the post-practise thought suppression instructions had a relatively immediate, moderately-sized impact on *Need-Neg* D_{IRAP}, which faded gradually but entirely across the *Need-Neg* D_{IRAP}s for test block pairs 2 and 3 (see Figure 6.7).

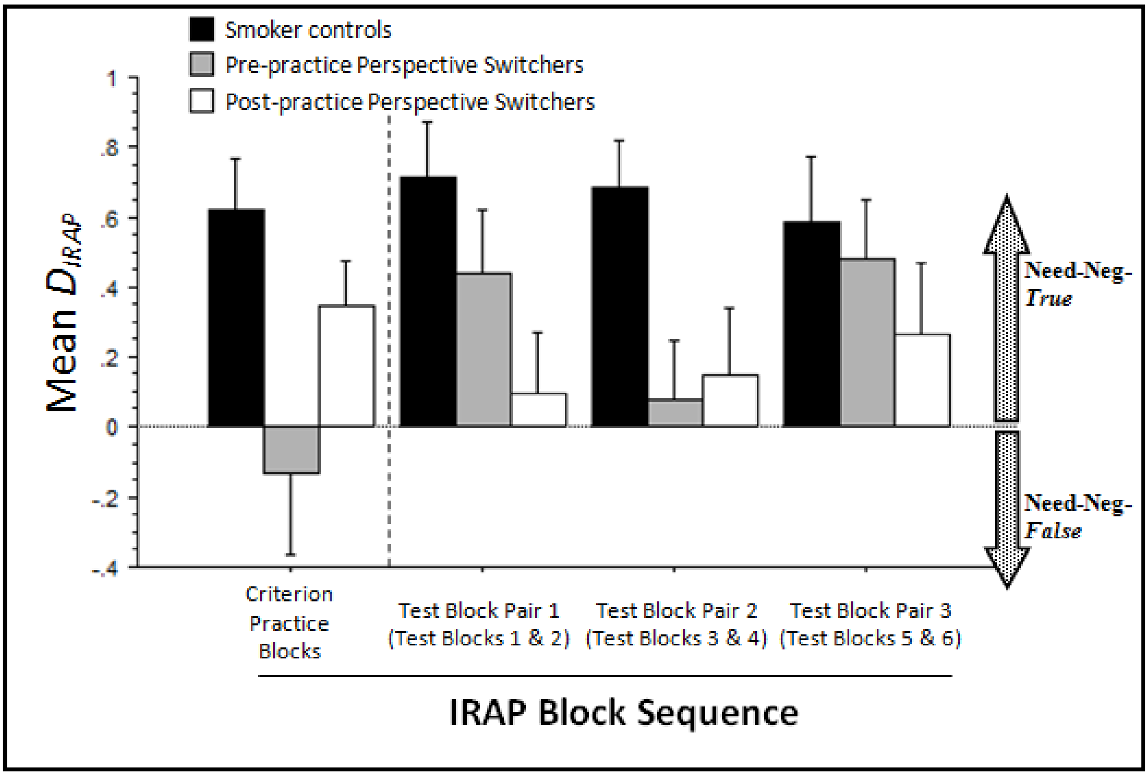

Figure 6.7. The two-way interaction between thought suppression and IRAP trial block sequence on *Need-Neg* D_{IRAP} (with standard error bars).

Indeed, more specifically, the post-practise instructions had cumulatively reduced *Need-Neg* D_{IRAP} by $\eta^2 = .05$ ($r \approx .22$) during test block pair 1 (i.e. as per the relevant F-test contrast); by approximately $\eta^2 = .048$ ($r \approx .22$) during test block pair 2 (i.e. by subtracting the η^2-effect for the relevant F-test contrast from its immediate predecessor); and finally by (approximately) a null $\eta^2 = .028$ ($r \approx .17$) during test block pair 3 (i.e. by adding the η^2-effect for the relevant F-test contrast from its immediate predecessor). Bearing this out, the post-practise group's *Need-Neg* D_{IRAP} was large to begin with during the criterion practise block pair (see above), but reduced to the neighbourhood of zero during test block pair 1, $t(15) = .54$, $p = .60$, $\eta^2 = .02$, $r = .14$; and then became small-to-moderate during test block pair 2, $t(15) = .75$, $p = .47$, $\eta^2 =$

.036, $r = .19$, before becoming moderately-sized during test block pair 3, $t(15) = 1.28$, $p = .22$, $\eta^2 = .10$, $r = .31$.[75]

The pre-practise group, by contrast, were a similarly large degree less than the control group on the *Need-Neg* D_{IRAP} during the criterion practise blocks, $t(30) = 2.74$, $p = .01$, $\eta^2 = .20$ (i.e. $r \approx .45$), as the post-practise group was during test block pairs 1 and 2 just after they had received the thought suppression instructions (see above). And indeed, confirming this, the pre-practise group was a moderate degree less on *criterion practise Need-Neg* D_{IRAP} than the supplementary control provided by pre-practise group on *criterion practise Need-Neg* D_{IRAP}, $t(30) = -1.77$, $p = .09$, $\eta^2 = .09$, $r \approx .31$. In particular, the size of this latter difference corresponded closely with the estimated size of the initial impact of these thought suppression instructions on the post-practise group's *block pair Need-Neg* D_{IRAP}s (i.e. the size of the interaction produced between the control and post-practise groups on *Need-Neg* D_{IRAP} from the criterion practise blocks to test block pair 1; see above).

However, after criterion practise, during the IRAP test phase, the difference between the control group and the pre-practise group reduced to moderately-sized in test block pair 1, $t(30) = 1.14$, $p = .26$, $\eta^2 = .04$ (i.e. $r \approx .20$), became large again in test block pair 2, $t(30) = 2.84$, $p = .008$, $\eta^2 = .21$ (i.e. $r \approx .46$), and finally almost disappeared in test block pair 3, $t(30) = .42$, $p = .68$, $\eta^2 = .006$ (i.e. $r \approx .08$). Confirming this pattern, planned F-tests revealed that the *Need-Neg* D_{IRAP} interaction between the control group and the pre-practise group from criterion practise to test pair 1 was moderately sized, $F(1, 30) = 1.94$, $p = .17$, $\eta_p^2 = .06$ (i.e. $r \approx .25$); and that the corresponding *Need-Neg* D_{IRAP} interaction between test pairs 1 and 2 was similarly sized but in the opposite direction, $F(1, 30) = 1.11$, $p = .30$, $\eta_p^2 = .036$ (i.e. $r \approx .19$); and moreover, that that the corresponding *Need-Neg* D_{IRAP} interaction between test pairs 2 and 3 was also moderately-sized but in the opposite direction to that between test block pairs 1 and 2, $F(1, 30) = 3.38$, $p = .08$, $\eta_p^2 = .10$ (i.e. $r \approx .32$). Furthermore, whereas the pre-practise group were a moderate degree less than the post-practise group on *Need-Neg* D_{IRAP} during the criterion practise blocks (see above), this relationship reversed during test block pair 1, $t(30) = 1.39$, $p = .18$, $\eta^2 = .06$, $r = .25$, before disappearing

[75] We highlighted the changing size of the post-practise group's *Need-Neg* D_{IRAP}s relative to zero to provide a preliminary indication of the *qualitative* impact of the thought suppression instructions on *Need-Neg* D_{IRAP} (i.e. whether these instructions succeeded in removing smokers' implicit preference for responding True rather than False to the *Need-Neg* trial-type topic). It is important to note, however, that this approach would not have been interpretable had the size of the control group's *block-pair Need-Neg* D_{IRAP}s not been relatively stable in relation to each other, $3.18 \leq t(15)\text{s} \leq 5.29$; $p\text{s} \leq .006$; $.40 \leq \eta^2\text{s} \leq .65$; $.63 \leq r\text{s} \leq .81$ (see Figure 5.8.1).

during test block pairs 2 and 3 (i.e. respectively, $t(30)$s = -.26, .81; ps = .79, .42; η^2 = .002, .02; r = .05, .15).

Thus, by accounting for the relevant baseline discrepancy, the pre-practise instructions reduced the smokers' *criterion practise Need-Neg* D_{IRAP} by a moderately-sized η^2 = .09 ($r \approx$ .31; i.e. as per the difference observed between the pre- and post-practise groups on *criterion practise Need-Neg* D_{IRAP}). And thereafter, the pre-practise instructions resulted in a cumulative reduction *Need-Neg* D_{IRAP} by test block pair 1 of approximately η_p^2 = .03 (i.e. by subtracting the η^2 = .06 effect for the F-test contrast for the pre- versus post-practise interaction between criterion practise and test block pair 1, from the *revised-*η^2 = .09 reduction observed in the criterion practise block pair to the η^2 = .06). And likewise, the pre-practise instructions resulted in a cumulative reduction *Need-Neg* D_{IRAP} by test block pair 2 of approximately η_p^2 = .066, but a cumulative increase in *Need-Neg* D_{IRAP} by test block pair 3 of approximately η_p^2 = .034.

On balance, therefore, the pre-practise thought suppression instructions moderately reduced *Need-Neg* D_{IRAP} during the criterion practise blocks, and this effect disappeared during test block pair 1, reappeared during test block pair 2, and again disappeared during test block pair 3 (see Figure 6.7). Indeed, bearing this out, the pre-practise group's *Need-Neg* D_{IRAP} was in the neighbourhood of zero to begin with during the criterion practise block pair, $t(15)$ = -.55, p = .59, η^2 = .02, r = .14, but increased to a large positive effect during test block pair 1, $t(15)$ = 2.46, p = .03, η^2 = .29, r = .54; but then became near zero again during test block pair 2, $t(15)$ = .45, p = .66, η^2 = .01, r = .12, before returning to a large size during test block pair 3, $t(15)$ = 2.83, p = .01, η^2 = .35, r = .59 (i.e. just as the post-practise group was at baseline during the criterion practise blocks).

In addition, the average number of pairs of practise blocks completed by the pre-practise group was 2.5, and as such, the foregoing pattern of effects collectively indicated that the impact of thought suppression on the pre-practise group's *block pair Need-Neg* D_{IRAP}s appeared to have a broadly similar developmental trajectory as it did with the post-practise group. In particular, as illustrated in Figure 6.7, the successive impact of the thought suppression instructions on the pre-practise group's *criterion Need-Neg* D_{IRAP} and *test pair 1 Need-Neg* D_{IRAP} (i.e. relative to the control group) was very similar in its progression to the impact of these instructions on the post-practise group's *Need-Neg* D_{IRAP}s for test blocks two and/or three (see Figure 6.7; and also compare the size of the relevant F-test interaction contrasts above). Overall, therefore, it appeared as though the thought suppression instructions moderately reduced the current smokers' *Need-Neg* D_{IRAP} (relative to the control group) for the first two pairs of IRAP blocks after their introduction (i.e. as per the post-practise groups' *block pair Need-Neg* D_{IRAP}s), but thereafter only in an oscillating manner from block pair to block pair until at least the fourth such pair (i.e. as per the pre-practise groups' *block pair Need-Neg* D_{IRAP}s).

Thought Suppression by Block Sequence on Need-Pos D_{IRAP}

There was a moderately-sized interaction between thought suppression and block sequence on *Need-Pos* D_{IRAP}, $F(6, 135) = 1.19$, $p = .31$, $\eta_p^2 = .05$ (i.e. $r \approx .22$), which qualified a large main effect of thought suppression, $F(2, 45) = 9.63$, $p = .0003$, $\eta_p^2 = .30$ (i.e. $r \approx .55$), and a null main effect of block sequence, $F(3, 135) = .10$, $p = .96$, $\eta_p^2 = .002$ (i.e. $r \approx .05$). To unpack the nature of this interaction, illustrated in Figure 6.8, we conducted the following planned comparisons to respectively compare *Need-Pos* D_{IRAP} between each pairing of the thought suppression groups with the control group on each level of block sequence.

The post-practise group differed to a statistically moderate degree from the control group on the *criterion practise Need-Pos* D_{IRAP}, $t(30) = 1.30$, $p = .20$, $\eta^2 = .05$; $r = .23$ (i.e. even though they had not yet been introduced to thought suppression instructions at this stage). Moreover, as illustrated in Figure 6.8, whereas the control group exhibited large positive *criterion practise Need-Pos* D_{IRAP}, $t(15) = 2.15$, $p = .05$, $\eta^2 = .24$; $r = .49$, the post-practise group's one was near zero, $t(15) = .12$, $p = .90$, $\eta^2 = .001$; $r = .03$. Therefore, on balance, it appeared as though there was somewhat of a mismatch between the thought suppression groups and the control group on *Need-Pos* D_{IRAP} at baseline (i.e. insofar as the control group exhibited an implicit preference for responding False rather than True to the *Need-Pos* trial-type topic at the criterion practise baseline, but the post-practise group did not). Nonetheless, upon introducing the thought suppression instructions this moderately-sized difference became large for test block pair 1, $t(30) = 2.97$, $p = .006$, $\eta^2 = .23$ (i.e. $r \approx .48$), and test block pair 2, $t(30) = 4.35$, $p = .0001$, $\eta^2 = .39$ (i.e. $r \approx .62$); before returning (at least somewhat) in test block pair 3 to the original moderately-sized difference, $t(30) = 2.28$, $p = .03$, $\eta^2 = .15$ (i.e. $r \approx .38$).

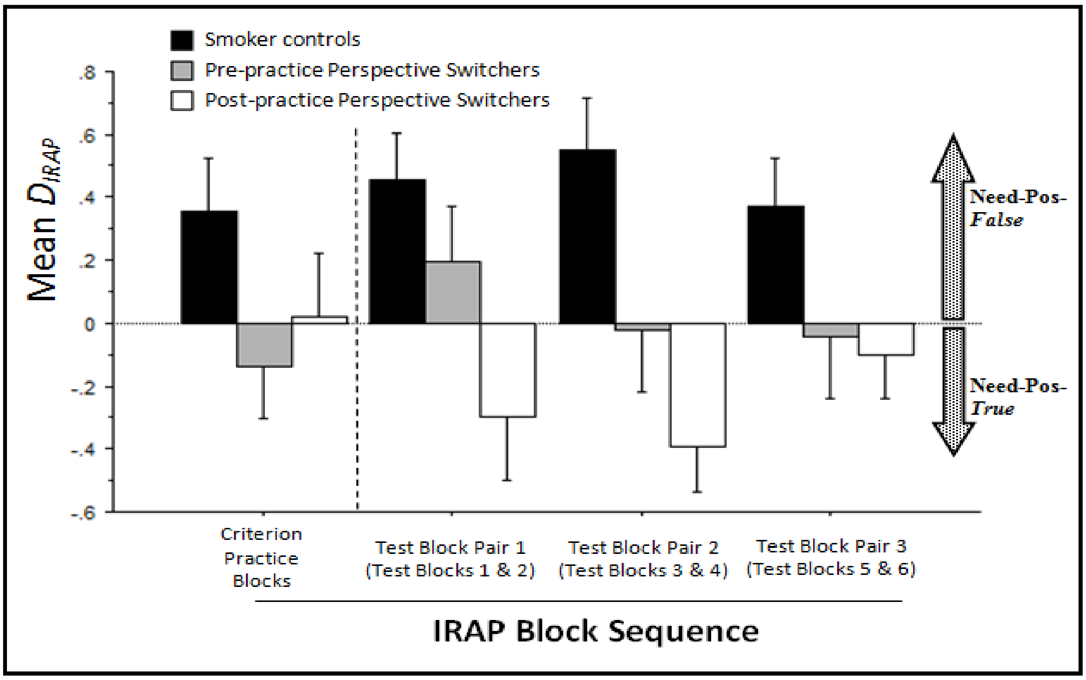

Figure 6.8. The two-way interaction between thought suppression and IRAP trial block sequence on *Need-Pos* D_{IRAP} (with standard error bars).

Confirming this pattern, planned *F*-test contrasts revealed that the relevant *Need-Pos* D_{IRAP} interaction between the control group and the post-practise group from criterion practise to test pair 1 was moderately-sized, $F(1, 30) = 1.50$, $p = .23$, $\eta_p^2 = .05$ (i.e. $r \approx .22$); but the corresponding interaction between test pairs 1 and 2 was null, $F(1, 30) = .37$, $p = .55$, $\eta_p^2 = .01$ (i.e. $r \approx .11$); and that between test pairs 2 and 3 was of a similar size to that between test pairs 1 and 2 but in the opposite direction, $F(1, 30) = 3.14$, $p = .09$, $\eta_p^2 = .09$ (i.e. $r \approx .31$). Thus, in summary, it appeared as though the post-practise thought suppression instructions had a relatively immediate, moderately-sized impact on *Need-Pos* D_{IRAP}, which lasted for block pairs 1 and 2 but disappeared during test block pair 3 (see Figure 6.8).

Indeed, more specifically, the post-practise instructions had cumulatively reduced *Need-Pos* D_{IRAP} by $\eta^2 = .05$ ($r \approx .22$) during test block pair 1 (i.e. as per the relevant *F*-test contrast); by approximately $\eta^2 = .06$ ($r \approx .24$) during test block pair 2 (i.e. by adding the η^2-effect for the relevant *F*-test contrast from its immediate predecessor); but had cumulatively increased *Need-Pos* D_{IRAP} by (approximately) a null $\eta^2 = .03$ ($r \approx .17$) during test block pair 3 (i.e. by subtracting the η^2-effect for the relevant *F*-test contrast from its immediate predecessor). However, it is important to bear in mind that the post-practise group's *Need-Pos* D_{IRAP} was near-zero to begin with during the criterion practise block pair (see above), but reduced to a moderately negative effect during test block pair 1, $t(15) = -1.46$, $p = .16$, $\eta^2 = .12$, $r = .35$; before then reducing further to large negative effect during test block pair 2, $t(15) = 2.80$, $p = .01$, $\eta^2 = .34$, $r = .59$, before returning towards the neighbourhood of zero during test block pair 3, $t(15) = -.75$, $p = .47$, $\eta^2 = .036$, $r = .19$.[76] As such, crucially, the post-practise thought suppression instructions appeared to initially have a temporary ironic impact on *Need-Pos* D_{IRAP} inducing a type of pro-smoking implicit evaluating that was not present at baseline. Namely, the post-practise instructions made it more likely initially that smokers would implicitly affirm that they do need to smoke when feeling positive craving-related emotions.

The pre-practise group, by contrast, were only a moderate-to-large degree less than the control group on the *Need-Pos* D_{IRAP} during the criterion practise blocks, $t(30) = 2.13$, $p = .04$, $\eta^2 = .13$ (i.e. $r \approx .36$); and they did not differ from the baseline control provided by the post-practise group on the *criterion practise Need-Pos* D_{IRAP}, $t(30) = -.63$, $p = .53$, $\eta^2 = .01$, $r \approx .11$ (i.e. who had yet to receive the thought suppression instructions). Thus, on balance, it appeared as though pre-practise thought suppression had a null impact on the *criterion practise Need-Pos* D_{IRAP} (i.e. despite the pre-practise *criterion practise Need-Pos* D_{IRAP} being moderately negative, and thus pro-smoking, unlike the other two groups, $t(15) = -.85$; $p = .41$; $\eta^2 = .05$; $r \approx .21$). Similarly, thereafter, during the IRAP test phase, the thought suppression instructions appeared to have only a borderline and fleeting impact on *Need-Pos* D_{IRAP}. Namely, the pre-practise group remained similarly less than the control group on *Need-Pos* D_{IRAP} not only during the criterion practise blocks (see above), but also during test block pair 1, $t(30) = 1.12$, $p = .27$, $\eta^2 = .04$ (i.e. $r \approx .20$), test block pair 2, $t(30) = 2.22$, $p = .03$, $\eta^2 = .14$ (i.e. $r \approx .38$), and test block pair 3, $t(30) = 1.63$, $p = .11$, $\eta^2 = .08$ (i.e. $r \approx .29$).

Confirming this pattern, the *Need-Pos* D_{IRAP} interaction between the control group and the pre-practise group from criterion practise to test pair 1 was null, $F(1, 30) = .56$, $p = .46$, $\eta_p^2 = .02$ (i.e. $r \approx .14$); and the corresponding *Need-Pos* D_{IRAP} interaction between test pairs 1 and 2 was only borderline moderately-sized in the opposite direction, $F(1, 30) = 1.41$, $p = .25$, $\eta_p^2 = .045$ (i.e. $r \approx .21$); followed by the corresponding *Need-Neg* D_{IRAP} interaction between test pairs 2 and 3 which reversed direction again but only to a null degree, $F(1, 30) = .30$, $p = .59$, $\eta_p^2 = .01$ (i.e. $r \approx .10$).

[76] We highlighted the changing size of the post-practise group's *Need-Pos* D_{IRAP}s relative to zero to provide a preliminary indication of the *qualitative* impact of the thought suppression instructions on *Need-Pos* D_{IRAP} (i.e. whether these instructions succeeded in removing smokers' implicit preference for responding False rather than True to the *Need-Pos* trial-type topic). It is important to note, however, that this approach would not have been interpretable had the size of the control group's *block-pair Need-Pos* D_{IRAP}s not been relatively stable in relation to each other, $2.15 \leq t(15)\text{s} \leq 3.34$; $p\text{s} \leq .05$; $.24 \leq \eta^2\text{s} \leq .43$; $.49 \leq r\text{s} \leq .65$ (see Figure 5.8.3).

Thus, accounting for the relevant baseline discrepancy, the pre-practise instructions reduced the smokers' *criterion practise Need-Pos* D_{IRAP} by a null $\eta^2 = .01$, ($r \approx .11$; i.e. as per the difference observed between the pre- and post-practise groups on *criterion practise Need-Pos* D_{IRAP}); and this led to a null cumulative increase of approximately $\eta_p^2 = .01$ during test block pair 1, a borderline moderate cumulative decrease of approximately $\eta_p^2 = .035$ during test block pair 2, and finally a null cumulative increase by test block 3 of approximately $\eta_p^2 = .025$. Indeed, bearing this out, the pre-practise *Need-Pos* D_{IRAP} started out in the neighbourhood of zero during criterion practise (see above), before then becoming moderately positive during test block pair 1, $t(15) = 1.09$; $p = .30$; $\eta^2 = .07$; $r \approx .27$, and again reducing to the neighbourhood of zero during test block pairs 2 and 3 (i.e. respectively, $t(15)$s = -.11, -.21; ps =.91, .84; η^2s = .0008, .003; rs ≈ .03, .05). As such, the pre-practise instructions appeared to have only a borderline, somewhat delayed and fleeting impact on *Need-Pos* D_{IRAP} during just test block pair 2, and it was in the desired direction. Overall, therefore, it appeared as if the thought suppression instructions initially had null to ironic effects on *Need-Pos* D_{IRAP} for the first three or four trial block pairs after their introduction (i.e. as per both suppressor groups' *block pair Need-Pos* D_{IRAP}s), followed by a fleeting desired effect for one trial block pair which disappeared in the final block pair thereafter (i.e. as per the pre-practise groups' last two *block pair Need-Pos* D_{IRAP}s; see Figure 6.8).

Thought Suppression by Block Sequence on Enjoy-Neg D_{IRAP}

There was a moderately-sized interaction between thought suppression and block sequence on *Enjoy-Neg* D_{IRAP}, $F(6, 135) = 2.71$, $p = .02$, $\eta_p^2 = .11$ (i.e. $r \approx .33$), which qualified a moderate-to-large main effect of thought suppression, $F(2, 45) = 3.10$, $p = .055$, $\eta_p^2 = .12$ (i.e. $r \approx .35$), and a null main effect of block sequence, $F(3, 135) = 1.30$, $p = .28$, $\eta_p^2 = .03$ (i.e. $r \approx .17$). To unpack the nature of this interaction, illustrated in Figure 6.9, we conducted the following planned comparisons to respectively compare *Enjoy-Neg* D_{IRAP} between each pairing of the thought suppression groups with the control group on each level of block sequence

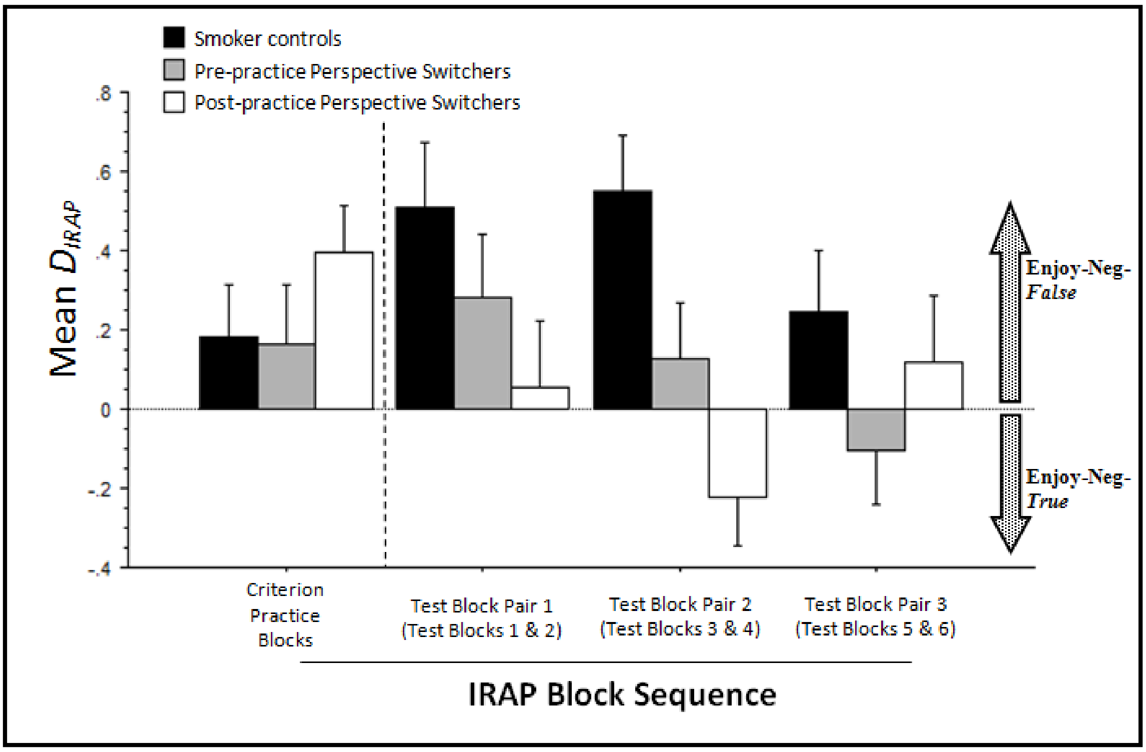

Figure 6.9. The two-way interaction between thought suppression and IRAP trial block sequence on *Enjoy-Neg* D_{IRAP} (with standard error bars).

The post-practise group was moderately greater than the control group on the *criterion practise Enjoy-Neg* D_{IRAP}, $t(30) = -1.20$, $p = .24$, $\eta^2 = .05$; $r = .21$ (i.e. even though they had not yet been introduced to the thought suppression instructions). However, given that the control group and the post-practise group both exhibited positive *criterion practise Need-Neg* D_{IRAP}s (i.e. respectively, $t(15)$s = 1.35, 2.15; ps = .20, .005; η^2s = .11, .42; rs = .33, .65), therefore, on balance, it appeared as though the thought suppression groups were reasonably well matched to control group on *Enjoy-Neg* D_{IRAP}. In any case, upon introducing the thought suppression instructions this moderate difference completely reversed for test block pair 1, $t(30) = 1.94$, $p = .06$, $\eta^2 = .11$ (i.e. $r \approx .33$) and test block pair 2, $t(30) = 4.11$, $p = .0003$, $\eta^2 = .36$ (i.e. $r \approx .60$); before then disappearing during test block pair 3, $t(30) = .56$, $p = .58$, $\eta^2 = .01$ (i.e. $r \approx .10$). Confirming this pattern, planned F-test contrasts revealed that the *Enjoy-Neg* D_{IRAP} interaction between the control group and the post-practise group from criterion practise to test pair 1 was large, $F(1, 30) = 9.67$, $p = .004$, $\eta_p^2 = .24$ (i.e. $r \approx .49$); and that in contrast the interaction between test pairs 1 and 2 was moderate-to-small, $F(1, 30) = 1.44$, $p = .24$, $\eta_p^2 = .05$ (i.e. $r \approx .21$); but that between test pairs 2 and 3 was moderate-to-large and in the opposite direction, $F(1, 30) = 1.65$, $p = .05$, $\eta_p^2 = .13$ (i.e. $r \approx .36$). Indeed, more specifically, the post-practise instructions had cumulatively reduced *Enjoy-Neg* D_{IRAP} by $\eta^2 = .24$ ($r \approx .49$) during test block pair 1 (i.e. as per the relevant F-

test contrast); by approximately $\eta^2 = .29$ ($r \approx .54$) during test block pair 2 (i.e. by adding the η^2-effect for the relevant *F*-test contrast from its immediate predecessor); and had cumulatively decreased *Enjoy-Neg* D_{IRAP} by approximately $\eta^2 = .16$ ($r \approx .40$) during test block pair 3 (i.e. by subtracting the η^2-effect for the relevant *F*-test contrast from its immediate predecessor).

Thus, it appeared as though the post-practise thought suppression instructions had a relatively large immediate impact on *Enjoy-Neg* D_{IRAP}, which strengthened moderately during test block pair 2, and then diminished by a moderate-to-large degree during test pair 3 (see Figure 6.9). However, before drawing this conclusion, it is important to bear in mind that the post-practise group's *Enjoy-Neg* D_{IRAP} became largely negative during test block pair 2, $t(15) = -1.78$, $p = .09$, $\eta^2 = .18$, $r = .42$. Thus, crucially, the post-practise thought suppression instructions appeared to have an ironic impact on *Enjoy-Neg* D_{IRAP} during test block pair 2, inducing a type of pro-smoking implicit evaluating that was not otherwise present. This was in contrast to the post-practise group's largely positive *Enjoy-Neg* D_{IRAP} during criterion practise (see above), which reduced to the neighbourhood of zero during test block pair 1, $t(15) = .33$, $p = .74$, $\eta^2 = .007$, $r = .08$, and test block pair 3, $t(15) = .68$, $p = .51$, $\eta^2 = .03$, $r = .17$.[77]

The pre-practise group, by contrast, remained non-negative on *Enjoy-Neg* D_{IRAP} throughout all four block pairs. Namely, they exhibited an *Enjoy-Neg* D_{IRAP} that was moderately positive during criterion practise, $t(15) = 1.11$, $p = .29$, $\eta^2 = .07$, $r \approx .26$, and during test pairs 1 and 2 (i.e. respectively, t[15]s = 1.79, .89; ps = .09, .39; η^2s = .18, .05; rs ≈ .42, .22), but in the neighbourhood of zero during test block pair 3, $t(15) = -.74$, $p = .47$, $\eta^2 = .03$, $r \approx .18$. And although the pre-practise group were similar to the control group on the *Enjoy-Neg* D_{IRAP} during the criterion practise blocks, $t(30) = .08$, $p = .94$, $\eta^2 = .0002$ (i.e. $r \approx .01$), crucially, they were moderately less than the baseline control provided by the post-practise group, $t(30) = -1.21$, $p = .24$, $\eta^2 = .09$ (i.e. $r \approx .30$). On balance, therefore, given randomly assignment between the two thought suppression groups, it appeared that the pre-practise group's *criterion practise Enjoy-Neg* D_{IRAP} was moderately reduced by the thought suppression instructions.

[77] And moreover, this was despite the fact that the control group's *Enjoy-Neg* D_{IRAP} changed in the opposite direction to the post-practise group's *Enjoy-Neg* D_{IRAP} across all three block pair transitions. That is, the control group's *Enjoy-Neg* D_{IRAP} was largely positive during test block pairs 1 and 2 (i.e. respectively, $t(15)$s = 3.13, 3.91; ps ≤ .007; η^2s = .40, .50; rs = .63, .71), but only moderately so during test block pair 3, $t(15) = 1.60$, $p = .13$, $\eta^2 = .15$, $r = .38$.

Thereafter, during the IRAP test phase, the pre-practise group's *Enjoy-Neg* D_{IRAP} trended less than the control group during test block pair 1, $t(30) = 1.01$, $p = .32$, $\eta^2 = .03$ (i.e. $r \approx .18$), and moderately less than the control group during test block pairs 2 and 3 (i.e. respectively, $t(30)$s = 2.13, 1.68; ps = .04, .10; η^2s = .13, .09; rs ≈ .36, .30). Confirming this pattern of gradual reduction, planned F-tests revealed that the *Enjoy-Neg* D_{IRAP} interaction between the control group and the pre-practise group from criterion practise to test pair 1 was relatively small, $F(1, 30) = .67$, $p = .42$, $\eta_p^2 = .02$ (i.e. $r \approx .15$); as was that between test pairs 1 and 2, $F(1, 30) = .58$, $p = .45$, $\eta_p^2 = .02$ (i.e. $r \approx .14$); followed by an even smaller *Need-Neg* D_{IRAP} interaction in the same direction between test pairs 2 and 3, $F(1, 30) = .08$, $p = .78$, $\eta_p^2 = .003$ (i.e. $r \approx .05$). Thus, by accounting for the relevant baseline discrepancy, the pre-practise thought suppression instructions reduced the smokers' *criterion practise Enjoy-Neg* D_{IRAP} by approximately $\eta^2 = .09$ ($r \approx .30$; i.e. as per the difference observed between the pre- and post-practise groups on *criterion practise Need-Pos* D_{IRAP}); and this led to a cumulative reductions of approximately $\eta_p^2 = .11$, $\eta_p^2 = .13$ and $\eta_p^2 = .127$ during test block pairs 1, 2 and 3, respectively. On balance, therefore, the thought suppression instructions appeared to immediately, persistently and indeed ironically eliminate the smokers' positive *Enjoy-Neg* effect across a sequence of approximately 5-6 trial block pairs after the introduction of those instructions. And moreover, the impact of those instructions was consistent across all of such trial block pairs except for one; an ironic pro-smoking *Enjoy-Neg* effect on post-practise IRAP test block pair 2 (see Figure 6.9).

Thought Suppression by Block Sequence on Enjoy-Pos D_{IRAP}

There was no main effect of thought suppression, $F(2, 45) = .13$, $p = .87$, $\eta_p^2 = .006$ (i.e. $r \approx .08$), or block sequence on *Enjoy-Pos* D_{IRAP}, $F(3, 135) = .19$, $p = .91$, $\eta_p^2 = .004$ (i.e. $r \approx .06$); and indeed the relevant two-way interaction was also null, $F(6, 135) = .15$, $p = .99$, $\eta_p^2 = .007$ (i.e. $r \approx .08$). Bearing this out, all four *block pair Enjoy-Pos* D_{IRAP} for all three groups were positive to a large degree, $t(15)$s ≥ 2.33; ps ≤ .03; η^2s ≥ .27; rs ≥ .52.

6.8.4 IRAP trial-type by Thought suppression by Order Analyses

We entered the *standard trial-type* D_{IRAP} data into a 3x2x2x2 mixed-repeated measures ANOVA, which crossed the two between-groups variables *thought suppression* (i.e. smoker controls versus pre-practise perspective switchers versus post-practise perspective switchers) and *trial block order* (i.e. pro-smoking-first versus anti-smoking-first), with the two IRAP trial-type variables.[78] We obtained no main effect for order, $F(1, 42) = .34$, $p = .56$, $\eta_p^2 = .008$ (i.e. $r \approx .09$), or for attribute stimulus class, $F(1, 42) = .09$, $p = .76$, $\eta_p^2 = .002$ (i.e. $r \approx .05$), but did for the thought suppression variable, $F(2, 42) = 5.68$, $p = .007$, $\eta_p^2 = .21$ (i.e. $r \approx .46$), and marginally for concept label, $F(1, 42) = 1.59$, $p = .21$, $\eta_p^2 = .04$ (i.e. $r \approx .19$). And critically, all four main effects were qualified by a moderately sized interaction effect with each other, as illustrated in Figure 6.10, $F(2, 42) = 1.93$, $p = .16$, $\eta_p^2 = .08$ (i.e. $r \approx .29$), and by multiple lower order interaction effects.[79]

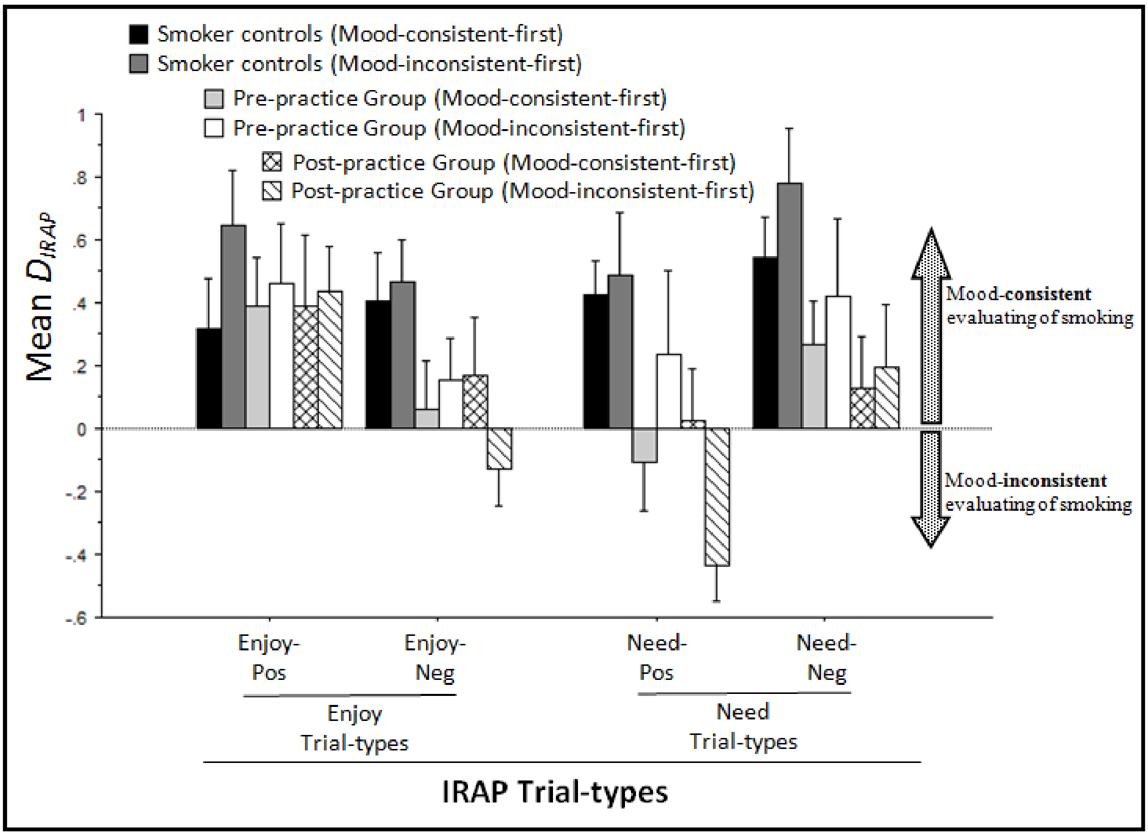

Figure 6.10. The three-way interaction between the thought suppression, block order and the two IRAP trial-type variables in terms of mean *standard trial-type* D_{IRAP} scores with standard error bars.

[78] We did not include IRAP block order in an analysis with IRAP block sequence in the current study for the same reasons as cited in Study 3 (i.e. related to the larger sample sizes that would have been required to compensate for the relatively few pairs of latency scores comprising each *block-pair trial-type* D_{IRAP} coupled with the instability of block order effects; see Lane et al., 2007; pp. 88-90; Nosek et al., 2005; VanVoorhis & Morgan, 2007, p. 48).

[79] Namely, there was a moderate three-way interaction between thought suppression and the two trial-type variables $F(2, 42) = 1.77$, $p = .18$, $\eta_p^2 = .08$ (i.e. $r \approx .28$); a large two-way interaction effect between the two trial-type variables, $F(1, 42) = 22.51$, $p < .0001$, $\eta_p^2 = .35$ (i.e. $r \approx .59$); a moderate-to-large two-way interaction between thought suppression and concept label, $F(2, 42) = 3.47$, $p = .04$, $\eta_p^2 = .14$ (i.e. $r \approx .38$); a moderate three-way interaction between block order and the two trial-type variables, $F(1, 42) = 2.70$, $p = .11$, $\eta_p^2 = .06$ (i.e. $r \approx .25$); and finally, a moderate two-way interaction between thought suppression and block order, $F(2, 42) = 1.15$, $p = .21$, $\eta_p^2 = .05$ (i.e. $r \approx .23$). All six remaining interaction effects were null, $Fs \leq .50, .204$, $ps \geq .61$, $\eta_p^2 s \leq .02$ (i.e. $rs \leq .15$).

In broad terms, this indicated that all four *trial-type* D_{IRAP} scores functioned differently from each other with respect to the interaction between the thought suppression and block order variables. To explore the nature of this four-way interaction, we therefore conducted a one-way follow-up ANOVA on each *trial-type* D_{IRAP} crossing thought suppression and block order.

Thought Suppression by Block Order on Need-Neg D_{IRAP}

There was a moderate-to-large main effect of thought suppression on *Need-Neg* D_{IRAP}, $F(2, 42) = 3.83$, $p = .03$, $\eta_p^2 = .15$ (i.e. $r \approx .39$); no main effect of block order, $F(1, 42) = 1.04$, $p = .31$, $\eta_p^2 = .02$ (i.e. $r \approx .15$); and indeed, only a null interaction between block order and thought suppression, $F(2, 42) = .11$, $p = .90$, $\eta_p^2 = .005$ (i.e. $r \approx .07$). Planned comparisons indicated that the pre- and post-practise groups exhibited similarly sized *Need-Neg* D_{IRAP}s, $t(30) = .87$, $p = .39$, $\eta^2 = .02$ (i.e. $r \approx .16$), that were respectively a moderate and large degree less than the control group's *Need-Neg* D_{IRAP} (i.e. respectively, $t(30)$s = 1.96, 2.81; one-tailed ps = .03, .004; η^2s = .11, .21; rs ≈ .34, .47). However, as we highlighted in our block sequence analysis above, the post-practise group was moderately less than the control group on *criterion practise Need-Neg* D_{IRAP} to begin with (i.e. before the post-practise neither group had received the thought suppression instructions; $\eta^2 = .09$, $r \approx .31$). And, therefore, we estimated that the overall impact of the thought suppression instructions on the pre- and post-practise group's respective *Need-Neg* D_{IRAP}s were null and moderate, respectively, *revised-*η^2 = .02, .12; *revised-r*s = .14, .35. Indeed, accordingly, the control group and the pre-practise group both exhibited a largely positive *Need-Neg* D_{IRAP} (i.e. respectively; $t[15]$s = 6.12, 2.58; ps ≤ .02; η^2s = .71, .31; rs ≈ .84, .55), but in contrast, the post-practise group merely exhibited a moderately positive *Need-Neg* D_{IRAP}, t(15) = 1.21, p =.24, η^2 = .09, r ≈.30 (i.e. much the same as at criterion practise baseline).

Thought Suppression by Block Order on Need-Pos D_{IRAP}

Thought suppression interacted with block order on *Need-Pos* D_{IRAP}, $F(2, 42) = 2.85$, $p = .07$, $\eta_p^2 = .12$ (i.e. $r \approx .35$), and this interaction qualified a large main effect of thought suppression, $F(2, 42) = 7.82$, $p = .001$, $\eta_p^2 = .27$ (i.e. $r \approx .52$), and a null main effect of block order, $F(1, 42) = .02$, $p = .89$, $\eta_p^2 = .0004$ (i.e. $r \approx .02$). Follow-up t-tests indicated that block order did not impact *Need-Pos* D_{IRAP} in the control group, $t(14) = -.28$, $p = .78$, $\eta^2 = .006$ (i.e. $r \approx .07$), but had an opposing impact on the pre-practise group, $t(14) = -1.18$, $p = .26$, $\eta^2 = .09$ (i.e. $r \approx .30$), relative to the post-practise group, $t(14) = 2.41$, $p = .03$, $\eta^2 = .29$ (i.e. $r \approx .54$). Namely, planned comparisons indicated that the pre- and post-practise groups exhibited similarly-sized *Need-Pos* D_{IRAP}s in the mood-consistent-first block order, $t(13) = -.56$, $p = .58$, $\eta^2 = .02$, $r \approx .15$, but largely different *Need-Pos* D_{IRAP}s from each other in the mood-inconsistent-first block order, $t(15) = 2.63$, $p = .02$, $\eta^2 = .32$, $r \approx .56$. As illustrated in Figure 6.10, the pre-practise group exhibited a moderately negative *Need-Pos* D_{IRAP} in the mood-consistent-first condition, $t[8] = -.70$, $p = .51$, $\eta^2 = .06$, $r \approx .24$, but a moderately positive *Need-Pos* D_{IRAP} in the mood-inconsistent-first condition, $t(6) = .89$, $p = .41$, $\eta^2 = .12$, $r \approx .34$. And conversely, the post-practise group exhibited a *Need-Pos* D_{IRAP} that was in the neighbourhood of zero during the mood-consistent-first condition, $t(5) = .14$, $p = .89$, $\eta^2 = .004$, $r \approx .06$, but negative to a large degree during the mood-inconsistent-first condition, $t(9) = -3.96$, $p = .03$, $\eta^2 = .64$, $r \approx .80$. In fact, the latter effect was as negative as the control group's *Need-Pos* D_{IRAP} was positive across both block orders (i.e. $t[15] = 4.22$; $p = .001$; $\eta^2 = .54$; $r \approx .74$)

Thus, as illustrated in Figure 6.10, the pre-practise instructions appeared to reduce the smokers' *Need-Pos* D_{IRAP} by a large degree (relative to the control group) in the mood-consistent-first block order, $t(15) = 2.78$, $p = .01$, $\eta^2 = .34$ (i.e. $r \approx .58$), but only to a moderate-to-small degree in the mood-inconsistent block order, $t(13) = .78$, $p = .45$, $\eta^2 = .045$ (i.e. $r \approx .21$). And moreover, in contrast, the post-practise instructions appeared to reduce *Need-Pos* D_{IRAP} by a large amount in the mood-consistent-first block order, $t(12) = 2.14$, $p = .05$, $\eta^2 = .28$ (i.e. $r \approx .53$); and by a very large amount that effectively reversed the polarity of *Need-Pos* D_{IRAP} in the mood-inconsistent-first block order, $t(16) = 4.33$, $p = .001$, $\eta^2 = .54$ (i.e. $r \approx .73$). In other words, during the pre-practise mood-inconsistent-first condition and the post-practise mood-inconsistent-first condition, the thought suppression instructions appeared to reduce how mood-consistent *Need-Pos* D_{IRAP} was without making it negative (or thus ironically pro-smoking); and in contrast, the pre-practise mood-consistent-first and the post-practise mood-inconsistent-first conditions both appeared to make *Need-Pos* D_{IRAP} negative.

Indeed, we obtained broadly the same findings even when we accounted for the fact that the post-practise group was moderately less than the control group on *criterion practise Need-Pos* D_{IRAP} (i.e. before either group had received the thought suppression instructions; $\eta^2 = .05$; $r = .23$). Namely, our revised estimates indicated that the overall reduction on *Need-Pos* D_{IRAP} due to the thought suppression instructions remained large in pre-practise mood-consistent-first condition (i.e. *revised*-$\eta^2 = .29$, *revised*-$r \approx .54$), large in the post-practise mood-consistent-first block order (i.e. *revised*-$\eta^2 = .23$, *revised-r* ≈ .48), and also large in the post-practise mood-consistent-first block order (i.e. *revised-*η^2 = .49, *revised-r* ≈ .70). Only the pre-practise mood-inconsistent-first group did not exhibit any overall impact of the thought suppression instructions once we had accounted for the aforementioned baseline discrepancy (i.e. *revised-*η^2 = .005, *revised-r* ≈ -.07). Thus, crucially, the only condition in which smokers' *Need-Pos* D_{IRAP}s were made less mood-consistent without making them pro-smoking was in the post-practise mood-consistent-first condition. And apart from the null effect of the pre-practise mood-inconsistent-first condition, both other thought suppression conditions ironically induced pro-smoking implicit evaluating among the current smokers (i.e. inducing a tendency to affirm one's need to smoke when experiencing positive craving-related emotions).

Thought Suppression by Block Order on Enjoy-Neg D_{IRAP}

Thought suppression interacted to a moderate degree with block order on *Enjoy-Neg* D_{IRAP}, $F(2, 42) = 1.10$, $p = .34$, $\eta_p^2 = .05$ (i.e. $r \approx .22$), thus qualifying a large main effect of thought suppression, $F(2, 42) = 4.47$, $p = .02$, $\eta_p^2 = .18$ (i.e. $r \approx .42$), and a null main effect block order, $F(1, 42) =.16$, $p = .69$, $\eta_p^2 = .004$ (i.e. $r \approx .06$). Follow-up *t*-tests indicated that block order did not impact *Enjoy-Neg* D_{IRAP} in either the control group, $t(14) = -.30$, $p = .77$, $\eta^2 = .006$ (i.e. $r \approx .08$), or the pre-practise group, $t(14) = -.45$, $p = .66$, $\eta^2 = .01$ (i.e. $r \approx .12$), but did have a moderate impact on *Enjoy-Neg* D_{IRAP} in the post-practise group, $t(14) = 1.48$, $p = .16$, $\eta^2 = .14$ (i.e. $r \approx .38$).

Planned comparisons indicated that the pre- and post-practise groups exhibited similarly-sized *Enjoy-Neg* D_{IRAP}s in the mood-consistent-first block order, $t(13) = -.46$, $p = .65$, $\eta^2 = .02$, $r \approx .13$, but moderately different *Enjoy-Neg* D_{IRAP}s from each other in the mood-inconsistent-first block order, $t(15) = 1.61$, $p = .13$, $\eta^2 = .15$, $r \approx .38$ (see Figure 6.10). In particular, the mood-inconsistent-first post-practise group exhibited a moderate-to-large negative *Enjoy-Neg* D_{IRAP}, $t(9) = -1.13$, $p = .28$, $\eta^2 = .12$ (i.e. $r \approx .35$), but the mood-consistent-first post-practise group and both block order pre-practise groups exhibited *Enjoy-Neg* D_{IRAP}s that at least trended positively. That is, the mood-inconsistent-first pre-practise group exhibited a moderate-to-large positive *Enjoy-Neg* D_{IRAP}, $t(6) = 1.17$, $p = .29$, $\eta^2 = .15$, $r \approx .38$, as did the mood-consistent-first post-practise group, $t(5) = .95$; $p = .39$, $\eta^2 = .15$, $r \approx .39$; and the mood-consistent-first pre-practise group trended towards a positive *Enjoy-Neg* D_{IRAP}, $t[8] = .38$, $p = .71$, $\eta^2 = .02$, $r \approx .13$.

Crucially, the control group exhibited a strongly positive *Enjoy-Neg* D_{IRAP} in both block orders, $t(15) = 4.40$, $p = .001$, $\eta^2 = .56$ (i.e. $r \approx .75$), and thus, it appeared that pre- and post-practise thought suppression each reduced *Enjoy-Neg* D_{IRAP} by at least a moderate degree. Specifically, *Enjoy-Neg* D_{IRAP} was reduced, relative to the control group, by a moderate-to-large degree across the pre-practise block order groups, $t(30) = 2.33$, $p = .03$, $\eta^2 = .15$ (i.e. $r \approx .39$), to a moderate degree for the mood-consistent-first post-practise group, $t(12) = .98$, $p = .35$, $\eta^2 = .07$ (i.e. $r \approx .27$), and indeed, to a large degree for the mood-inconsistent-first post-practise group, $t(16) = 3.39$, $p = .004$, $\eta^2 = .42$ (i.e. $r \approx .65$). Indeed, these reductions were even larger when we accounted for the fact that the post-practise group was moderately more than the control group on *criterion practise Enjoy-Neg* D_{IRAP} (i.e. before either group had received the thought suppression instructions; $\eta^2 = .09$, $r \approx .30$). Namely, our revised-estimates indicated that the overall reduction on *Enjoy-Neg* D_{IRAP} due to the thought suppression instructions was large for the pre-practise group (i.e. *revised-*$\eta^2 = .24$, *revised-*$r \approx .49$), and indeed large for both post-practise block orders (i.e. respectively, *revised-*η^2s = .16, .51; *revised-r*s ≈ .40, .71). Thus, in summary, the pre-practise thought suppression instructions reduced the degree to which smokers were mood-consistent on *Enjoy-Neg* D_{IRAP} without making them ironically pro-smoking in either block order. And the post-practise thought suppression instructions did so only in the mood-consistent-first block order. However, by contrast, the post-practise mood-inconsistent-first condition ironically induced negative *Enjoy-Neg* D_{IRAP}s, such that smokers implicitly affirmed that they enjoy smoking when they experience negative craving-related feelings.

Thought Suppression by Block Order on Enjoy-Pos D_{IRAP}

There was no main effect of either thought suppression or block order on *Enjoy-Pos* D_{IRAP}, *F*s ≤ 1.14, *p*s ≥ .29, η_p^2s ≤ .03 (i.e. *r*s ≤ .16), and moreover no interaction, $F(2, 42) = .41$, $p = .67$, $\eta_p^2 = .02$ (i.e. $r \approx .14$). Planned comparisons confirmed that all three groups exhibited very similarly-sized *Enjoy-Pos* D_{IRAP}s, *t*(30)s ≤ .37, *p*s ≥ .72, η^2s ≤ .005 (i.e. *r*s ≤ .07). Namely, *Enjoy-Pos* D_{IRAP} was positive to a large degree for not just the control group, $t(15) = 3.92$, $p = .001$, $\eta^2 = .51$ (i.e. $r \approx .71$), but also for both the pre- and post-practise groups (i.e. respectively, *t*[15]s = 3.60, 3.56; *p*s = .003, .003; η^2s = .46, .46 (i.e. *r*s ≈ .68, .68).

Response Latency Analyses of Thought Suppression and Block Order

As in Study 3, follow-up analyses suggested that the thought suppression instructions made their impact on all three of these *trial-type* D_{IRAP}s by delaying (i.e.

interfering with) mood-consistent IRAP responses more than by mood-inconsistent IRAP responses (i.e. rather than by facilitating mood-inconsistent relative to mood-consistent IRAP responding; for details see Appendix 22). In contrast, however, block order operated differently depending upon not just thought suppression, but also upon the particular *trial-type* D_{IRAP} involved. Whereas the pre-practise block order effect on *Need-Pos* D_{IRAP} occurred mainly by inducing faster mood-inconsistent IRAP responding, both post-practise block order effects (i.e. on *Need-Pos* D_{IRAP} and *Enjoy-Neg* D_{IRAP}) occurred mainly by interfering with (i.e. slowing) mood-consistent IRAP responding.

6.8.5. The Impact of the Thought Suppression Instructions on *trial-type* D_{IRAP} Internal Reliability

The thought suppression instructions did not appear to have much if any overall impact on the internal reliability of the current *standard trial-type* D_{IRAP}s[80] except for upon *Enjoy-Neg* D_{IRAP}. Namely, as detailed in Table 6.1, both thought suppression groups exhibited similarly good internal reliability as the control group on all of the current *trial-type* D_{IRAP}s except for *Enjoy-Neg* D_{IRAP}, where they each exhibited an almost identically moderate amount less internal reliability than the control group, respectively, Zs = 1.25, 1.33, ps = .21, .18 (i.e. rs ≈ .22, .24).

[80] Note that it would have been possible to calculate *Spearman-Brown corrected* split-half reliabilities for each group's *extended trial-type* D_{IRAP}s. However, the pre-practise group was instructed to perspective switch during the criterion practise blocks but the post-practise and control groups were not, and therefore it would have confounded the thought suppression instructions (with time since their delivery) to have compared these three groups with respect to the internal reliabilities of their respective *extended trial-type* D_{IRAP}s.

Table 6.1
The Spearman-Brown split-half reliabilities (r_{sb}) of the IRAP trial-type effects for the control group, and the pre- and -post thought suppression groups. 'Compensated D_{IRAP} r_{sb}s' approximated what r_{sb}s the various D_{IRAP}s would have if, with all else equal, they were comprised of the same number of trials as an IAT effect (for algorithm see Appendix 11).

	D_{IRAP} r_{sb}	Compensated D_{IRAP} r_{sb}
Control Group		
Enjoy-Pos D_{IRAP} [a]	.87****	96****
Enjoy-Neg D_{IRAP} [a]	.62**	.87****
Need-Pos D_{IRAP} [a]	.47*	.78***
Need-Neg D_{IRAP} [b]	.66**	.89****
Pre-practise Group		
Enjoy-Pos D_{IRAP} [a]	.77***	.93****
Enjoy-Neg D_{IRAP} [a]	.23	.54*
Need-Pos D_{IRAP} [a]	.67**	.89****
Need-Neg D_{IRAP} [a]	.70***	.90****
Post-practise Group		
Enjoy-Pos D_{IRAP} [a]	.66**	.89****
Enjoy-Neg D_{IRAP} [a]	.20	.50*
Need-Pos D_{IRAP} [a]	.49*	.79***
Need-Neg D_{IRAP} [a]	.78***	.93****

[a] $n = 16$; [b] $n = 14$. [#] $p \leq .10$, * $p \leq .05$; ** $p \leq .01$; *** $p \leq .001$; **** $p \leq .0001$; all ps for r_{sb} one-tailed.

6.9. DISCUSSION (STUDY 4)

Manipulation checks confirmed that the current smokers experienced relatively high cognitive load during the IRAP-based thought suppression tasks – with approximately 67-87% of the smokers from the pre- and post-practise groups offering unsolicited reports that they were unable to avoid adhering less and less to the thought suppression instructions as the IRAP progressed. Moreover, approximately 47-60% of the thought suppression groups even offered unsolicited reports that they had found the thought suppression instructions so difficult to adhere to during the IRAP that they ceased attempting to do so well before completion. In addition, further manipulation checks indicated that the current smokers were remarkably consistent in deriving the perspective of a lifelong non-smoker as being relatively free from the symptoms of tobacco addiction; and as being a perspective from which smoking is evaluated as neither enjoyable nor needed even during corresponding craving-related moods. Indeed, more specifically, whereas over three quarters of the current smokers mentioned deriving anti-smoking scenarios as a means of imagining perspectives from which they would not typically feel a need to smoke, only about a third mentioned deriving any scenarios on the benefits of abstinence and/or smoking-bans in order to do so.

Therefore, much like as in Study 3, it appeared as though the current smokers' experience of non-smokers was predominantly stigmatizing, rather than socially supportive in ways that are likely to foster abstinence (see Gifford & Humphreys, 2007, *p.* 359). And crucially, this stigmatizing approach to contradiction-based thought suppression had limited effectiveness as a means for smokers to temporarily eliminate their pro-smoking implicit evaluating on demand. Specifically, this tactic was initially successful in eliminating smokers' positive *Need-Neg* effects for the first two pairs of IRAP blocks after its introduction. However, thereafter it did so only in an oscillating manner from block pair to block pair for the duration of the relevant IRAPs. Indeed, these thought suppression instructions appeared to have even less of the desired impact on smokers' implicit evaluating of *Need-Pos*. Namely, after a delay of three or four trial block pairs during which the smokers' *Need-Pos* effects reduced ironically, sometimes even transforming into pro-smoking effects, the smokers' *Need-Pos* effects only increased as desired during the penultimate IRAP trial block pair measured here, before disappearing in the last such trial block pair.

Worse still, the thought suppression instructions ironically eliminated the smokers' positive (and thus anti-smoking) *Enjoy-Neg* effects immediately and persistently throughout a sequence of approximately 5-6 trial block pairs following their introduction. Crucially, this was despite the fact that the thought suppression instructions were specifically designed to be irrelevant to the smokers' enjoyment of smoking. Indeed, in one trial block pair, approximately 4-5 trial block pairs after the introduction of those instructions, the smokers' positive *Enjoy-Neg* effect not only reduced to the neighbourhood of zero, but transformed into a negative and thus pro-smoking *Enjoy-Neg* effect.

In summary, therefore, the current thought suppression instructions may have initially had the desired impact on the smokers' implicit evaluating of the *Need-Neg* topic, but this benefit was unstable. And moreover, it was bought at the cost of ironically prompting the smokers to implicitly evaluate the *Need-Pos* and *Enjoy-Neg* trial-types in a consistently less anti-smoking, and indeed sometimes more pro-smoking manner than before. However, the smokers' implicit evaluating of the *Enjoy-Pos* trial-type were entirely unaffected by the thought suppression instructions. And as such, crucially, it appeared as though the thought suppression instructions primarily cued smokers to (temporarily) derive evaluative perspectives that were compatible with smokers' pre-experimentally established implicit evaluating of *Enjoy-Pos*, but incompatible with their pre-experimentally established implicit evaluating of *Need-Neg*.

For example, as a result of trying to imagine that they did not need to smoke when feeling negative, it is possible that the current smokers may have temporarily reverted to implicitly evaluating smoking during negative craving-related moods as being enjoyable for the relief it provides – as is characteristic of the early stages of tobacco addiction. Indeed, not only did the current pattern of trial-type effects reflect the relatively pro-smoking enjoyment-related implicit evaluating exhibited by undergraduate non-smokers in Study 2 (who were at risk of smoking by virtue of being undergraduate students). Moreover, just as these non-smokers from Study 2 exhibited relatively high internal reliability on all of the current trial-types except *Enjoy-Neg*, likewise so did both of the thought suppression groups – thus indicating that all three groups were similarly conflicted in responding to *Enjoy-Neg*. Furthermore, if the current smokers had switched to implicitly evaluating from such perspectives emphasizing reward- over relief-focused smoking, this would also explain why having received the thought suppression instructions they were less likely to deny, and more likely to affirm *Need-Pos*. On balance, therefore, it appeared as though the thought suppression instructions may simply have prompted the current smokers to temporarily revert from the need-focused pro-smoking perspectives characteristic of advanced tobacco addiction, to the enjoyment-focused pro-smoking perspectives characteristic of the earliest stages of tobacco addiction. In fact, this seemed all the more likely from the point of view that all of the aforementioned effects of thought suppression on smokers' implicit evaluating of *Need-Neg, Need-Pos* and *Enjoy-Neg* occurred more strongly in response to the post-practise instructions as compared to the pre-practise instructions.

Namely, the pre-practise thought suppression instructions reduced the smokers' *standard Need-Neg* effect by a moderate amount, but the pre-practise instructions did not have any overall impact on the smokers' standard *Need-Neg* effect (apart from undermining its internal reliability). And correspondingly, the most pronounced ironic effects of thought suppression on smokers' implicit evaluating of *Need-Pos* and *Enjoy-Neg* were exhibited by the post-practise group rather than by the pre-practise group. Indeed, even with the post-practise group the relevant ironic instructional effects were expressed most strongly immediately after instruction, and tended to fade gradually over successive IRAP block pairs thereafter. As such, crucially, this pattern of ironic effects suggested that the current thought suppression instructions inadvertently encouraged smokers to temporarily derive reward-focused pro-smoking scenarios. Therefore, the current thought suppression instructions may not have provoked the kind of collateral, delayed rebound effects that we observed in smokers' implicit evaluating during Study 3 as thought suppression waned over time; but they certainly did have more immediate ironic effects on smokers' smoking-related implicit evaluating (of *Need-Pos* and *Enjoy-Neg*). In fact, as we will now detail, these ironic perspective switching processes persisted so long that they were often apparent on the smokers' standard *Need-Pos* and *Enjoy-Neg* effects (i.e. which covered a timeframe of at least 15-20 minutes).

Although the smokers' standard *Need-Neg* and *Enjoy-Pos* effects were not affected by IRAP block order sequence, as intended, their standard *Need-Pos* and *Enjoy-Neg* effects were complicated by interactions with this variable. In summary, the post-practise group exhibited an ironic negative effect on both *Enjoy-Neg* and *Need-Pos* in the mood-inconsistent-first block order, and by contrast the pre-practise group did so during the mood-consistent-first block order; but crucially, all other thought suppressor *Enjoy-Neg* and *Need-Pos* effects trended similarly positive. Thus, curiously, block order appeared to moderate (the impact of thought suppression on) the pre- and post-practise *Need-Pos* effects in opposite directions to each other, and moderated the relevant post-practise *Enjoy-Neg* (thought suppression) effect without moderating its pre-practise counterpart.

More specifically, during the post-practise IRAP measurement phase the mood-inconsistent-first block order appeared to amplify the ironic reductions we observed in the smokers' *Enjoy-Neg* and *Need-Pos* effects so much that unlike the consistent-first block order it generated pro-smoking (negative) *Enjoy-Neg* and *Need-Pos* effects. And conversely, the thought suppression instructions transformed the standard pre-practise *Need-Pos* effect from anti-smoking (positive) to pro-smoking (negative) in the mood-consistent-first IRAP block order, but did not have a persistent enough impact in the mood-inconsistent-first block order to change the standard pre-practise *Need-Pos* effect at all. Overall, this implied that block order moderated (the impact of thought suppression on) smokers' pro-smoking implicit evaluating less the longer it had been since the relevant thought suppression instructions had been issued. And as such, crucially, this confirmed that the current IRAP block order instructional effects were a function of the thought suppression instructions, rather than vice versa.

Granted, it was not possible within the current design to determine what changes in perspective were responsible for the pre-practise block order interaction on *Need-Pos* being in the opposite direction to the two post-practise block order interactions on *Need-Pos* and *Enjoy-Neg*. There were many possible ways in which each smokers' personal scenario might have evolved to produce the current block order interactions. And as such, we must acknowledge that it is possible that this localised pattern of block order effects may not be replicable, but rather relatively idiosyncratic, given that many of the relevant block order cells happened to contain relatively few participants (i.e. the relevant three ironic negative effects were respectively based upon c*ell n*s of 9, 8 and 6).

Nonetheless, it is worth noting that the post-practise block order interactions were compatible with our tentative account above of how the current thought suppression instructions temporarily cued the smokers to reframe their implicit evaluating of *Enjoy-Neg* and *Need-Pos* in terms of enjoying smoking. Namely, it is possible that the more pronounced ironic effects we observed on post-practise *Enjoy-Neg* and *Need-Pos* effects in the mood-inconsistent-first block order as compared to its mood-consistent-first counterpart, was due to the latter block order being more immediately relevant to that enjoyment-focused strategy – and thus more likely to cue smokers to derive it. Indeed, confirming this, follow-up analyses indicated that both of the post-practise block order effects resulted from the same response processes as all of the post-practise thought suppression effects. Namely, they resulted from the mood-inconsistent-first condition amplifying the extent to which the thought suppression instructions interfered with (i.e. slowed down) smokers' pre-experimentally established implicit evaluating in each case.

By contrast, the only block order effect to emerge among pre-practise group's IRAP data, was that on their implicit evaluating of *Need-Pos*, which operated by facilitating (i.e. speeding up) smokers' affirming of *Need-Pos* (i.e. contrary to their pre-experimentally established tendency to deny *Need-Pos*). And thus, it appeared that as the (pre-practise) smokers' tired of adhering to the current thought suppression instructions, the mood-consistent-first block order somehow cued the relevant enjoyment-focused thought suppression scenarios more persistently among the smokers than the mood-inconsistent-first block order. Overall, therefore, as in Study 3, the current pattern of IRAP block order effects comported closely with the idea that thought suppression instructions interact with IRAP block order according to how relevant and thus supportive each block order is to the ongoing impact of those instructions on the relevant IRAP trial-type (at any given time).

In any case, unfortunately, these foregoing block order interactions appeared to exacerbate rather than ameliorate the ironic impact of our need-focused thought suppression instructions on smokers implicit evaluating of *Enjoy-Neg* and *Need-Pos*. Granted, the current thought suppression instructions influenced smokers' pro-smoking implicit evaluating in a manner that was, at least initially, more robust against interference from block order, and thus other background variables, than those used in Study 3. Namely, block order did not interact with the current thought suppression instructions on smokers' *Need-Neg* and *Enjoy-Pos* effects; and moreover, even though it did on the smokers' *Need-Pos* and *Enjoy-Neg* effects, the relevant interactions were relatively small as compared to the main effect of thought suppression in each case. However, even so, the thought suppression instructions only temporarily eliminated the smokers' pro-smoking implicit evaluating of *Need-Neg*, all while they inadvertently provoked the smokers to implicitly evaluate *Need-Pos* and *Enjoy-Neg* in a more pro-smoking manner. Indeed, whatever about temporarily eliminating the smokers' pro-smoking implicit evaluating of *Need-Neg* on demand, certainly at no point did these instructions induce the smokers' to implicitly evaluate *Need-Neg* in an anti-smoking manner. In conclusion, therefore, the focused thought suppression examined here may have been more effective in temporarily eliminating implicit evaluating integral to tobacco addiction than the relatively ad hoc thought suppression examined in Study 4. However, even this more focused thought suppression tactic nevertheless still had drawbacks, insofar as it immediately and relatively persistently induced ironic effects on secondary aspects of implicit evaluating related to tobacco addiction.

6.10. GENERAL DISCUSSION: CHAPTER 6

The research contained within the current chapter provided a first glimpse into the dynamics of how thought suppression specifically impacts the intrusive aspects of implicit evaluating that it is generally used to contradict. In particular, from the point of view that thought suppression may sometimes be useful in temporarily postponing (tobacco) cravings, we examined how immediately and persistently effective it would be in eliminating implicit evaluating integral to tobacco addiction. In summary, we found that the relatively ad hoc approach to thought suppression examined in Study 3 only very temporarily eliminated implicit evaluating integral to tobacco addiction; and moreover not on demand but only after intermittent delays. To illustrate, smokers had been engaging in ad hoc thought suppression for approximately 5 minutes before it had any impact on their pro-smoking *Smoking-Pos* effect (i.e. based on the fact that on average, it took the thought suppressors about 2.3 minutes to complete each trial block pair on the relevant IRAP). And even then, when the smokers' positive *Smoking-Pos* effect did eliminate, it was only for at most 2.3 minutes (i.e. one trial block pair), and then it reinstated for another 2.3 minutes before finally eliminating again for the last 2.3 minutes of our (post-practise) IRAP measurements.

Granted, this popular ad hoc approach brought about pervasive and more immediate anti-smoking changes among the three other, secondary aspects of smoking-related implicit evaluating measured by the unconditional-feelings-IRAP. Crucially, however, these secondary effects were likewise also relatively unstable, lasting no more than 2.3 minutes at a time; and moreover, thereafter these short-lived effects tended to at least temporarily rebound in a pro-smoking direction. Overall, therefore, the ad hoc thought suppression instructions examined in Study 3 had a pervasive influence on smokers' implicit evaluating of both pro- and anti-smoking topics, but that influence was rather brief, erratic and often counterproductive across successive pairs of IRAP trial blocks.

As such, true to its moniker, the current smokers' ad hoc approach to thought suppression likely involved smokers deliberately contradicting different aspects of their smoking-related implicit evaluating at different stages of the unconditional-feelings-IRAP. Indeed, the fact that the impact of this approach on all four aspects of smokers' implicit evaluating was moderated by IRAP block order, confirmed that it was prone to cue-based interference from background variables. Therefore, in Study 4 we sought to examine whether a focused approach to thought suppression might be more effective in eliminating implicit evaluating integral to tobacco addiction. To test this idea, Study 4 involved instructing smokers to engage in thought suppression of just that particular aspect of implicit evaluating that we had so far found to be most integral to tobacco addiction. Namely, smokers were instructed to imagine themselves as lifelong non-smokers in terms of familiar scenarios in which they would typically not experience any need to smoke; and this tactic was designed to specifically contradict their pro-smoking implicit evaluating of *Need-Neg*.

Crucially, this more focused approach to thought suppression did appear to improve matters as compared to how immediately and persistently the ad hoc approach examined in Study 3 impacted comparable implicit evaluating. As soon as the smokers were instructed to imagine their chosen scenarios, it immediately eliminated their pro-smoking *Need-Neg* effects to zero for twice as long as the ad hoc thought suppression eventually eliminated smokers' positive *Smoking-Pos* effects in Study 3. Namely, it did so for up to 4.2 minutes before the original pro-smoking *Need-Neg* effects began to reinstate again (i.e. on average, it took the thought suppressors about 2.1 minutes to complete each trial block pair on the relevant IRAP). Then, following a period of reinstatement for up to 2.1 minutes, the smokers' pro-smoking *Need-Neg* effect again eliminated for up to 2.1 minutes, before finally reinstating again for the last 2.1 minutes of our IRAP measurements. In addition, unlike the corresponding effect observed in Study 3, smokers' focused thought suppression of their implicit evaluating of *Need-Neg* was not moderated by IRAP block order – thus indicating that it was relatively robust against cue-based interference from background variables.

Nonetheless, as reviewed earlier, our focused approach to thought suppression of tobacco cravings still had drawbacks, insofar as it immediately and relatively persistently induced ironic effects on secondary aspects of implicit evaluating related to tobacco addiction. By neglecting to contradict or thus reduce smokers' enjoyment-focused pro-smoking implicit evaluating of *Enjoy-Pos*, it appeared as though our focused thought suppression instructions inadvertently primed smokers to implicitly evaluate other more ambiguous topics from that default enjoyment-focused perspective. Namely, at the same time as our focused thought suppression instructions eliminated the smokers' pro-smoking *Need-Neg* effect, it also made their *Need-Pos* and *Enjoy-Neg* effects intermittently more pro-smoking.

Overall, therefore, the current research indicated that focused thought suppression may be a more viable means of temporarily eliminating implicit evaluating integral to tobacco addiction than the commonly used ad hoc approach examined in Study3. However, our findings also indicated that any such benefits come at the cost of cueing (and perhaps sensitizing) other aspects of smokers' pro-smoking implicit evaluating. This then, is perhaps why smokers typically engage in ad hoc thought suppression as in Study 3. Whenever they initially focus upon suppressing one intrusive aspect of smoking-related implicit evaluating smokers may inadvertently provoke another aspect of smoking-related implicit evaluating requiring its own thought suppression strategy – thus provoking an escalating cycle of increasing cognitive load wherein smokers' thought suppression strategies induce an ever wider range of rebound effects, which in turn require smokers to derive ever more thought suppression strategies (e.g. Hooper, Saunders & McHugh, 2010; Hooper et al., 2012; also cf. Germeroth et al., 2013; Hagger, Leaver, Esser, Leung, Te Pas, Keatley, Chan, & Chatzisarantis, 2013; Heatherton & Wagner, 2011; Heckman, Ditre, & Brandon, 2012; Shiffman, West, & Gilbert, 2004; Perkins, 2009; Tiffany & Wray, 2012; Wray et al., 2013).

Of course, further research is needed to fully test this latter model of thought suppression. However, at the very least, our findings indicated that smokers should only ever use focused thought suppression as briefly as possible when attempting to postpone tobacco cravings for whatever reason (e.g. as a means of safeguarding some more effective, but more prolonged coping strategy). Otherwise, they risk inadvertently experiencing the intrusion of secondary tobacco cravings that they would also need to manage in an ever escalating cycle of cognitive load, increasingly divorced from valued activities, until they eventually relent and smoke. Indeed, as per our findings in Study 4, when smokers' attempts at focused suppression of their tobacco cravings begin to become ad hoc in pursuit of secondary tobacco cravings, this is when their implicit evaluating is most likely to rebound in a pro-smoking direction.

Nonetheless, it is of course possible that smokers' thought suppression of tobacco cravings may be more effective when they are under less of a progressive cognitive load than demanded by the current thought suppression protocols. However, this seems unlikely to be important for the following reasons. Smokers only seldom experience tobacco cravings under low cognitive load; rather, tobacco cravings are characteristic of relatively demanding situations that involve being challenged by some aspect of negative affect combined with nicotine deprivation (e.g. whether feelings of stress, boredom, fatigue or sadness; Carter & Tiffany, 1999a; Heckman et al., 2013; Hwang & Yun 2015; McKee et al., 2011; Perkins et al., 2013; Torres & O'Dell, 2015). Confirming this, smokers' explicit self-reports of tobacco cravings are generally very poor prospective predictors of their smoking behaviour, or a smoker's risk of relapse during abstinence, unless those explicit cravings are measured during cognitively taxing situations occurring during acute nicotine deprivation (see Hagger et al., 2013; Heatherton & Wagner, 2011; Heckman, Ditre, & Brandon, 2012; O'Connell, Schwartz, & Shiffman, 2008; Shiffman, West, & Gilbert, 2004; Shmueli & Prochaska, 2012; see also Childs & De Wit, 2010; Conklin & Perkins, 2005; Parrott & Murphy, 2012; Perkins, 2009; Perkins et al., 2008; Germeroth et al., 2013; Tiffany & Wray, 2012; Wray et al., 2013).

Indeed, as alluded to earlier, those who are most addicted to tobacco are also most likely to use thought suppression as a means of eliminating aversive aspects of their ongoing evaluating in lieu of doing so by smoking (see Erskine et al., 2010; Erskine et al., 2012; Farris et al., 2015; Lee, An, Levin, & Twohig, 2015; McCallion & Zvolensky, 2015; Moss et al., 2015; Toll, Sobell, Wagner, & Sobell, 2001). And moreover, abstaining smokers are classically at most risk of relapsing to smoking when they are under cognitive load during stressful situations (see Baker et al., 2004; Brandon et al., 2004; Erskine et al., 2015; Farris et al., 2015; Heatherton & Wagner, 2011; Heckman et al., 2013; McCallion & Zvolensky, 2015; Shiffman & Waters, 2004). Thus, during the very times when smokers most often engage in thought suppression of tobacco cravings, it seems likely to be least effective in relation to problematic implicit evaluating. In fact, emerging research suggests that even in the absence of secondary cognitive challenges, smokers are sufficiently cognitively taxed over short periods (i.e. less than 5 minutes) engaging in thought suppression of their tobacco cravings for it to result in rebound effects in those cravings (e.g. Heckman et al., 2012; Sayers & Sayette, 2013). On balance, therefore, given that the current smokers were relatively satiated for tobacco smoking, if anything our current findings on the limited effectiveness of thought suppression as a method of postponing tobacco cravings are optimistic.

CHAPTER 7: A First Examination of How Smokers' Implicit Evaluating is moderated by Suppression-oriented Nicotine Abstinence during Acute Stress (Study 5)

7.1. INTRODUCTION

Studies 3 and 4 demonstrated that when smokers engaged in thought suppression of implicit evaluating integral to tobacco addiction it tended to have both immediate and delayed ironic effects on various aspects of their pro-smoking implicit evaluating. In particular, these findings together indicated that thought suppression is by itself unlikely to be an effective means for smokers to control or temporarily postpone their tobacco cravings even for as little as two minutes at a time. Indeed, if anything, our findings arguably constituted a relatively optimistic appraisal of the effectiveness of thought suppression for controlling tobacco cravings insofar as both studies were conducted with smokers who were relatively satiated for tobacco smoking. Crucially, smokers are characteristically most prone to experiencing tobacco cravings during the first few days of unaided smoking-cessation, and as a result this is also the period during which smokers are likely to struggle most with suppressing their tobacco cravings. Namely, the most common strategy used by smokers to manage their heightened cravings during the early stages of unaided smoking-cessation is to deliberately suppress them in various ways (e.g. by continually deriving counterexamples, by avoiding tempting situations, by attempting to distract oneself, by using nicotine replacement therapies and/or by eventually relapsing; see Studies 4 & 5; see also Allen, Bade, Hatsukami, & Center, 2008; Brown, Lejuez, Kahler, Strong, & Zvolensky, 2005; Erskine et al., 2010; Erskine et al., 2015; Farris et al., 2015; Lee et al., 2015; O'Connell et al., 2002; O'Connell et al., 2007, p. 79; Moss et al., 2015; Sayers & Sayette, 2013; Toll et al., 2001).

Furthermore, as alluded to in Chapters 5 and 6, the most characteristic reason smokers (and theorists) offer for relapse during the early stages of unaided smoking-cessation is some sort of depletion of one's ability to deliberately sustain (i.e. motivate) thought suppression for long enough until their tobacco cravings subside. Indeed, just as smokers' cravings are peaking during the first few days of unaided smoking-cessation, their risk of relapsing is also typically peaking (i.e. followed by dramatic reductions in both variables over the next four weeks until they reach a relatively low plateau; Allen et al., 2008; Brown, Lejuez, Strong, Kahler, Zvolensky, Carpenter, Niaura, & Price, 2009, p. 498; Hughes, Keely, & Naud, 2004; Kirshenbaum, Olsen, & Bickel, 2009).

Therefore, on the face of it, it appears that the intrusion of cravings into smokers' ongoing evaluating may play a pivotal causal role in tobacco addiction, particularly during unaided smoking-cessation (e.g. Allen et al., 2008).

However, whatever about there being relatively few studies modelling the developmental progression of tobacco cravings in terms of smokers' questionnaire-based, introspective reports during unaided smoking-cessation (Hughes et al., 2004, p. 35; Kirshenbaum et al., 2009; MacKillop, Brown, Stojek, Murphy, Sweet, & Niaura, 2012; Roewer, Wiehler, & Peters, 2015), there are even fewer studies examining how smokers' pro-smoking implicit evaluating changes during this process. This is remarkable in itself when one considers that the concept of tobacco cravings refers, by definition, to implicit evaluative processes. Indeed, as reviewed at the beginning of this thesis, what is perhaps even more remarkable is that, more often than not, what few studies have examined this topic resulted in null or anomalous findings. There were many reasons why this might have been the case. For example, none of the relevant studies examined nicotine deprivation relative to a baseline, but merely between groups – thus making it possible that any relevant effects were obscured relative to unknown baseline differences. Indeed, all such findings were based exclusively upon implicit measures that were unable to distinguish between implicit evaluating addressing different smoking-related topics. Therefore, the relevant implicit measures were unable to specifically target smokers' implicit evaluating of topics that were characteristically relevant to how they experience unaided nicotine abstinence. As such, it was perhaps relatively unlikely that all of the various types of implicit evaluating being measured would all co-vary similarly in tandem with unaided nicotine abstinence. In any case, to us, the most likely reason why researchers have failed to establish any consistent relationship between nicotine deprivation and implicit evaluating is because they did not examine how acute stress moderated any such relationship (but for minimal work related to attention rather than evaluating per se; see Field, Munafó, & Franken, 2009; McCarthy, Gloria, & Curtin, 2009).

Apart from the fact that our first two studies indicated that smokers' relief-focused implicit evaluating of smoking was particularly integral to tobacco addiction, smokers' most commonly stated reason for lapsing during smoking-cessation is to obtain relief from negative affect (i.e. whether due to challenging situations or due to resisting tobacco cravings; Bernstein, Trafton, Ilgen, & Zvolensky, 2008; Collins et al., 2011; Cosci, Aldi, & Nardi, 2015; Erskine et al., 2010; Erskine et al., 2012; Farris et al., 2015; Lee, An, Levin, & Twohig, 2015; McCallion & Zvolensky, 2015; McKay, Franklin, Patapis, & Lynch, 2006; McKee et al 2003; Moss et al., 2015; Shiffman & Waters, 2004; Shiffman, Dunbar, Li, Scholl, Tindle, Anderson, & Ferguson, 2014; Toll, Sobell, Wagner, & Sobell, 2001; Waters et al., 2009, p. 330). In fact, during the past decade or so, a substantial literature has emerged demonstrating that a smokers' ability to tolerate distress in order to satisfy simple short-term task goals is predictive of whether they will remain abstinent from smoking (Brown et al., 2002; Brown et al., 2005; Brown et al., 2009; Kahler et al., 2012; Zvolensky, Farris, Guillot, & Leventhal, 2014); and also of how distressing they found doing so (Abrantes et al., 2008); of how intensely they experienced tobacco cravings (Collins, Nair, & Komaroff, 2011; Heckman et al., 2013); the length of their last smoking-cessation attempt (Daughters, Lejuez, Kahler, Strong, & Brown, 2005); and even their likelihood of actually attending smoking-cessation treatment once enrolled (MacPherson, Stipelman, Duplinsky, Brown, & Lejuez, 2009). Indeed, there is also a wide range of evidence indicating that engaging in prolonged thought suppression of tobacco cravings may even induce those aspects of negative affect that smokers' typically smoke to get rid of (Brandt, Bakhshaie, Garey, Schmidt, Leventhal, & Zvolensky, 2015; Conlkin & Perkins, 2005; Dijkstra & Menninga, 2015; Feldner, Leen-Feldner, Zvolensky, & Lejuez, 2006; McKay et al., 2006; Spring, Cook, Appelhans, Maloney, Richmond, Vaughn, Vanderveen, & Hedeker, 2008; Zvolensky et al., 2014). However, much like the prevailing literature on tobacco cravings, the literature on distress tolerance has generally not examined what (implicit) evaluative processes may be integral to distress tolerance (Hooper et al., 2010, 2012; Kapson, Leddy, & Haaga, 2012; Kishita et al., McKay et al., 2006; Schloss & Haaga, 2011; see also Bernstein et al., 2008; Cosci et al., 2015; Cosci, F., Bertoli, G., & Abrams, 2013; McHugh, Daughters, Lejuez, Murray, Hearon, Gorka, & Otto, 2011; Sütterlin, Schroijen, Constantinou, Smets, Van den Bergh, & Van Diest, 2013; see also Zvolensky et al., 2014).

In fact, to date, only one study has explored whether smokers' implicit evaluating may play a role in distress tolerance processes (and thus relapse processes) during the early stages of smoking-cessation. Encouragingly, this study observed a large prospective correlation between smokers' baseline implicit evaluating of experiential avoidance on an approach/avoid IAT, and the length of time in hours that they managed to abstain unaided from smoking (Cameron et al., 2013). Indeed, confirming that the smokers' implicit evaluating of experiential avoidance on the IAT was likely a major component of their distress tolerance during smoking-cessation, Cameron et al. also found a large correlation between this implicit evaluating and one of two traditional measures of distress tolerance. However, Cameron et al.'s study did not measure implicit evaluating of distress tolerance in relation to smoking specifically; nor smokers' implicit evaluating of smoking or quitting; much less an experimental examination of the impact of acute stress on smokers ongoing implicit evaluating of smoking or quitting.

Therefore, in the current study, we were particularly interested in using an IRAP to experimentally examine how unaided smoking-abstinence moderated the baseline-controlled impact of acute stress on daily smokers' implicit evaluating. In particular, we designed a *relapse-IRAP* to measure smokers' implicit evaluating of not just smoking, but also the prospect of trying to quit smoking. We did this mainly because many theories of health psychology, and indeed of tobacco addiction, assert that one's likelihood of remaining abstinent from unhealthy behaviour is motivationally very dependent upon one's ongoing evaluating of doing so (Abrams et al., 2003; Armitage & Connor, 2001; Brandon et al., 2005; Curry et al., 1997; Downey et al., 2001; Norcross et al., 2011; Prochaska & Norcross, 2001; Smith, Carter, Chapman, Dunlop, & Freeman, 2015; Topa & Moriano, 2010; Uppal, Shahab, Britton, & Ratschen, 2013; Wiers et al., 2010). And yet, crucially, to our knowledge, this was the first study to examine (smokers') implicit evaluating of abstinence, much less how any such implicit evaluating changed across the early stages of engaging in unaided (suppression-oriented) abstinence (e.g. see Chassin et al., 2010; Köpetz et al., 2013; Roefs et al., 2011; Sheeran et al., 2013; Smith et al., 2013; Smith & De Houwer, 2014; Uppal et al., 2013, p. 432; Wiers & Hoffman, 2010; Webb, Sheeran & Pepper, 2012).

Given the relatively fleeting malleability effects we observed upon smokers' implicit evaluating in studies 3 and 4, and related concerns about the need to identify component processes, we were eager to measure smokers' implicit evaluating as soon after stress induction as possible (McKay et al., 2006; see also Kapson et al., 2012; Kazdin, 2007; Schloss & Haaga, 2011). Therefore, in order to achieve this, we incorporated the criterion practise block effects into our scoring of the relapse-IRAP delivered immediately after stress induction. Moreover, given that this would have been the current smokers' third cycle of the relapse-IRAP within 24 hours, it seemed likely that all participants would achieve the relevant IRAP response criterion during their first pair of IRAP practise blocks.

By contrast, however, when it came to analysing the current smokers' implicit evaluating at baseline and under unaided nicotine abstinence prior to stress induction, we did so only in terms of standard *trial-type* D_{IRAP}s – thus, using the IRAP criterion blocks as a kind of contextual buffer. We did this because when uncontrolled, any fleeting contextual effects encroaching during the practise phase of an IRAP would, in principle, obscure one’s ability to detect the much more gradual, and thus stable, influence of nicotine abstinence on the current smokers’ implicit evaluating. Crucially, the body metabolises nicotine exponentially ever more gradually over the first few days of abstinence, and even in the earliest stages of nicotine abstinence withdrawal symptoms do not normally change across the 10-20 minutes it usually takes participants to complete an IRAP (e.g. Allen et al., 2008; Hukkanen et al., 2005; MacKillop et al., 2012; SRNT Subcommittee on Biochemical Verification, 2002).

In addition, given that daily smokers are most likely to relapse during approximately the first 12-48 hours of unaided smoking-cessation we chose to examine the impact of acute stress and unaided nicotine abstinence on smokers’ implicit evaluating during the first half of this abstinence timeframe, between 14-24 hours of abstinence (Allen et al., 2008; Brown et al., 2009, p. 498; Hughes et al., 2004; Kirshenbaum et al., 2009; Ussher, Beard, Abikoye, Hajek, & West, 2013). Our primary aim in choosing this specific time-period was to maximize the potential for observing problematic implicit evaluating (i.e. tobacco cravings), while also minimizing the potential for a disproportionate participant drop-out rate in our abstinence group due to smoking relapse (i.e. as compared to our control group who were instructed to smoke as usual throughout). Thus, having measured all of the current smokers’ implicit evaluating at baseline during the afternoon, we did so again approximately 24 hours later both before and after exposing them to a succession of validated stress induction tasks in the presence of cues for smoking (i.e. their preferred brand of tobacco).

Crucially, given that smokers metabolize nicotine at a much slower rate during sleep than while awake we deemed it important to schedule participants’ baseline and follow-up sessions during the mid-to-late afternoon (e.g. Hukkanen, Jacob, & Benowitz, 2005; SRNT Subcommittee on Biochemical Verification, 2002). Moreover, from the outset, we informed all smokers that if they were randomly assigned to abstain from smoking that they should do so for *at least* 14 hours before they attended their chosen follow-up session (i.e. 24 hours after their chosen baseline session). Thus, by allowing the abstainer group some discretion as to when they initiated their abstinence from smoking, we hoped to minimize the potential for any unusual patterns of smoking at baseline resulting from the common strategy of smoking more in preparation for imminent abstinence (see Allen et al., 2008; Bancroft et al., 2003; O'Connell et al., 2002). Likewise, this also allowed us to postpone informing the current smokers as to which experimental group they had been randomly assigned until after they had completed the baseline measurement session – thus, further minimizing the potential for smokers' expectations about the following 24 hours to confound baseline measurements between groups.

Also, given that we were keen to model smokers' characteristic tendency to deliberately suppress their tobacco cravings as a means of remaining abstinent, we specifically sampled daily smokers who were intolerant of, and thus likely to deliberately suppress their tobacco cravings (e.g. whether by smoking, by deliberately disputing those cravings, or by distracting oneself, etc.). Namely, daily smokers who would eventually like to quit smoking but who were also currently not willing to abstain unaided from smoking for even the relevant 14-24 hour period, unless they received a relatively substantial monetary payment for doing so (for more details see Method). In addition, to verify that the current smokers were all relatively inclined to deliberately suppress tobacco cravings throughout the current study, we measured these tendencies at each stage of baseline and follow-up using the AIS and also two complementary measures of distress tolerance. Then, at the end of the follow-up session we asked all abstaining smokers whether they would smoke immediately afterwards – with an affirmative answer confirming their status as being reluctant to experience tobacco cravings even though they ultimately valued quitting.

Lastly, we opted to measure reward-focused, rather the relief-focused implicit evaluating of smoking for two main reasons. Firstly, we demonstrated in studies 3 and 4 that smokers were prone to ironic increases in their reward-focused implicit evaluating in favour of smoking while attempting to deliberately suppress their relief-focused implicit evaluating in favour of smoking. And therefore, insofar as the current acute stress induction tasks required the current smokers to manage an increased occurrence of relief-focused tobacco cravings, we expected similar rebound effects in their reward-focused pro-smoking implicit evaluating to occur in response to these tasks – particularly during nicotine abstinence, but perhaps also while smoking as usual. Moreover, by contrast, Study 4 revealed that smokers are capable of reducing their relief-focused implicit evaluating for most of the duration of an IRAP. And therefore, secondly, it seemed likely that reward-focused pro-smoking implicit evaluating of smokers would ironically be much more prone to rebound effects in response to acute stress than their relief-focused implicit evaluating in favour of smoking. Indeed, pilot testing with six participants indicated that relief-focused implicit evaluating appeared to be relatively unchanged by the current acute stress induction tasks during unaided nicotine abstinence.

7.2. METHOD

Participants

We recruited 30 smokers (18 female) from among the student population of the University of Wales, Swansea, on the basis that they had been smoking regularly for at least 1 year; typically at a rate of least 10 cigarettes per day; and that they wished to quit smoking eventually, but did not plan to quit smoking within the following two weeks. These smokers were each randomly assigned without replacement between an experimental group tasked with abstaining from smoking (and all nicotine replacement products) for at least 14 hours (i.e. the abstainer group), and another experimental group tasked with smoking as usual during the same time period (i.e. the control group). All participants were recruited with the understanding that if they were randomly assigned to the abstainer group they would receive £25 subject to confirmation via a "biological breath test" (see below) that they had been abstinent from smoking during their chosen 14-24 hour period of abstinence. In addition, during recruitment, we also informed participants that regardless of whether they abstained or not, they would receive £5 for completing a 40 minute baseline measurement protocol, followed by a 60 minute outcome measurement protocol approximately 24 hours later (i.e. to encourage participants to return at follow-up, and to accurately disclose whether they had consumed nicotine-based products or not). Furthermore, we offered all potential participants the chance to win an additional £10 if they scored in the top 33% percentile on a "challenging test of attention" during the second experimental session which, unbeknownst to the current smokers, was designed to induce psychological stress. Crucially, in line with previous research in the area, this latter incentive was designed to enhance the extent to which this test described below, the PASAT-C, experimentally induced psychological stress during the second experimental session (e.g. Brown et al., 2002, p. 181; Daughters et al., 2005, p. 209; Kapson et al., 2012, pp. 1234-1235; Schloss & Haaga, 2011; Zvolensky et al., 2014, p. 226).

We bio-verified all participants' ongoing smoking-status both at baseline and follow-up using the *Bedfont Micro⁺ Smokerlyzer* carbon monoxide monitor whereby smokers are required to hold a deep breath for 15 seconds before fully exhaling into this device. Crucially, the half-life of expired-CO was optimal to detect whether smokers' had been abstinent from smoking for periods of between 14-24 hours; indeed, by contrast all other biological metabolites of smoking exhibit relatively prolonged half-lives that prevent them from verifying such a short period of abstinence (see SRNT Subcommittee on Biochemical Verification, 2002; Hukkanen et al., 2005). In line with the Micro$^+$ Smokerlyzer operating manual and indeed subsequent research recommendations from the literature, we required all smokers to have an expired CO reading of at least 8 parts per million (*ppm*) at baseline, the control group to have a minimum reading of 8*ppm* at a similar time the following day from baseline; and crucially, for the abstainers to have a maximum reading of 6*ppm* at a similar time the following day from baseline (Brown et al., 2002; Godtfredsen, Prescott, Vestbo, & Osler, 2006, pp. 1519-1521; Leitch, Harkawat, Askew, Masel, & Hendrick, 2005; Perkins, Karelitz, & Jao, 2013; SRNT Subcommittee on Biochemical Verification, 2002, pp. 151-152). No participants were excluded from the current study on this basis.

However, all participants were exposed to a brief pipeline procedure at baseline misinforming them that the Micro$^+$ Smokerlyzer could detect whether they had used any nicotine-based products during the preceding 14-24 hours (i.e. contrary to what we told participants CO monitors are not capable of detecting nicotine consumption except by means of smoking). On this basis, one abstainer voluntarily admitted to using nicotine replacement products during their chosen period of abstinence, and so his data was excluded from further analyses. In addition, one other smoker were excluded for withdrawing their participation during the follow-up session in order to smoke without further delay.

On average, the remaining 28 smokers (18 females) provided a baseline expired CO sample of 12.5*ppm* (*SD* = 4.08, *range* = 8-24), the control group provided a follow-up expired CO sample of 12.1*ppm* (*SD* = 3.5, *range* = 8-18), and the abstainer group provided a follow-up expired CO sample of 3.07*ppm* (*SD* = .80, *range* = 2-4). In addition, at baseline, the smokers were moderately willing to quit smoking on the 11-point *Readiness to Quit Ladder* (*M* = 4.8; *SD* = 1.03; *range* = 3-7; see Abrams et al., 2003, pp. 31-33; see also Wong & Cappella, 2009); were aged 24.8 years (*SD* = 8.6; *range* = 19-50); had been smoking for 9.3 years (*SD* = 8.2; *range* = 1-36); smoked 16.4 cigarettes per day (CPD; *SD* = 4.8; *range* = 10-28); registered on the HONC as being moderately psychologically dependent on smoking (*M* = 6.7; *SD* = 3.1; *range* = 0-10; cf. Wellman, Di Franza, Pbert, et al., 2005; Wellman, Savageau, et al. 2006); and were moderately inclined to smoke as a means of experiential avoidance (*M* = 45.4; *SD* = 10.3; *range* = 23-64; see Farris et al., 2015).

Apparatus

Mood Induction and Distress Tolerance Tasks

All participants were required to complete two mood induction tasks in between the two IRAP sessions implemented as part of the follow-up protocol (see Procedure). The first mood induction task, adapted from Samson and Rachman (1989), Barnes-Holmes, Barnes-Holmes, Smeets, & Luciano (2004), and Conklin and Perkins (2005) was primarily designed to induce sad mood states. Like previous versions of this task, the current version required participants to listen to a piece of sad music, Albinoni's "Adagio in G Minor", for seven minutes while simultaneously watching a series of pictorial slides from the International Affective Picture System (IAPS; Lang, 1988) chosen to be congruent with the negative mood engendered by Alblnoni's Adagio in G Minor.

In addition, in order to amplify the extent to which this music and video induced sad moods in smokers we also required participants to complete a thought-listing procedure at the same time (e.g. see Barnes-Holmes et al., 2004; Cahill et al., 2007). Apart from the fact that there is longstanding literature attesting to the effectiveness of narrative-based techniques as a means of inducing negative moods (e.g. Dasgupta, DeSteno, Williams, & Hunsinger, 2009; Hernandez, Vander Wal, & Spring, 2003; Kučera & Haviger, 2012; Robinson, Grillon, & Sahakian, 2012; Spring et al., 2008), our main reason for incorporating this feature was to better ensure that the current smokers would engage with Albinoni's "Adagio in G Minor" in the sad manner we intended. In particular, the thought-listing component of our musical mood-induction task involved instructing the current smokers “to reflect during the music upon events and things in your life that have made you feel sad, frustrated, anxious, worried or even angry to the point that you would need to smoke a cigarette”; and to continually write those reflections down on paper one at a time as soon as they occurred (see Appendix 23 for further details). In addition, as a supplementary manipulation check, we also required participants to immediately rate how positively or negatively they felt about each of these reflections using a 7-point semantic differential ranging from -3 labelled *Negative Feeling*, to 0 labelled *Neutral*, and on to +3 labelled *Positive Feeling*.

The second mood induction procedure, called the PASAT-C (Lejuez, Kahler, & Brown, 2003), a modified version of the Paced Auditory Serial Addition Task (PASAT; Gronwall, 1977), was specifically developed to experimentally simulate acute psychological stress in laboratory conditions. Like the musical mood induction procedure described above, the PASAT-C also incorporated its own questionnaire-based manipulation checks designed to measure participants' levels of psychological stress both immediately before and two-thirds of the way through this task. Namely, participants were required to rate in turn how much they currently felt anxious, irritable, distracted, happy, uncomfortable, and frustrated using six respective visual analogue scales ranging from 0 labelled "None" to 100 labelled "Extreme" (for more details see Appendix 24; Brown et al., 2002; MacPherson et al., 2009).

Crucially, in addition to providing an effective means of simulating acute psychological stress (e.g. Lejuez, Kahler, Brown, 2001; Feldner et al., 2006), smokers' responses to the PASAT-C also appear to be predictive of various clinically important features of their addiction to tobacco. For example, the length of time that smokers' persist with the PASAT-C is predictive of their ability to remain abstinent from smoking for at least 24 hours (Brown et al., 2002; Brown et al., 2005), and also of how distressing they found doing so (Abrantes et al., 2008); the length of their last smoking-cessation attempt (Daughters et al., 2005); and even their likelihood of actually attending smoking-cessation treatment once enrolled (MacPherson et al., 2009).

However, in one study smokers' persistence with the PASAT-C did not prospectively predict a smoker's risk of relapsing within 28-days of quitting smoking unaided, when a shorter less complicated measure of distress tolerance called *breath-holding duration* did (Brown et al., 2009). Indeed, this variable, the length of time that smokers' are willing to hold their breath for a researcher timing them, has long been known to predict their ability to sustain smoking-cessation (Brown et al., 2002; Hajek, 1991; Hajek, Belcher, & Stapleton, 1987; and more recently see also Bernstein et al., 2008; Cosci et al., 2015; Kahler et al., 2012). Therefore, in order to supplement the distress tolerance measure provided by smokers' persistence on the PASAT-C, we also measured breath-hold duration within the current study. It is important to bear in mind however that the breath-hold task was not intended as means of mood-induction. Crucially, asking smokers' to hold their breath for as long as they are able does not typically induce measurable physiological or self-report changes in smokers' mood. Rather smokers' breath-holding duration appears to be related to how much they anticipate physical discomfort from this task and/or their willingness to experience any such physical discomfort (e.g. Cosci et al., 2015; Cosci et al., 2013; McHugh et al., 2011; Sütterlin et al., 2013; see also Zvolensky et al., 2014).

Questionnaire-based State and Trait Measures

All participants completed two trait measures of tobacco addiction at baseline; namely, the Hooked on Nicotine Checklist (HONC; DiFranza et al., 2002) and the

Avoidance and Inflexibility Scale (AIS; see Farris et al., 2015). In addition, we also used an updated version of the DBHQ developed across the preceding four studies, to conduct exhaustive semi-structured interviews with participants regarding their current and historical smoking-status as well as their basic demographics (see Appendix 25). In addition, as manipulation checks designed to separate the mood and craving effects of nicotine abstinence versus stress induction, participants were also required to complete the following measures immediately after each of three IRAP sessions (i.e. one at baseline and two at follow-up; see below). The first of these manipulation checks was a standardised measure of reward- versus relief-focused tobacco cravings, the brief (10-item) version of the Questionnaire of Smoking Urges (QSU-brief; Cox, Tiffany, & Christen, 2001). The second manipulation check was a 20-item standardised measure of positive and negative affect, the state version of the Positive and Negative Affect Schedule (PANAS; Watson, Clark, and Tellegen, 1988; see also Crawford & Henry, 2004). In particular, we administered the state version of the PANAS with the instruction: "Please indicate to what extent you feel this way right now." And the third of these manipulation checks was a 6-item *incentive rating scale* designed to measure changes in smokers' motivation to engage in commonly valued activities (see Barnes-Holmes et al., 2004, p. 98; Clark & Teasdale, 1985).

An IRAP Measuring Implicit Evaluating of Smoking and Quitting Smoking

We employed the same version of the IRAP program as in previous chapters, but using different label and target stimuli. As before, participants were presented with the response options "True" and "False" on every IRAP trial but this time the concept label stimuli were "SMOKING makes Me feel" and "Trying NOT to SMOKE makes Me feel", and critically, the positive and negative craving-related mood words previously used as target stimuli were modified for compatibility with these label stimuli (i.e. especially from a typical smokers' perspective). Namely, the positive target words were: *Pleasant*, *Better*, *Outgoing*, *Calm*, *Comfortable*, and *Relaxed*. And the negative target words were: *Stressed*, *Worse*, *Withdrawn*, *Unpleasant*, *Tense*, and *Irritable*. In particular, we chose these particular positive target words so that they were apt to both smokers' vernacular aspirations for quitting smoking, and also to their vernacular reasons for smoking. And likewise, we chose these negative target words that they were apt to both smokers' vernacular reasons for failing in smoking-cessation, and also to their vernacular reasons for disliking smoking (and thus wanting to quit; see Curry, Grothaus, & McBride, 1997; Downey, Rosengren, & Donovan, 2001; McCaul, Hockemeyer, Johnson, Zetocha, Quinlan, & Glasgow, 2006).

The researcher instructed participants before each pro-smoking/anti-quitting IRAP practise block to "Respond as if smoking makes you feel *good*, and as if trying not to smoking makes you feel *bad*"; and before each anti-smoking/pro-quitting practise block to "Respond as if smoking makes you feel *good*, and as if trying not to smoking makes you feel *bad*." Otherwise, the current IRAP was the same as those used in the preceding studies contained in this thesis. And like before, in order to counterbalance IRAP trial block order, half of the participants received odd numbered IRAP blocks that required a pro-smoking pattern of responding and even numbered blocks that required anti-smoking responding; for the remaining participants the opposite applied (odd numbered blocks required anti-smoking responding and even numbered blocks pro-smoking responding).

Measures of Explicit Evaluating

We employed four feeling thermometers each requiring participants to mark a visual analogue scale ranging from zero labelled "Extremely Cold (Disagree)" to ninety-nine labelled "Extremely Warm (Agree)", in response to a statement summarizing one of the IRAP trial-types. The four statements in question were "SMOKING makes Me feel GOOD", "SMOKING makes Me feel BAD", "Trying NOT to SMOKE makes Me feel GOOD", and "Trying NOT to SMOKE makes Me feel BAD".

Procedure

Participants were required to complete two experimental sessions, scheduled approximately 24 hours apart. During the initial session lasting approximately 30-40 minutes, all participants were first required to provide a baseline sample of expired CO; to indicate when they had last smoked; and to then complete the current relapse-IRAP with a maximum average response latency of 3000*ms* and a minimum average response accuracy of 80%. Thereafter, participants completed the corresponding measures of explicit evaluating, before then completing four baseline manipulation checks: the QSU-brief; the HONC; the AIS; the PANAS; and a six-item 'incentive rating scale' designed to measure smokers' motivation to engage in commonly valued activities.

Then, the researcher interviewed participants using an updated version of the DBHQ in an open-ended fashion to reveal any unanticipated details with relevance to the current known-groups sampling agenda. And this was followed by the researcher asking the smokers "to stand up, and when ready take a deep breath and hold it for as long as possible while I record the length of your attempt with this stopwatch." Finally, at the end of the baseline session, all smokers were informed as to whether they had been randomly assigned to abstain from cigarettes versus smoke as usual during the 14-24 hours before they attended the relevant follow-up session the next day. Thus, apart from during the IRAP practise trials, the interview, and the breath-holding duration task, the current smokers completed their baseline tasks alone in a sound-proofed cubicle.

Crucially, as during recruitment, all abstainers were reminded at the end of the baseline session that they were free to smoke until the late evening but that the closer they got to midnight while still smoking the higher their chances of failing the CO breath-test during their agreed follow-up session the following day. Accordingly, when the current smokers returned to complete the follow-up session, the first thing that the researcher asked all of them (i.e. not just the abstainers) was how long it had been since they had last smoked (or used any other nicotine-based product). Then, the researcher measured the concentration of CO in each participant's follow-up breath sample. Next, all participants completed the relapse-IRAP again, followed by the same sequence of questionnaire-based measures used at baseline except for the HONC. And this was respectively followed by the musical mood induction procedure, the PASAT-C, a third implementation of the relapse-IRAP, the corresponding explicit measures, and the same three manipulation checks again that were measured before musical mood induction (i.e. QSU-brief, the PANAS, and the six-item incentive rating scale).

Additionally, just before completing this third implementation of the relapse-IRAP all smokers were asked to show the researcher what tobacco product that they had with them. Crucially, all participants were instructed at the end of the baseline session to bring their preferred tobacco product with them to the follow-up session, so that ostensibly, the researcher could record details of how much nicotine and/or tar were in those products to see whether they corresponded to the relevant smokers' breath CO readings. The main purpose of doing this was to serve as a pretext for placing smokers' preferred tobacco product within their field of vision, but out of reach, as a cue for smoking while they completed their third implementation of the relapse-IRAP (e.g. Carter & Tiffany, 1999a; Collins et al., 2011; Heckman et al., 2013). Moreover, by explaining that the relevant nicotine and tar information was needed to individually calibrate each smoker's CO sample cut-off, we also hoped to encourage abstaining smokers to disclose if they had used any nicotine replacement products during the preceding 14-24 hours. Then, finally, before debriefing, all participants were required to complete the breath-holding duration task once more. Much like during baseline, the current smokers completed most of their follow-up tasks in a sound-proofed cubicle. Crucially, however, the researcher sat in silence with each participant as they completed the PASAT-C in order to heighten their sense of being evaluated, and to thus make it more likely that they would find the task stressful as it became progressively more unrealistic to adhere to (see Bosnes, Dahl, & Almkvist, 2015; Eastvold, Belanger, & Vanderploeg, 2012; Hopko, Hunt, & Armento, 2005; Horwitz & McCaffrey, 2008; Sonderegger, A., & Sauer, 2009; Yantz & McCaffrey, 2005). In all the follow-up procedure thus took approximately 1 hour to complete.

Ethical Considerations

The present research was approved by the Research Ethics Committee at Swansea University. Each participant provided informed consent on their own behalf based explicitly on the idea that their participation was on an anonymous basis; and on the idea that they could voluntarily withdraw their participation from the study at any point in time and without explanation.

7.3. RESULTS

7.3.1. Scoring the IRAP Data

All 28 smokers included in the current analyses maintained a minimum response accuracy of 80% and/or a maximum response speed of 3000*ms* on average across the test trials, on all three relapse-IRAPs. Given that the effects of nicotine abstinence are typically much more gradual than the 10-20 minutes it typically takes participants to complete an IRAP, we therefore scored the resulting data from both of the first two relapse-IRAPs using the standard D_{IRAP}-algorithm. By contrast, however, unlike the gradual rate at which nicotine leaves the body, the current stress induction tasks were designed to bring about relatively immediate and acute changes in tobacco cravings much as we observed in Studies 3 and 4. And so, in order to capture the most immediate effects of these acute stress induction tasks, we therefore scored the data from the third relapse-IRAP using the *extended* D_{IRAP}-algorithm we introduced earlier (i.e. which was designed to incorporate data from IRAP criterion practise blocks).

7.3.2. Manipulation Checks for Suppression-oriented Abstinence and Acute Stress

At baseline, the control group and the abstainer group were very similarly, moderately inclined to engage in suppression-oriented abstinence from nicotine as per their AIS scores, $t(26) = -.19$, $p = .85$, $\eta^2 = .001$, $r = .04$ (i.e. respectively, *M*s = 45.8, 45.0; *SD*s = 9.3, 11.4; *ranges* = 31-64, 23-56). However, as a result of the 16 hours on average that the abstainer group had been nicotine abstinent at follow-up their AIS scores increased to a large degree relative to the control group, $F(1, 26) = 6.13$, $p = .02$, $\eta_p^2 = .19$, $r \approx .44$; and this finding remained stable across stress induction, $F(1, 26) =$

.07, p = .79, η_p^2 = .003, $r \approx$ .05. Specifically, whereas the abstainer group exhibited an average AIS score of 45.0 at baseline (*SD* = 11.4; *range* = 23-56), they exhibited an average AIS score of 48.40 initially at follow-up (*SD* = 9.9; *range* = 33-60), and one of 49.33 shortly after stress induction (*SD* = 13.1; range = 23-65). And by contrast, the control group exhibited an average AIS score of 45.8 at baseline (*SD* = 9.3; range = 31-64), a similar average AIS score of 43.69 at follow-up (*SD* = 9.3; range = 31-63), and indeed one of 45.15 shortly after stress induction (*SD* = 7.8; range = 35-61).

Likewise, as further evidence that nicotine abstinence reduced the smokers' willingness to experience tobacco cravings, the control group persisted for over two thirds as long on average with the PASAT-C as the abstainer group, $F(1, 26)$ = 1.99, p = .17, η_p^2 = .07, $r \approx$ .27 (i.e. respectively, *M*s = 405, 263; *SDs* = 247, 281; ranges = 4-600, 3-600). Moreover, whereas seven of the control group completed the full 600 seconds required by the PASAT-C, only four of the abstainer group did, and this also constituted a moderately-sized statistical effect, $\chi^2(1, 28)$ = 1.20, p = .27, *Cramer's V* = .21 (i.e. $r \approx$ .21). Furthermore, at the end of the follow-up session we asked all abstaining smokers whether they would smoke immediately afterwards, and all reported that they would definitely do so (i.e. indicating a reluctance to experience tobacco cravings even at the cost of sacrificing their ultimate desire to quit smoking). However, the log-transformed[81] time that the current smokers were willing to hold their breath for was not affected by whether they had been stressed during nicotine abstinence versus while smoking as usual, $F(1, 26)$ = .10, p = .75, η_p^2 = .004, $r \approx$ .07.

In addition, all of the current smokers reported large reductions across the first two stages of the PASAT-C in the extent to which they experienced happiness, $F(1, 26)$ = 11.45, p = .002, η_p^2 = .32, $r \approx$ .57; and large increases in the degree to which they experienced difficulty concentrating, $F(1, 26)$ = 12.44, p = .002, η_p^2 = .32, $r \approx$ .57, anxiety, $F(1, 26)$ = 5.93, p = .02, η_p^2 = .19, $r \approx$ .43, irritability, $F(1, 26)$ = 16.68, p = .0004, η_p^2 = .39, $r \approx$ .63, and frustration, $F(1, 26)$ = 18.94, p = .0002, η_p^2 = .42, $r \approx$ .65.[82] Crucially, nicotine deprivation did not interact with any of the foregoing changes in explicit mood ratings across the PASAT-C, F(1, 26)s $\leq$ .70, *p*s $\geq$ .41, η_p^2s $\leq$ .026, *r*s $\leq$

[81] Given that response latencies (i.e. as opposed to response latency differences) have a strong tendency towards being positively skewed it is standard practice in the distress tolerance literature to log-transform breath holding latency as a measure of distress tolerance (e.g. see Bernstein et al., 2008; Brown et al., 2009; Cosci et al., 2015; Kahler et al., 2012).

[82] By contrast, as intended by the developers of the PASAT-C, we did not observe any changes in the current smokers' ratings of bodily discomfort across this task, $F(1, 26)$ = .85, p = .37, η_p^2 = .03, $r \approx$ .18; and nor did it interact with nicotine abstinence in this regard, $F(1, 26)$ = .02, p = .88, η_p^2 = .001, $r \approx$ .03 (see Lejuez et al., 2003, p. 291).

.16, except moderately so with respect to those referring to feelings of frustration, $F(1, 26) = 2.96$, $p = .10$, $\eta_p^2 = .10$, $r \approx .32$. Namely, the PASAT-C had less impact on the levels of frustration reported by the abstaining smokers as compared to the control group, $F(1, 26) = 18.94$, $p = .0002$, $\eta_p^2 = .42$, $r \approx .65$. And yet, nonetheless, the PASAT-C still brought about statistically large increases in the levels of frustration that the abstainers, $t(12) = -1.77$, $p = .10$, $\eta^2 = .18$, $r = .43$, and the control group explicitly reported, $t(12) = -4.67$, $p = .0005$, $\eta^2 = .65$, $r = .80$. Specifically, the PASAT-C increased the control group's reported levels of frustration from an initially low level beforehand (i.e. *M* = 17.5, *SD* = 18.7; *range* = 3-60), to a similarly moderate level (i.e. *M* = 54.3, *SD* = 30.8; *range* = 3-92). And at the same time, the PASAT-C increased the abstainer group's already high levels of frustration due to unaided abstinence (i.e. *M* = 54 versus 69.9, *SD* = 27.2 versus 30.1; *range* = 1-81 versus 4-100). In other words, it appeared as though abstaining from nicotine at least partially induced a ceiling-effect in the current smokers' reported feelings of frustration, such that the PASAT-C failed to increase them any further. Indeed, confirming this, the abstainer group reported a large degree more feelings of frustration at first follow-up than the control group, $t(26) = 4.07$, $p = .0001$, $\eta^2 = .39$, $r \approx .62$.

As further confirmation of this pattern of moods, we also found a large increase the smokers' ratings of negative moods on the PANAS due to nicotine abstinence, $F(1, 26) = 20.37$, $p = .0001$, $\eta_p^2 = .44$, $r \approx .66$, which resulted in the abstainers reporting a large degree more negative moods relative to the control group at follow-up, $t(26) = 2.18$, $p = .04$, $\eta^2 = .15$, $r \approx .39$; a difference which in turn did not appear to be affected by the stress induction, $F(1, 26) = .01$, $p = .91$, $\eta_p^2 = .0001$, $r \approx .01$. Rather, the stress induction tasks appeared to induce a similarly large increase in explicit reports of negative mood on the PANAS for both experimental groups, $F(1, 26) = 4.68$, $p = .04$, $\eta_p^2 = .16$, $r \approx .40$.

In addition, concordantly, we also observed moderate reductions in the current smokers' ratings of positive moods on the PANAS due to nicotine abstinence, $F(1, 26) = 2.07$, $p = .16$, $\eta_p^2 = .07$, $r \approx .27$, which the stress induction task further reduced by a similarly large statistical amount for both experimental groups, $F(1, 26) = 18.83$, $p = .0002$, $\eta_p^2 = .42$, $r \approx .65$ (i.e. the relevant interaction effect was null, $F(1, 26) = .59$, $p = .45$, $\eta_p^2 = .02$, $r \approx .15$). Likewise, nicotine abstinence induced large reductions in the current smokers' motivations to engage in commonly valued activities as per their responses to the six incentive rating questionnaire scales, $F(1, 26) = 5.85$, $p = .02$, $\eta_p^2 =$

.18, $r \approx .43$; and crucially, this difference remained unchanged across the stress induction tasks, $F(1, 26) = .91$, $p = .35$, $\eta_p^2 = .03$, $r \approx .18$. Lastly, confirming our experimental manipulation of nicotine abstinence, the abstainer group also exhibited a very large increase in both reward-focused tobacco cravings, $F(1, 26) = 32.75$, $p < .0001$, $\eta_p^2 = .56$, $r \approx .75$, and relief-focused tobacco cravings, $F(1, 26) = 14.51$, $p = .0008$, $\eta_p^2 = .36$, $r \approx .60$, from baseline to just before the stress induction tasks (i.e. relative to the control group). By contrast, however, much like the pattern of frustration mood ratings we observed above, the stress induction tasks induced a large increase in the control group's reward-focused QSU tobacco cravings without affecting the abstainers' corresponding ratings $F(1, 26) = 11.23$, $p = .003$, $\eta_p^2 = .30$, $r \approx .55$; and also without affecting either group's relief-focused QSU tobacco cravings any further, $F(1, 26) = .78$, $p = .39$, $\eta_p^2 = .03$, $r \approx .17$.

7.3.3. Baseline Analyses of IRAP Trial-type by Trial Block Order

The IRAP data were initially entered into a 2x2x2 mixed-repeated measures ANOVA, which crossed the between-groups variable IRAP trial block order (i.e. pro- versus anti-smoking-first) with the two IRAP trial-type variables. Trial block order did not interact with either of the trial-type variables, $F(1, 26)$s $\leq .31$, ps $\geq .58$, η_p^2s $\leq .01$, rs $\leq .11$. However, trial block order exerted a moderately sized main effect across the baseline IRAP trial-type data such that the trial-type effects obtained from the anti-smoking-first condition tended to be more positive than those obtained from the pro-smoking-first condition, $F(1, 26) = 2.13$, $p = .16$, $\eta_p^2 = .08$, $r \approx .27$. Moreover, as illustrated in Figure 7.1, there was also a large interaction between the concept and target trial-type variables, $F(1, 26) = 19.60$, $p = .0002$, $\eta_p^2 = .43$, $r \approx .66$, which qualified main effects for both variables, respectively; $F(1, 26)$s = 8.84, 3.23; ps = .006, .08; η_p^2s = .25, .11; rs $\approx$ .50, .33.

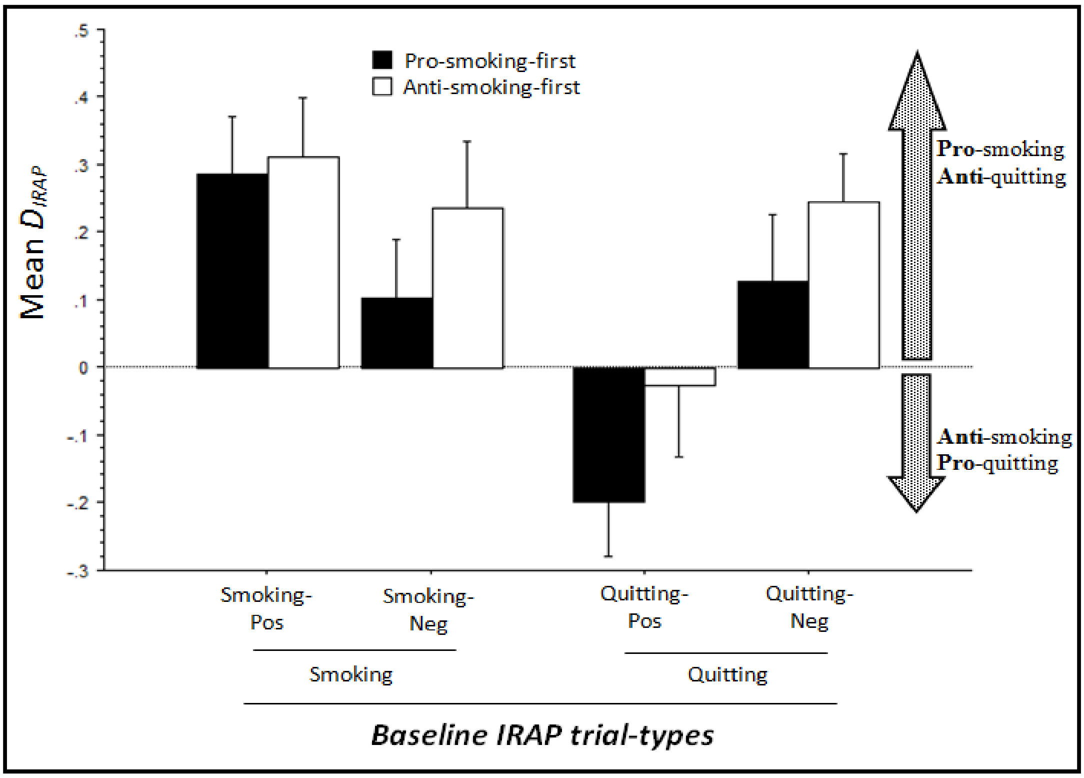

Figure 7.1. Mean *trial-type* D_{IRAP} scores, with standard error bars, split by IRAP trial block order and both IRAP trial-type variables.

Furthermore, planned follow-up analyses revealed that the current smokers exhibited large pro-smoking effects on both trial-types dealing with smoking (i.e. respectively, $t(27)$s = 5.25, 2.72; ps < .01; η^2s = .51, .22; rs ≈ .71, .46), and also a large anti-quitting effect on the *Quitting-Neg* trial-type, $t(27) = 3.50$, $p = .002$, $\eta^2 = .31$, $r = .56$; but by contrast, overall, they also showed a moderately-sized pro-quitting implicit tendency to affirm *Quitting-Pos*, $t(27) = -1.61$, $p = .12$, $\eta^2 = .09$, $r = .30$.

7.3.4. IRAP trial-type by Nicotine Abstinence

Between-groups Analysis

We entered the IRAP data into a 2x2x2x2 mixed -repeated measures ANOVA, crossing the between-groups variable nicotine abstinence (i.e. the abstainer group versus the control group) with three within-subjects factors: the *nicotine abstinence time factor* from the first relapse-IRAP to the second (i.e. spanning the 24-hour time period before stress induction), and the two IRAP trial-type variables. This resulted in only one interaction between nicotine abstinence and time; namely, a moderately-sized interaction with IRAP target-type, $F(1, 26) = 2.64$, $p = .12$, $\eta_p^2 = .09$, $r \approx .30$ (i.e. all other interactions involving these variables were null, $F[1, 26]$s ≤ .83, ps ≥ .37, η_p^2s ≤

.03, rs $\leq$ .18).[83] As per Figure 7.2, this interaction implied that nicotine abstinence made the current smokers more inclined to affirm smoking, and to discount trying to quit smoking, when these topics were phrased in terms of relevant positive feelings. And by contrast, this interaction also suggested that nicotine abstinence encouraged the opposite pattern across the two IRAP trial-types phrased in terms of corresponding negative feelings. However, follow-up analyses revealed that the relevant simple effects were both null, $F(1, 26)$s $\leq$ .61, ps $\geq$ .44, η_p^2s $\leq$ .02, rs $\leq$ .15. On balance, therefore, given that all other interactions between the experimental groups and time period 1 were null, nicotine abstinence appeared to have little if any effect on the smokers' implicit evaluating as measured by the current relapse-IRAP.

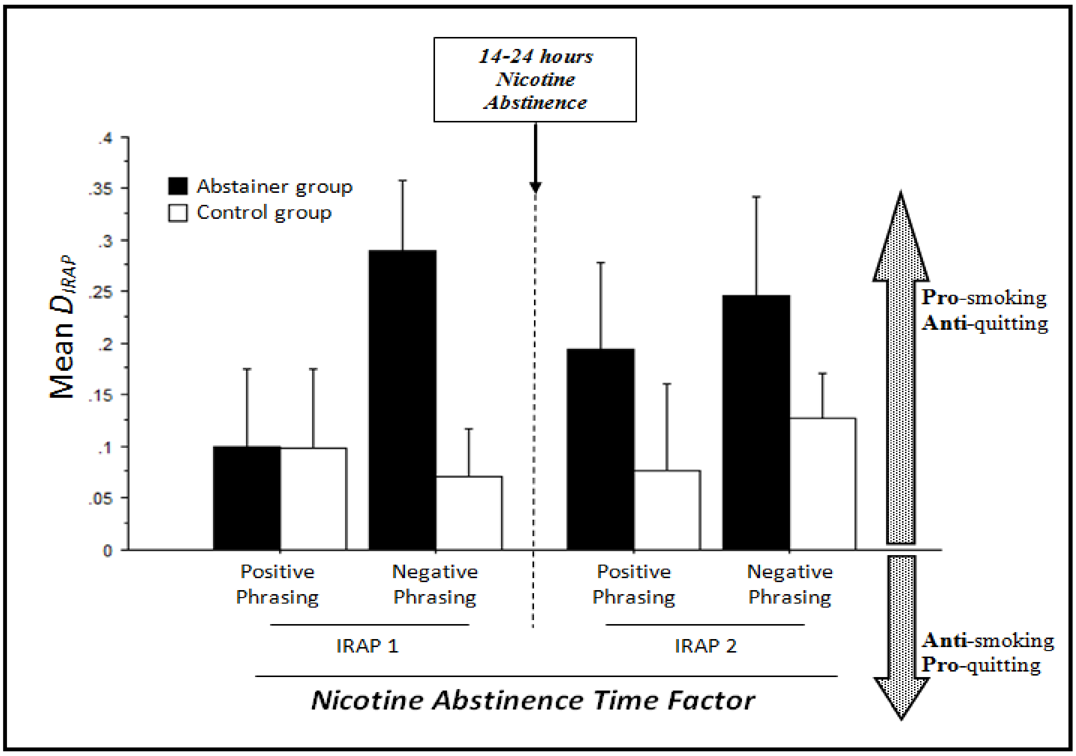

Figure 7.2. Mean *trial-type* D_{IRAP} scores, with standard error bars, for the abstainer group versus the control group smokers grouped by IRAP target-type and across time period 1.

[83] For clarity, we omitted IRAP trial block order from our primary analyses of how nicotine abstinence and acute stress impacted smokers' implicit evaluating. In particular, the current study incorporated only the minimum number of participants usually required per IRAP block order condition to make parametric statistical comparisons of difference legitimate (i.e. ns = 5-8; see see Lane et al., 2007; pp. 88-90; VanVoorhis & Morgan, 2007, p. 48). Therefore, whatever about analysing block order effects in terms of the two IRAP trial-type variables at baseline with respect to one or two groups (as in the previous chapter), there was a geometrically greater chance of observing spurious block order effects had we incorporated this variable into our primary analysis of how nicotine abstinence and acute stress impacted smokers' implicit evaluating – and particularly given the unprecedented nature of these primary analyses. Thus, to avoid distracting from the central focus of this research with speculations about secondary and indeed premature issues involving IRAP block order, we postponed these latter issues for clarification by future research. Crucially, the current primary analyses were relatively safe from any such confounds insofar as it counterbalanced the block order variable, and also insofar as all of the block order interactions we observed in previous chapters were relatively modest in size (i.e. as compared to the effects of the primary independent variables they interacted with in each case).

7.3.5. IRAP Block Sequence Analyses of Nicotine Abstinence by Acute Stress

We entered the IRAP data into a 2x2x2x5 mixed-repeated measures ANOVA, crossing the between-groups variable nicotine abstinence with three within-subjects factors: the two IRAP trial-type variables, and a five level *acute stress time factor* spanning the standard *trial-type* D_{IRAP}s from the second relapse-IRAP and the four block pair *trial-type* D_{IRAP}s from the third relapse IRAP. This resulted in a moderate-to-small interaction among all four variables, $F(1, 26) = 1.09$, $p = .37$, $\eta_p^2 = .04$, $r \approx .20$, which qualified a moderately-sized three-way interaction between nicotine abstinence, the acute stress time factor and IRAP target-type, $F(1, 26) = 2.59$, $p = .04$, $\eta_p^2 = .09$, $r \approx .30$; and also a two-way interaction between the two former variables, $F(1, 26) = 2.45$, $p = .05$, $\eta_p^2 = .09$, $r \approx .29$. The only remaining interaction involving nicotine abstinence and the acute stress time factor was that with IRAP concept-type – and it was null, $F(1, 26) = .31$, $p = .87$, $\eta_p^2 = .01$, $r \approx .11$. In broad terms, this indicated that all four *trial-type* D_{IRAP} scores functioned differently to each other with respect to the interaction between the nicotine abstinence and the acute stress time factor. To explore the nature of this four-way interaction, we therefore conducted a one-way follow-up ANOVA on each *trial-type* D_{IRAP}, crossing nicotine abstinence with the acute stress time factor in each case.

Smoking-Pos by Nicotine Abstinence by the Acute Stress Time Factor

Nicotine abstinence interacted to a moderate degree with the acute stress time factor on *Smoking-Pos*, $F(1, 26) = 2.04$, $p = .09$, $\eta_p^2 = .07$, $r \approx .27$, and this qualified a large main effect of nicotine abstinence, $F(1, 26) = 5.22$, $p = .03$, $\eta_p^2 = .17$, $r \approx .41$, and by contrast the acute stress time factor had no main effect, $F(1, 26) = .07$, $p = .99$, $\eta_p^2 = .003$, $r \approx .05$. Crucially, as illustrated in Figure 7.3, the main effect for nicotine deprivation resulted almost exclusively from the abstainers being a large degree more pro-smoking than the controls on the first two block pair *Smoking-Pos* D_{IRAP}s immediately after stress induction (i.e. respectively, $t(26)$s = 2.63, 2.15; $p = .01, .04$; $\eta^2 = .21, .15$; $r \approx .46, .39$). In fact, there was no difference between the abstainers and the control group on *Smoking-Pos* during either the second relapse-IRAP, or the final two test block pairs of the third relapse-IRAP, $t(26)$s $\leq .67$, $p \geq .51$, η^2s $\leq .02$, rs $\leq .13$.

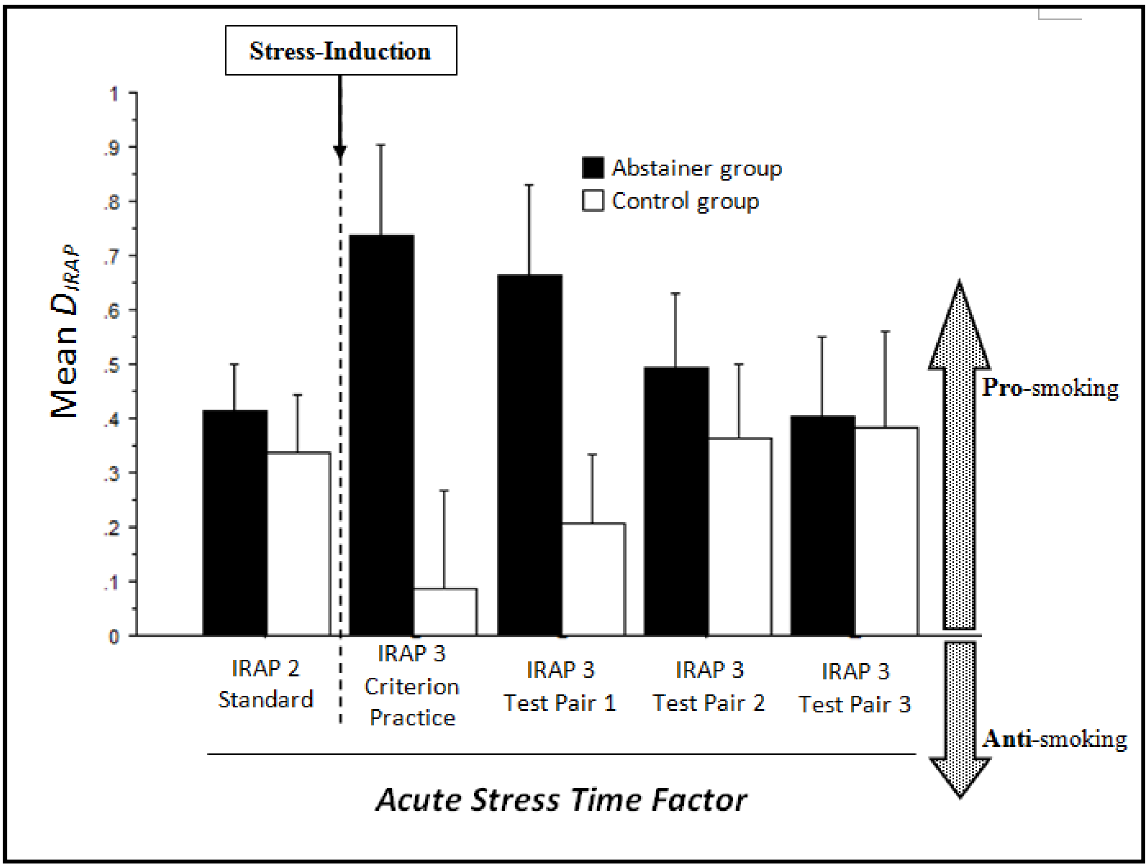

Figure 7.3. Mean *Smoking-Pos* D_{IRAP} scores, with standard error bars, for the abstainer group versus the control group smokers split by the acute stress time factor.

Confirming this, planned follow-up ANOVAs revealed a moderate-to-large interaction between nicotine abstinence and acute stress across the *standard Smoking-Pos* D_{IRAP}s from the second relapse-IRAP versus the *criterion practise Smoking-Pos* D_{IRAP}s from the third relapse-IRAP, $F(1, 26) = 4.03$, $p = .06$, $\eta_p^2 = .13$, $r \approx .37$. And moreover, this effect appeared to hold from criterion practise to test block pair 1 on the third relapse-IRAP, $F(1, 26) = .38$, $p = .54$, $\eta_p^2 = .01$, $r \approx .12$, before then mostly reversing from test block pairs 1 to 2 of the third relapse-IRAP, $F(1, 26) = 1.51$, $p = .23$, $\eta_p^2 = .05$, $r \approx .23$, and stabilising to its original levels from test block pairs 2 to 3 on that IRAP, $F(1, 26) = .16$, $p = .69$, $\eta_p^2 = .006$, $r \approx .08$. Moreover, the foregoing pattern of stress induced changes occurred, even though both the abstainers and the control group already exhibited a very strongly pro-smoking *Smoking-Pos* D_{IRAP} during relapse-IRAP 2, shortly before the relevant stress induction (i.e. respectively, ts = 4.94, 3.15; ps ≤ .008; η^2s = .64, .45; rs ≈ .80, .67).

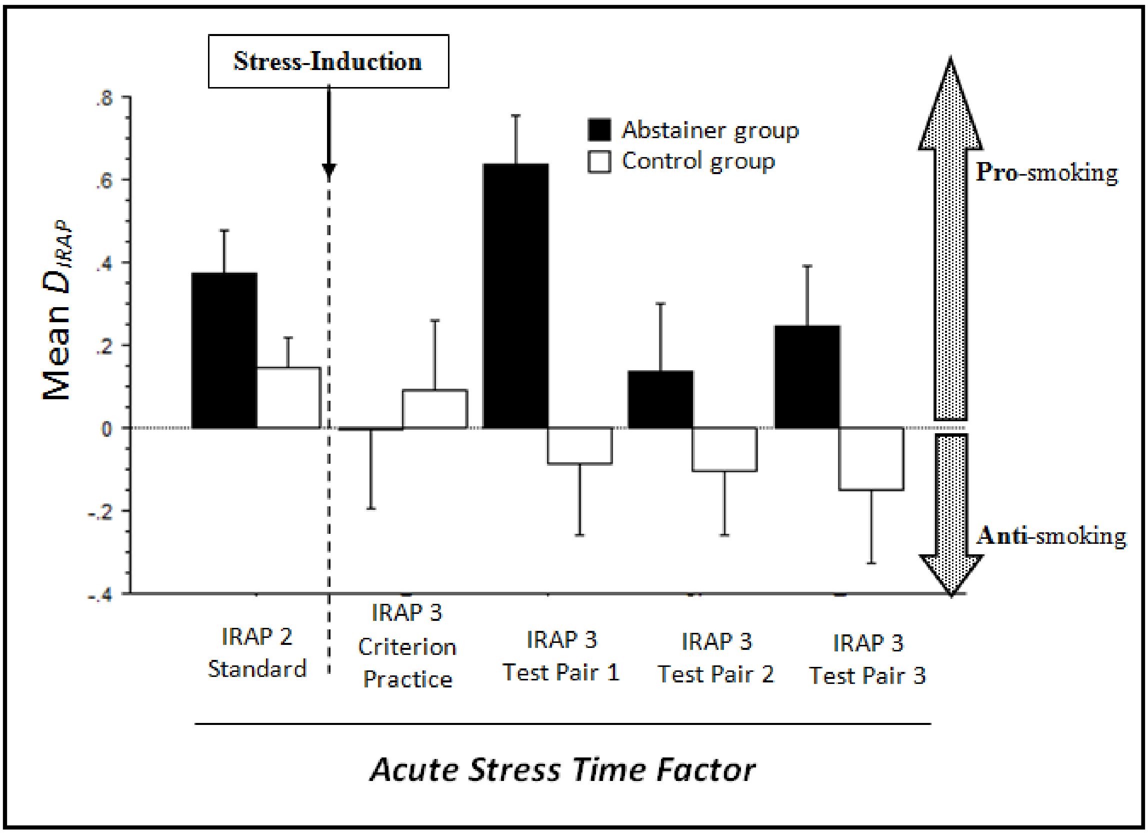

Figure 7.4. Mean *Smoking-Neg* D_{IRAP} scores, with standard error bars, for the abstainer group versus the control group smokers split by the acute stress time factor.

Smoking-Neg by Nicotine Abstinence by the Acute Stress Time Factor

Nicotine abstinence interacted to a moderate degree with the acute stress time factor on *Smoking-Neg*, $F(1, 26) = 2.37$, $p = .06$, $\eta_p^2 = .08$, $r \approx .29$, and this qualified a large main effect of nicotine abstinence, $F(1, 26) = 5.29$, $p = .03$, $\eta_p^2 = .17$, $r \approx .41$, and a moderately-sized main effect due to the acute stress time factor, $F(1, 26) = 1.76$, $p = .14$, $\eta_p^2 = .06$, $r \approx .25$. As illustrated in Figure 7.4, planned follow-up ANOVAs revealed that the abstainers initially became moderately less pro-smoking on *Smoking-Neg* as a result of the acute stress induction during the criterion practise blocks for relapse-IRAP 3, $F(1, 26) = 1.95$, $p = .17$, $\eta_p^2 = .07$, $r \approx .26$. However, shortly thereafter, during the first test block pair of relapse-IRAP 3 the abstainer's *Smoking-Neg* D_{IRAP} rebounded to double that degree in the pro-smoking direction, $F(1, 26) = 8.76$, $p = .007$, $\eta_p^2 = .25$, $r \approx .50$; before then reducing back to near its original levels during test block pair 2, $F(1, 26) = 4.36$, $p = .05$, $\eta_p^2 = .14$, $r \approx .38$, and test block pair 3, $F(1, 26) = .26$, $p = .60$, $\eta_p^2 = .01$, $r \approx .10$.

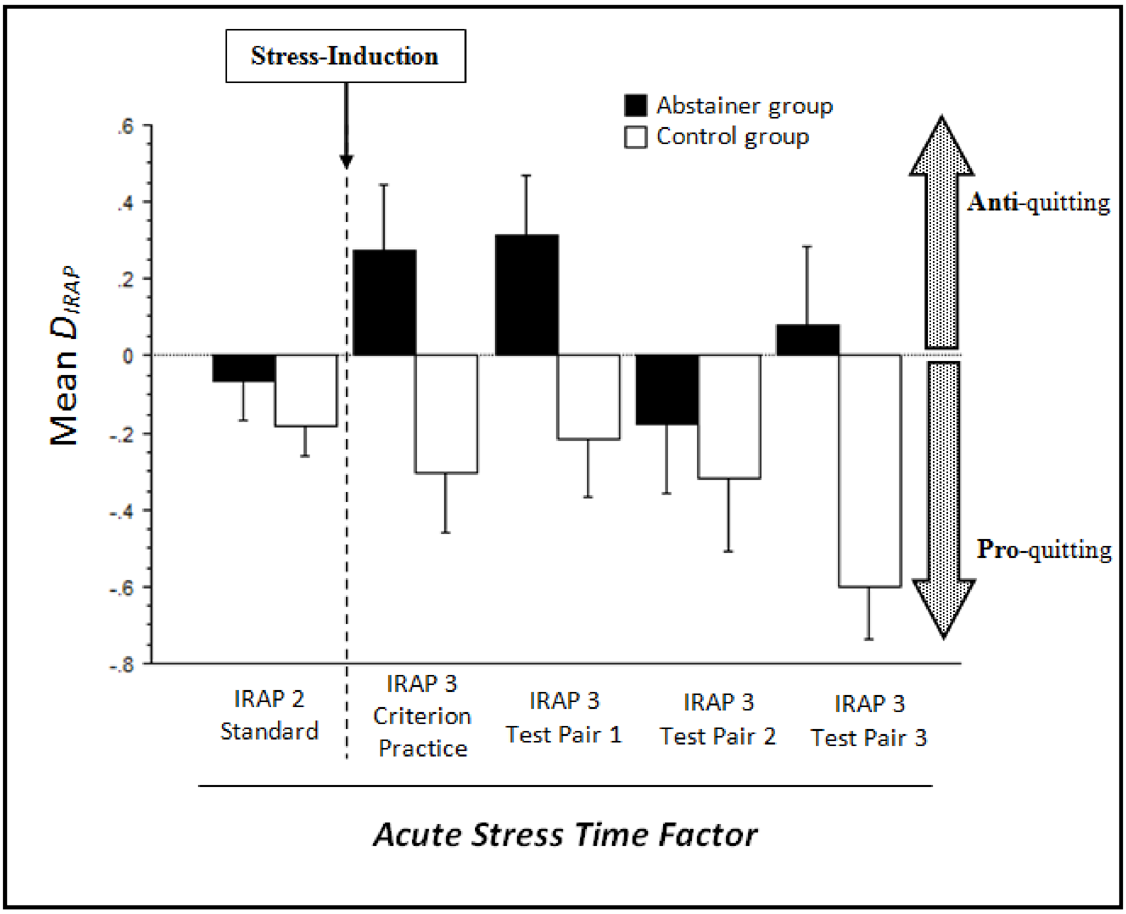

Figure 7.5. Mean *Quitting-Pos* D_{IRAP} scores, with standard error bars, for the abstainer group versus the control group smokers split by the acute stress time factor.

Quitting-Pos by Nicotine Abstinence by the Acute Stress Time Factor

Nicotine abstinence interacted to a moderate degree with the acute stress time factor on *Quitting-Pos*, $F(1, 26) = 1.58$, $p = .18$, $\eta_p^2 = .06$, $r \approx .24$, and this qualified a large main effect of nicotine abstinence, $F(1, 26) = 10.38$, $p = .003$, $\eta_p^2 = .29$, $r \approx .53$, and a moderately-sized main effect due to the acute stress time factor, $F(1, 26) = 1.76$, $p = .14$, $\eta_p^2 = .06$, $r \approx .25$. As illustrated in Figure 7.5, planned follow-up ANOVAs revealed that the abstainers initially became moderately more pro-smoking on *Quitting-Pos* as a result of the acute stress induction during the criterion practise blocks for relapse-IRAP 3, $F(1, 26) = 3.32$, $p = .08$, $\eta_p^2 = .11$, $r \approx .34$; and that this effect lasted for the first test block pair of relapse-IRAP 3, $F(1, 26) = .02$, $p = .89$, $\eta_p^2 = .0009$, $r \approx .03$, before partially reversing during the next test block pair, $F(1, 26) = 1.49$, $p = .23$, $\eta_p^2 = .05$, $r \approx .23$, and then restoring again during the final test block pair, $F(1, 26) = 3.45$, $p = .07$, $\eta_p^2 = .12$, $r \approx .34$.

Quitting-Neg by Nicotine Abstinence by the Acute Stress Time Factor

Nicotine abstinence did not interact with the acute stress time factor on *Quitting-Neg*, $F(1, 26) = .69$, $p = .60$, $\eta_p^2 = .03$, $r \approx .16$; and nor was there a main effect of the acute stress time factor, $F(1, 26) = .23$, $p = .92$, $\eta_p^2 = .009$, $r \approx .09$; but only a borderline main effect of nicotine abstinence, $F(1, 26) = 1.07$, $p = .31$, $\eta_p^2 = .04$, $r \approx .20$ (i.e. note that as per our nicotine abstinence analyses above this main effect was due to random baseline differences).

7.3.6. IRAP trial-type analyses of Nicotine Abstinence by Acute Stress while Disregarding Block Order[84]

We entered the IRAP data into a 2x2x2x2 mixed-repeated measures ANOVA, crossing the between-groups variable nicotine abstinence with three within-subjects factors: the acute stress time factor disregarding block sequence; and the two IRAP trial-type variables. This resulted in only two interactions between nicotine abstinence and the acute stress time factor; namely, a moderately-sized interaction with each other, $F(1, 26) = 2.58$, $p = .12$, $\eta_p^2 = .09$, $r \approx .30$, and a smaller one with IRAP target-type, $F(1, 26) = 1.57$, $p = .22$, $\eta_p^2 = .06$, $r \approx .24$, but none with IRAP concept-type, $F(1, 26)$s $\leq .29$, ps $\geq .59$, η_p^2s $\leq .01$, rs $\leq .11$. Crucially, as illustrated in Figure 7.6, the three-way interaction between nicotine abstinence, time period 2 and IRAP target-type indicated that acute stress plus nicotine deprivation stimulated pro-smoking/anti-quitting implicit evaluating relatively strongly for the two trial-types phrased positively (i.e. Smoking-Pos and Quitting-Pos), $F(1, 26) = 3.77$, $p = .06$, $\eta_p^2 = .12$, $r \approx .36$, but in contrast did not have an overall effect on the two trial-types phrased negatively (i.e. Smoking-Neg and Quitting-Neg), $F(1, 26) = .54$, $p = .47$, $\eta_p^2 = .02$, $r \approx .14$.

[84] Note that, as in studies 3 and 4, we did not include IRAP block order in an analysis with IRAP block sequence because the current study did not incorporate enough participants per block order condition to make it feasible to analyse stable block order effects at the level of *block-pair trial-type* D_{IRAP}s. In particular, each *block-pair trial-type* D_{IRAP} incorporated a minimal number of pairs of IRAP response latencies (i.e. six), and the current study incorporated only the minimum number of participants per IRAP block order condition to make parametric statistical comparisons of difference legitimate (i.e. *n*s = 7-12; see see Lane et al., 2007; pp. 88-90; VanVoorhis & Morgan, 2007, p. 48). Indeed, based on the fact that we have already observed stable patterns of block order effects at the level of *standard trial-type* D_{IRAP}s during studies one and two, we estimated that the current study would have required at least three times as many participants as it did in order to observe stable block order effects at the level of *block-pair trial-type* D_{IRAP}s (i.e. assuming that three times as many participants would compensate for the three times as many response latencies comprising a *standard* versus *block-pair trial-type* D_{IRAP}).

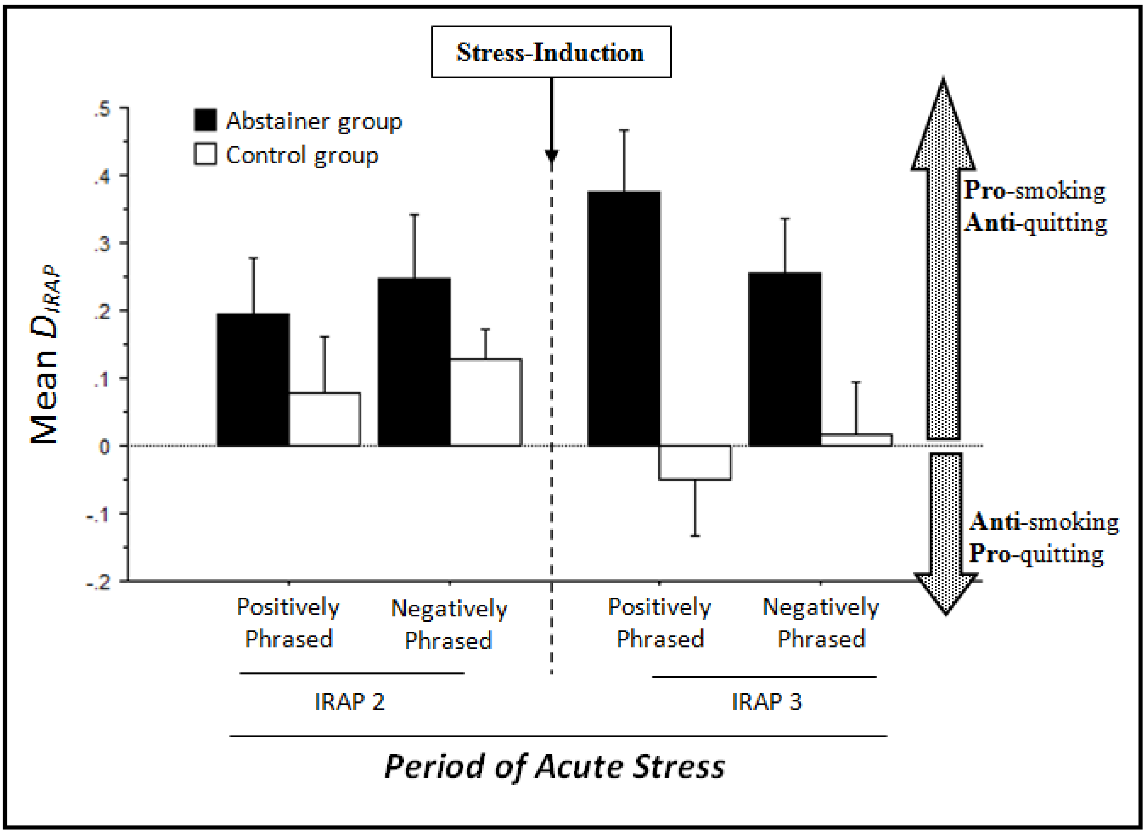

Figure 7.6. Mean *trial-type* D_{IRAP} scores, with standard error bars, for the abstainer group versus the control group smokers grouped by IRAP target-type and across acute stress time factor (i.e. disregarding IRAP block sequence).

7.3.7. The Impact of Nicotine Abstinence and Acute Stress on *trial-type* D_{IRAP} Internal Reliability

Baseline trial-type D_{IRAP} Internal Reliability

The current smokers' exhibited poor internal reliability on all four IRAP trial-types from the first relapse-IRAP; and in fact on one trial-type, *Smoking-Pos*, their IRAP effect even exhibited some degree of negative internal reliability, $r_{sb}(28) = -.42$ (i.e. *compensated* $r_{sb} = -.59$). By contrast, the smokers' remaining three trial-type effects each exhibited almost a complete lack of internal reliability, $-.16 \leq r_{sb}s \leq -.03$ (i.e. $-.43 \leq$ *compensated* $r_{sb}s \leq -.11$).

Analyses of the Impact of Nicotine Deprivation on trial-type D_{IRAP} Internal reliability

The internal reliability of the control group's *Smoking-Pos* D_{IRAP}, $r_{sb}(12) = .74$ (i.e. *compensated* $r_{sb} = .92$), and their *Quitting-Pos* D_{IRAP}, $r_{sb}(13) = .75$ (i.e. *compensated* $r_{sb} = .92$), was moderately higher during the second relapse-IRAP as compared to at baseline, $Zs \geq 2.76$, $ps \leq .005$ (i.e. $rs \geq .43$).[85] By contrast, however, the internal reliability of their *Smoking-Neg* D_{IRAP}, $r_{sb}(13) = -.68$ (i.e. *compensated* $r_{sb} = -.89$), and their *Quitting-Neg* D_{IRAP}, $r_{sb}(13) = -.60$ (i.e. *compensated* $r_{sb} = -.86$), were if

[85] We calculated these Z-tests based on the uncorrected *trial-type* D_{IRAP} r_{sb}s.

anything more negative during the second relapse-IRAP as compared to baseline (i.e. respectively, *Z*s = 1.98, 1.08, *p*s = .05, .28; i.e. *r*s = .32, .17).

The abstainers, by contrast, exhibited low internal reliability on all but one of the four *trial-type* D_{IRAP}s resulting from the second relapse-IRAP. Namely, they exhibited relatively positive internal reliability on *Quitting-Neg* D_{IRAP}, $r_{sb}(15) = .41$ (i.e. *compensated* $r_{sb} = .74$), which was moderately higher than at baseline, $Z = 2.18$, $p = .03$ (i.e. $r \approx .33$). And by contrast, the abstainers' exhibited internal reliability on the remaining three trial-type effects that was similar to the baseline internal reliability of those trial-type effects, *Z*s ≤ .71, *p*s ≥ .48 (i.e. *r*s ≤ .11). Namely, they exhibited negative internal reliability on their *Smoking-Pos* D_{IRAP}, $r_{sb}(15) = -.52$ (i.e. *compensated* $r_{sb} = -.81$), and on their *Smoking-Neg* D_{IRAP}, $r_{sb}(15) = -.32$ (i.e. *compensated* $r_{sb} = -.65$), coupled with near-zero internal reliability on their *Quitting-Pos* D_{IRAP}, $r_{sb}(15) = -.08$ (i.e. *compensated* $r_{sb} = -.26$).

Analyses of the Impact of Nicotine Deprivation and Acute Stress on trial-type D_{IRAP}

Internal reliability

During the third relapse-IRAP, following the stress induction tasks, the internal reliability of the control group's *Smoking-Pos* D_{IRAP}, $r_{sb}(13) = -.13$ (i.e. *compensated* $r_{sb} = -.37$), and their *Quitting-Pos* D_{IRAP}, $r_{sb}(12) = .39$ (i.e. *compensated* $r_{sb} = .72$), were both reduced relative to their relatively respective high internal reliabilities during the second relapse-IRAP (i.e. respectively, *Z*s = 2.35, 1.22, *p*s = .02, .22; i.e. *r*s = .47, .24). Moreover, the internal reliability of their *Quitting-Neg* D_{IRAP} remained similarly as negative during the third relapse-IRAP, $r_{sb}(13) = -.31$ (i.e. *compensated* $r_{sb} = -.64$), as during the second, $Z = .83$, $p = .41$ (i.e. $r = .17$). By contrast, however, the internal reliability of the control group's *Smoking-Neg* D_{IRAP}, $r_{sb}(12) = .46$ (i.e. *compensated* $r_{sb} = .72$), was relatively positive having been strongly negative during the second relapse-IRAP, $Z = 2.89$, $p = .004$ (i.e. $r = .58$).

The internal reliability abstainers' *Smoking-Pos* D_{IRAP} during the third relapse-IRAP, $r_{sb}(15) = .57$ (i.e. *compensated* $r_{sb} = .84$), improved to a large degree relative to during the second relapse-IRAP, $Z = 3.0$, $p = .003$ (i.e. $r = .55$). However, the internal reliability of their *Smoking-Neg* D_{IRAP}, $r_{sb}(15) = -.45$ (i.e. *compensated* $r_{sb} = -.77$) was similarly as negative during the third relapse-IRAP as during the second, $Z = .37$, $p = .71$ (i.e. $r = .07$). And indeed, their *Quitting-Pos* D_{IRAP}, $r_{sb}(15) = -.78$ (i.e. *compensated* $r_{sb} = -.93$) became more negative during the third relative to the second relapse-IRAP, $Z = 1.75$, $p = .08$ (i.e. $r = .32$).

7.3.8. Analyses of the Impact of Nicotine Abstinence and Acute Stress on Explicit Evaluating of Smoking and Quitting Smoking

Baseline Analyses

We initially entered the baseline explicit evaluating data into a 2x2x2 repeated-measures ANOVA, which crossed nicotine abstinence with the two IRAP trial-type topic variables: concept-type and target-type. This resulted in one moderately-sized main effect for target-type, $F(1, 26) = 3.90$, $p = .06$, $\eta_p^2 = .13$, $r \approx .36$, but both other main effects, F(1, 26)s $\leq .65$, ps $\geq .43$, η_p^2s $\leq .02$, rs $\leq .16$, and all three interaction effects were null, F(1, 26)s $\leq .08$, ps $\geq .37$, η_p^2s $\leq .003$, rs $\leq .05$. Planned one-sample *t*-tests revealed that the current smokers were not only strongly inclined explicitly deny "SMOKING makes Me feel BAD" and to explicitly affirm "Trying NOT to SMOKE makes Me feel BAD", $t(26)$s ≥ 3.45, $p \leq .002$, η^2s $\geq .31$, rs $\geq .55$, but even more strongly inclined to explicitly affirm "SMOKING makes Me feel GOOD" and to explicitly deny "Trying NOT to SMOKE makes Me feel GOOD", $t(26)$s ≥ 5.66, $p < .0001$, η^2s $\geq .54$, rs $\geq .74$.

On the Impact of Unaided Nicotine Abstinence on Smokers' Explicit Evaluating

We entered the feeling thermometer data into a 2x2x2x2 mixed-repeated measures ANOVA, crossing the between-groups variable nicotine abstinence with three within-subjects factors: the nicotine abstinence time factor from the first relapse-IRAP to the second (i.e. spanning the 24-hour time period before stress induction), and the two IRAP trial-type topic variables concept-type and target-type. This resulted in a moderately-sized interaction among all four variables, $F(1, 26) = 2.20$, $p = .15$, $\eta_p^2 = .08$, $r = .28$, which qualified a similarly-sized interaction among nicotine abstinence, the nicotine abstinence time factor and target-type, $F(1, 26) = 2.16$, $p = .15$, $\eta_p^2 = .08$, $r = .28$; and, both remaining interactions between the former two variables were null, $F(1, 26)$s $\leq .55$, ps $\geq .47$, η_p^2s $\leq .02$, rs $\leq .14$. Follow-up ANOVAs revealed that nicotine abstinence only impacted the current smokers' explicit evaluating of one of the four relevant topics; namely it made them less inclined to explicitly affirm "Trying NOT to SMOKE makes Me feel BAD", $F(1, 26) = 1.55$, $p = .22$, $\eta_p^2 = .06$, $r = .24$ (i.e. the interaction effects for the three remaining trial-type topics were all null, $F(1, 26)$s $\leq .70$, ps $\geq .41$, η_p^2s $\leq .03$, rs $\leq .16$).

On the Impact of Unaided Nicotine Abstinence and Acute Stress on Smokers' Explicit Evaluating

We entered the IRAP data into a 2x2x2x2 mixed-repeated measure ANOVA, crossing the between-groups variable nicotine abstinence with three within-subjects factors: the acute stress time factor (i.e. disregarding block sequence), and the two IRAP trial-type variables. This resulted in only one three-way interaction between nicotine abstinence and the acute stress time factor: namely, a moderately-sized interaction with the target-type topic variable, $F(1, 26) = 1.68$, $p = .21$, $\eta_p^2 = .06$, $r = .25$ (i.e. all three other interactions involving nicotine abstinence and the acute stress time factor were null, $F(1, 26)$s $\leq .97$, ps $\geq .33$, η_p^2s $\leq .036$, rs $\leq .19$). However, follow-up analyses revealed that acute stress under nicotine abstinence had no effect on either the current smokers' explicit evaluating of the positively-phrased *Smoking-Pos* and *Quitting-Pos* trial-type topics, $F(1, 26) = .17$, $p = .68$, $\eta_p^2 = .0007$, $r = .08$, or upon their explicit evaluating of the negatively-phrased *Smoking-Neg* and *Quitting-Neg* trial-type topics, $F(1, 26) = .07$, $p = .80$, $\eta_p^2 = .002$, $r = .05$. Therefore, on balance, the acute stress induction had relatively little impact on the abstainers explicit evaluating relative to the control group.

However, as per a moderately-sized interaction between the acute stress time factor and the two trial-type topic variables, $F(1, 26) = 2.38$, $p = .14$, $\eta_p^2 = .08$, $r = .29$, the stress induction tasks did differentially affect the current smokers' explicit evaluating of the four relevant topics – and crucially, irrespectively of nicotine abstinence. Namely, follow-up F-tests indicated that these tasks moderately increased the extent to which the current smokers were inclined to explicitly deny "SMOKING makes Me feel BAD", $F(1, 26) = 1.32$, $p = .26$, $\eta_p^2 = .05$, $r = .22$, explicitly affirm "Trying NOT to SMOKE makes Me feel GOOD", $F(1, 26) = 1.61$, $p = .22$, $\eta_p^2 = .06$, $r = .24$. And that by contrast, the acute stress induction did not change the extent to which the current smokers were inclined to explicitly affirm "SMOKING makes Me feel GOOD", $F(1, 26) = .01$, $p = .94$, $\eta_p^2 = .0002$, $r = .02$, or the extent to which they were inclined to explicitly affirm "Trying NOT to SMOKE makes Me feel BAD", $F(1, 26) = .05$, $p = .83$, $\eta_p^2 = .002$, $r = .04$.

7.4. DISCUSSION

We confirmed with multiple manipulation checks that at baseline both experimental groups were moderately inclined to cope with their tobacco cravings by deliberately suppressing them, and that crucially, after 14-24 hours of unaided nicotine abstinence this inclination actually increased relative to the control group. Indeed, further manipulation checks confirmed that just as the abstainers were becoming more inclined to cope with their tobacco cravings using suppression-oriented strategies they explicitly reported experiencing fewer positive emotions, less motivation to engage in commonly valued activities, and more negative moods due to nicotine abstinence. Moreover, this pattern worsened further as a result of the stress induction. In particular, the stress induction tasks induced large decreases in how much both experimental groups reported experiencing happiness, in tandem with how much they reported experiencing increases in the extent to which they experienced irritability, frustration – and crucially, difficulty concentrating. The latter finding is particularly noteworthy given that suppression-oriented strategies for coping with tobacco cravings and/or related negative affect depend fundamentally upon one's ability to maintain concentration on the task of doing so. In addition, these early stages of unaided nicotine abstinence also appeared to induce substantial increases in the current smokers' reward-focused and relief-focused tobacco cravings.

The stress induction tasks did not, however, increase these latter questionnaire-based ratings any further among the abstainers. And yet, they did increase the control groups' reward-focused tobacco cravings and feelings of frustration to about the same respective levels as induced by nicotine abstinence in the abstainer group. In other words, it appeared that while the abstainers reported noticing some changes in their mood due to the stress induction tasks, they did not report any changes in their tobacco cravings or their levels of frustration. Overall, therefore, it appeared as though abstaining from nicotine imposed a ceiling-effect upon the current smokers' ability to notice changes in their smoking-related implicit evaluating (i.e. in the form of tobacco cravings), which did not apply to their moods (as much).

Indeed, confirming this further, we observed no changes in the current smokers' explicit evaluating of the four IRAP trial-type topics examined by the IRAP due to acute stress, except for an ironic decrease in the extent to which both groups' were inclined to explicitly affirm that "SMOKING makes Me feel BAD", $F(1, 26) = 1.32$, $p = .26$, $\eta_p^2 = .05$, $r = .22$, and an ironic increase in their tendency to explicitly affirm that "Trying NOT to SMOKE makes Me feel GOOD." Indeed, likewise, the only change that we observed in smokers' explicit evaluating of the four IRAP trial-type topics due to nicotine abstinence was an ironic decrease in the extent to which they were inclined to affirm "Trying NOT to SMOKE makes Me feel BAD". In other words, counter intuitively, the smokers' questionnaire-based evaluating implied that unaided nicotine abstinence, and/or the stress induction tasks made them more, not less inclined to quit smoking.

Crucially, however, the current smokers' implicit evaluating broadly implied the opposite of this corresponding explicit evaluating. Firstly, we observed no changes in the smokers' implicit evaluating of any of the four IRAP trial-types due to nicotine abstinence. Rather just like at baseline, at the beginning of follow-up both experimental groups were strongly inclined to affirm "SMOKING makes Me feel GOOD"; to deny "SMOKING makes Me feel BAD"; to affirm "Trying NOT to SMOKE makes Me feel BAD"; but also moderately inclined to affirm "Trying NOT to SMOKE makes Me feel GOOD". In other words, the current smokers exhibited highly pro-smoking implicit evaluating with respect to the *Smoking-Pos*, *Smoking-Neg*, and *Quitting-Neg* topics, but by contrast they also exhibited moderately pro-quitting implicit evaluating of the *Quitting-Pos* topic. On balance, therefore, even though all of the current smokers were willing to abstain from smoking for 14-24 hours unaided for monetary compensation, their implicit evaluating was ambivalent among these four topics both before and after this period of abstinence. Indeed, as per Figure 7.1, just as we observed a pro-quitting rebound effect on smokers' implicit affirming of *Quitting-Pos* in the pro-smoking-first IRAP condition at baseline (which ostensibly instructed a pro-smoking perspective); conversely, we also observed an opposite trend in those aspects of the smokers' pro-smoking implicit evaluating that were similarly occasional. Namely, we observed a trend for the anti-smoking-first IRAP block order (ostensibly instructing an anti-smoking perspective) to induce similarly-sized pro-smoking rebound effects in their implicit denying of *Smoking-Neg* and implicit affirming of *Quitting-Neg* – with no such trend for the smokers' relatively strong, consistent and integral tendency to implicitly affirm *Smoking-Pos*.[86]

Secondly, although the acute stress induction tasks did not affect smokers' implicit denying of the *Quitting-Neg* topic, in contrast to our explicit evaluating findings above, these tasks induced greater pro-smoking and less pro-quitting implicit evaluating on the remaining three trial-types. For example, even though both groups of smokers implicitly evaluated *Smoking-Pos* in a strongly pro-smoking manner before stress induction, the stress induction tasks nevertheless brought about a large increase in this tendency among the abstainers (i.e. relative to the control group who also received these

[86] Granted, we did not obtain a statistical interaction between IRAP trial block order and either of the two IRAP trial-type topic variables, but only a main effect of block order. However, on balance, given the relatively small sample size employed here per block order experimental cell, we opted here to tentatively report the trend of block order effects implied by Figure 7.1 on a trial-type by trial-type basis.

tasks). Crucially, however, this stress-induced increase in the abstainer's implicit affirming of *Smoking-Pos* lasted for only the time-span of approximately two pairs IRAP trial blocks. In particular, all but one of the current smokers satisfied the response criteria for the third relapse-IRAP within the first pair of practise blocks. And therefore, given that this IRAP was delivered immediately after the stress induction tasks, the effects we observed on *Smoking-Pos* across criterion practise and test block 1 of the third relapse-IRAP were captured without much if any delay. Thus, if we assume that it took participants approximately two minutes to complete each pair of IRAP trial blocks (i.e. as in Studies 3 and 4), it appeared as though the abstainers experienced large increases in their pro-smoking implicit evaluating of *Smoking-Pos* for a duration of 4-5 minutes after they had finished the stress induction tasks.

Furthermore, we also observed large co-occurring anti-quitting changes in the abstainer's implicit evaluating of *Quitting-Pos* such that whereas the control group was pro-quitting on *Quitting-Pos* throughout the third relapse-IRAP, by contrast the abstainers were anti-quitting on *Quitting-Pos* during the first two pairs of trial blocks captured by this IRAP. In fact, having then temporarily reverted to the same pro-quitting implicit evaluating of *Quitting-Pos* during the test block pair 2 of the third relapse-IRAP, the abstainers became a large degree less pro-quitting than the controls during the final pair of trial blocks captured by this IRAP. Thus, applying similar logic as above, acute stress appeared to undermine smoker's pro-quitting implicit evaluating for at least 8-9 minutes after they had finished the relevant stress induction tasks.

Indeed, as per the current smokers *Smoking-Neg* effects, we even observed some evidence that the stress induction tasks caused abstainers to become ambivalent, and indeed implicitly more defensive with regard to negative aspects of their smoking throughout the third relapse-IRAP. Namely, although the abstainers were implicitly inclined to deny "SMOKING makes Me feel BAD" shortly before stress induction, by contrast during the first block pair of the third relapse-IRAP they became implicitly indecisive about this, followed by implicitly denying *Smoking-Neg* in the next trial block pair even more strongly than originally. Moreover, during the remaining two trial block pairs of the third relapse-IRAP the abstainers continued to implicitly deny *Smoking-Neg* (albeit less so than in the previous trial block) while the control group tended to implicitly affirm *Smoking-Neg* (albeit to a relatively modest degree). Overall, therefore, it appeared as though acute stress had an even more prolonged impact in undermining smokers' pro-quitting implicit evaluating during unaided abstinence, than it had in promoting increased pro-smoking implicit evaluating integral to tobacco addiction. Moreover, the acute stress tasks did not appear to have any impact on smokers' implicit evaluating of any of the four relevant trial-type topics when they had been smoking as usual beforehand.

And yet, crucially, most of the foregoing nuances were almost entirely obscured when we analysed the smokers' IRAP data in terms of trial-type effects averaged across all four qualifying pairs of IRAP trial blocks (i.e. as per the extended D_{IRAP}-algorithm). In particular, all this latter analysis revealed was that acute stress during unaided nicotine abstinence stimulated pro-smoking/anti-quitting implicit evaluating relatively strongly for the two trial-types phrased positively (i.e. *Smoking-Pos* and *Quitting-Pos*), without affecting the corresponding implicit evaluating of those smoking as usual; but that in contrast, acute stress during unaided nicotine abstinence had no such effect on the two trial-types phrased negatively relative to smoking as usual (i.e. *Smoking-Neg* and *Quitting-Neg*).

Nonetheless, by examining the internal reliability of the smokers' standard overall trial-type effects we did find evidence that unaided nicotine abstinence, and indeed even just the expectation of unaided nicotine abstinence, appeared to induce highly conflicted implicit evaluating of smoking and quitting smoking among these smokers throughout the relevant relapse-IRAPs. Specifically, once the control group had been made aware that they were not required to abstain from nicotine, during the second relapse-IRAP they exhibited relatively high internal reliability throughout their pro-smoking implicit affirming of *Smoking-Pos* (i.e. as in Study 1), and also throughout their pro-quitting implicit affirming of *Quitting-Pos*. By contrast, when all of the current smokers were anticipating whether or not they would be randomly assigned to abstain from nicotine, they exhibited similar *Smoking-Pos* and *Quitting-Pos* but respectively with relatively negative and near-zero internal reliabilities. Likewise, during the second relapse-IRAP, following 14-24 hours of unaided nicotine abstinence, the abstainer group also exhibited similar *Smoking-Pos* and *Quitting-Pos* as the control group on the second relapse-IRAP, but again, respectively, with relatively negative and near zero internal reliabilities. In summary, it therefore appeared as though the current smokers were normally highly consistent in their implicit evaluating of *Smoking-Pos* and *Quitting-Pos*, but that when faced with unaided nicotine abstinence the current smokers became highly implicitly indecisive in their evaluating of these two topics. In fact, the negative internal reliability of the smokers' implicit evaluating of *Smoking-Pos* (both during and in anticipation of unaided nicotine abstinence) implied that not only were they implicitly indecisive about this topic when faced with nicotine abstinence, but moreover oscillating periodically between affirming versus denying this topic from IRAP trial to trial.

However, unlike the control group's implicit evaluating of *Smoking-Pos* and *Quitting-Pos* during the second relapse-IRAP, their implicit evaluating of the corresponding negatively-phrased topics (*Smoking-Neg* and *Quitting-Neg*) both exhibited a large degree of negative internal reliability. And although this oscillating implicit indecisiveness lessened somewhat, at baseline under anticipation of the possibility of unaided nicotine abstinence, the current smokers' implicit evaluating of both topics still exhibited internal reliability near zero. However, having engaged in nicotine abstinence itself, the abstainer group exhibited relatively positive internal reliability in their implicit denying of *Smoking-Neg*, even though their implicit affirming of *Quitting-Neg* exhibited near-zero internal reliability. In summary, therefore, it appeared as though the current smokers were generally very conflicted about their implicit tendency to deny *Smoking-Neg* and to affirm *Quitting-Neg* – except during nicotine abstinence when the smokers' appeared to relatively decisive about their pro-smoking tendency to implicitly deny *Smoking-Neg*. Crucially, this was in contrast to their initially internally-consistent tendency to implicitly affirm *Smoking-Pos* and to affirm *Quitting-Pos* which was undermined by (the prospect of) nicotine abstinence. Thus, just as smokers were feeling more overwhelmed by their tobacco cravings, they also became more decisive about implicitly defending their need to smoke (i.e. by asserting that it does not make them feel bad), even as they became more implicitly indecisive about whether smoking makes them feel good.

Furthermore, as a result of the stress induction tasks, even having smoked as usual, the control group's implicit evaluating of *Smoking-Pos* lost its initially high internal reliability. And yet, by contrast, the control group's implicit evaluating of *Quitting-Pos* appeared to mostly retain the high internal reliability it exhibited shortly before the stress induction tasks. In other words, following the stress induction tasks the control group were implicitly indecisive about affirming *Smoking-Pos*, but still implicitly decisive about affirming *Quitting-Pos*. Crucially, the control group explicitly reported substantial increases in their tobacco cravings as a result of the stress induction tasks (along with negative affect and decreases in positive affect). And therefore, the foregoing differential pattern of internal reliability may have resulted from the control group noticing how uncomfortable and distracting their tobacco cravings made them feel while completing the stress induction tasks (i.e. making them more implicitly indecisive about affirming *Smoking-Pos* and yet still implicitly decisive about affirming a desire to quit smoking in terms of *Quitting-Pos*).

By contrast, however, the abstainer group implicitly evaluated *Smoking-Pos* with greater internal reliability just as they came to affirm this topic ever more strongly as a result of the stress induction tasks. And moreover, at the same time, the internal reliability of their implicit evaluating of *Quitting-Pos* became severely negative (having been highly positive) just as they came to implicitly deny this topic as a result of the stress induction tasks (i.e. having implicitly affirmed it shortly before stress induction). Indeed, similarly, the abstainers also exhibited relatively low internal reliability in their implicit affirming of *Smoking-Neg* and their implicit affirming of *Quitting-Neg* during the final relapse-IRAP.

In addition, just as the control group's implicit affirming of *Quitting-Neg* exhibited negative (or near-zero) internal reliability (i.e. as per the second relapse-IRAP), they also did so shortly after completing the stress induction tasks. Indeed, lastly, just as the abstainers attained some degree of internal reliability in their implicit evaluating of *Smoking-Neg* shortly before stress induction as a result of nicotine abstinence (i.e. having abstained from nicotine), once the control group had experienced the stress induction tasks the internal reliability of their implicit denying of *Smoking-Neg* similarly improved (having been near-zero beforehand).

Overall, therefore, the foregoing differential pattern of internal reliability effects confirmed our earlier block sequence analyses of the smokers' *Smoking-Pos*, *Smoking-Neg* and *Quitting-Pos* trial-type effects – namely that the stress induction tasks severely undermined smokers' pro-quitting implicit evaluating while simultaneously amplifying and stabilising pro-smoking aspects of their implicit evaluating integral to tobacco addiction. Crucially, from this point of view, the current findings systematically implied that smoker's usual efforts to engage in suppression-oriented management of their tobacco cravings during unaided nicotine abstinence appeared to be highly self-defeating when most needed – during acute stress. That is, both with respect to reducing tobacco cravings, and also with respect to (deliberately) maintaining one's own motivation to continue such efforts (i.e. particularly given that both experimental groups reported experiencing greater difficulties concentrating when stressed). Moreover, in revealing such things, the current findings also provided a plausible explanation of the mystery as to why the literature has failed over a relatively long period of time to obtain any consistent effects of nicotine deprivation upon smoking-related implicit cognition – namely, because they did not account for the interaction of acute stress with nicotine deprivation; and nor did they account in any way for the strategies that smokers' used to engage in unaided nicotine abstinence. Indeed, it is worth noting, that even though both the control group and the abstainers were cued for smoking after the stress induction tasks using their favourite brand of tobacco, even then only the abstainers implicit evaluating was impacted by the stress induction tasks (i.e. at least for the minimum two minute time periods within which we measured that implicit evaluating).

CHAPTER 8: GENERAL DISCUSSION

8.1. An Overview of the Main Findings in the Current Empirical Work

The present thesis aimed at developing the IRAP to provide a preliminary functional model of how implicit evaluating variously motivates tobacco addiction depending upon context. In particular, from the outset, we sought to use the IRAP to clarify two major issues about tobacco addiction: why smokers continue to smoke even when they may explicitly dislike doing so; and also to explore how smoking-related implicit evaluating responds to classic formulations of self-control involving deliberate suppression of tobacco cravings. Overall, our findings were encouraging from the point of view that throughout our research the IRAP consistently achieved an unmatched degree of discriminant and criterion validity not just in cross-section, but also experimentally. Indeed, as we review further below, the IRAP not only succeeded in measuring smoking-related implicit evaluating with an unparalleled degree of precision; but moreover, owing to this precision, we were able to explore the IRAP's internal reliability and its block order effects as two potentially important new measures of the stability of implicit evaluating related to one's potential for self-control. Furthermore, owing to the relatively high periodicity of the IRAP's trial block structure, we were also able to offer the literature its first short-interval momentary time-course analysis of implicit evaluating, in order to determine how immediately, persistently and consistently it reacted to various key experimental variables related fleetingly to tobacco addiction.

Our first study, described in Chapter 4, was primarily about demonstrating that the IRAP is more effective than the IAT as a means of investigating tobacco addiction. It involved developing a smoking-related IAT with stimuli that were specifically chosen on a known-groups basis to minimise the range of extraneous smoking-related implicit evaluating normally resulting from the relativity of its tasks. To our knowledge, this was the first time that anyone has specifically designed an IAT to maximise its experimental determinacy, and thus criterion validity, by minimising the degree to which it cued extraneous implicit evaluating. As reviewed earlier, our primary aim in doing so was to set a gold-standard benchmark against which to compare the precision and criterion validity afforded by an equivalent IRAP. And indeed, confirming the overarching rationale we developed across Chapters 1-3, the relevant IAT succeeded in providing an uncommonly high-quality benchmark against which to compare the criterion validity of any smoking-related IRAP. In fact, it outperformed its predecessors in terms of correlating with *all* of the cross-sectional tobacco addiction criteria that we employed.

Moreover, in all cases, the size of the relevant criterion correlations were either unprecedented, or else at the upper end of those previously achieved in the domain. It is therefore all the more impressive that the equivalent smoking-related IRAP performed consistently better than the IAT both in terms of diagnosing smoking-status, and in terms of the correlations it achieved with all of the five continuous tobacco addiction criteria we measured. In addition, unlike the IAT, this IRAP succeeded in discriminating particular types of pro- and anti-smoking implicit evaluating as being developmentally integral versus collateral to tobacco addiction across time (i.e. years smoking). Thus, crucially, Study 1 demonstrated that even when we focused on implicit evaluating topics specifically designed to favour the IAT, the IRAP still outperformed it in terms of both discriminant and criterion validity.

Accordingly, we focused our second study upon developing an IRAP to precisely distinguish between two theoretically important sub-types of smokers' typical reasons for smoking; namely reward- versus relief-focused reasons for smoking. This seemed particularly appropriate given that the IRAP we used in Study 1 indicated that smokers' implicit evaluating of their typical reasons for smoking were broadly integral to tobacco addiction; and that in contrast, their implicit evaluating of typical reasons against smoking, and of public restrictions on their smoking, were relatively collateral to tobacco addiction. Indeed, confirming the more precise distinctions we incorporated into the IRAP in Study 2, its trial-type effects replicated and indeed systematically extended the discriminant and criterion validity achieved by the IRAP we used in Study 1. In fact, as far as we are aware, the relief-focused *Need-Neg* trial-type effect we measured in study two correlated with continuous tobacco addiction criteria at a level well above anything previously observed in the literature on smoking-related implicit cognition – and what is more, it did so consistently across all five such criteria we measured.

Indeed, owing to the mood-conditional phrasing of the trial-type topics we measured in Study 2, we were able to clarify that tobacco addiction is alternately motivated by *both* reward- and relief-focused implicit evaluative proceses in different respective mood-contexts – and in a manner which varies according to one's stage of tobacco addiction. This was an important finding from the point of view that most dominant (implicit) theories of tobacco addiction are at impasse with each other in terms of arguing about whether tobacco addiction is driven (in absolute terms) by reward- versus relief-focused (implicit) evaluating (e.g. see Conklin et al., 2004; Everitt & Robbins, 2005; Farris et al., 2015; Robinson & Berridge, 2008; Stacy & Wiers, 2010; Tibboel et al., 2011; Tibboel et al., in press). And crucially, as such, our IRAP findings offered a plausible means of reconciling multiple contradictory points of view from across the tobacco addiction literature (i.e. in broadly the same way as recommended for the literature on cognitive neuroscience by Vahey & Whelan, 2015).

More specifically, the relevant IRAP findings indicated that reward-focused implicit evaluating of smoking in celebratory contexts was dominantly pro-smoking among a group of non-smokers characteristically at risk of initiating smoking. And that by contrast, relief-focused implicit evaluating of smoking in craving-related negative mood contexts appeared to be more instrumental in the development of tobacco addiction per se, than reward-focused implicit evaluating of smoking in general. Indeed, by contrast, our IRAP findings indicated that reward-focused pro-smoking implicit evaluating was a secondary motivator of tobacco addiction that only appeared to apply in contexts that involved smokers going about routine aspects of their daily business in the absence of strong craving-related moods. Interestingly, this pattern of findings corroborated the fact that researchers have been relatively unsuccessful in inducing tobacco cravings with positive mood inductions (Heckman et al., 2013; Wray, Gass, & Tiffany, 2013). And moreover, it also corroborated the fact that smokers tend to explicitly cite reward-focused reasons for smoking only when not nicotine deprived (Veilleux et al., 2013), and/or in the context of performing mundane daily routines (Bancroft et al., 2003; Cook et al., 2004; Copeland et al., 1995; McEwen, West, & McRobbie, 2008; Vidrine, Vidrine, Costello, Mazas, Cofta-Woerpel, Mejia, & Wetter, 2009). Importantly, this pattern of findings is highly consistent with the traditional theoretical view that tobacco addiction gradually intensifies as a primary function of how much one evaluates smoking as being necessary to regulate negative moods resulting from tobacco withdrawal (i.e. rather than as a function of how much one evaluates smoking as being emotionally rewarding; see Baker et al., 2004; DiFranza, 2015; D'Souza, & Markou, 2011, p. 5; McCallion & Zvolensky, 2015).

Overall, therefore, the findings from studies one and two revealed that smokers' implicit evaluating appears to motivate tobacco addiction in a highly systematic manner depending on the presence of positive versus negative craving-related moods. And as such, the two IRAP's we used in studies one and two provided a relatively precise means of determining how implicit evaluating motivates tobacco addiction. Then, having identified what particular aspects of implicit evaluating were most predictive of tobacco addiction (and when), the remaining part of the current research sought to develop a preliminary experimental understanding of how the most common strategy used by smokers for unaided smoking-cessation typically fails to circumvent those aspects of implicit evaluating apparently motivating tobacco addiction. Namely, the third and fourth studies contained with the current thesis sought to provide not only a first short-interval momentary time-course analysis of how persistently and/or consistently thought suppression impacts any (smoking-related) implicit evaluating it contradicts, but a first momentary time-course analysis of addiction-related implicit evaluating in general. This was important from the point of view that relapse during smoking-cessation usually occurs rather precipitously, often within a period of just a few minutes (see McKay, Franklin, Patapis, & Lynch, 2006; Shiffman, 2009; see also Marhe, Waters, van de Wetering, & Franken, 2013; Waters et al., 2010).

In summary, our findings from Study 3 indicated that an ad hoc approach to thought suppression only very temporarily eliminated implicit evaluating integral to tobacco addiction; and moreover not on demand but only after intermittent delays. In addition, it also brought about pervasive and more immediate anti-smoking changes among the three other, secondary aspects of smoking-related implicit evaluating we measured. And therefore, true to its moniker, we found that smokers' ad hoc approach to deliberately suppressing the extent to which they engaged in pro-smoking evaluating likely involved deliberately contradicting different aspects of their smoking-related implicit evaluating at different stages of the unconditional-feelings-IRAP. Indeed, the fact that the impact of this approach on all four aspects of smokers' implicit evaluating was moderated by IRAP block order, confirmed that it was prone to cue-based interference from background variables.

Therefore, in our fourth empirical study, we refined our analysis by exploring the widely speculated idea that focused thought suppression of particular aspects of one's tobacco cravings might be more successful than an ad hoc approach to postponing and/or controlling any such intrusive pro-smoking implicit evaluating. Crucially, this research revealed that focused thought suppression of relief-focused pro-smoking implicit evaluating was relatively successful as compared to ad hoc implicit evaluating insofar as it immediately, consistently and persistently suppressed the relief-focused pro-smoking implicit evaluating it targeted for at least four minutes (i.e. as compared to a delayed and intermittent two minutes in Study 3). However, this focused approach to thought suppression also had severe drawbacks. Namely, at the same time as it eliminated the pro-smoking relief-focused implicit evaluating it targeted, it also immediately and relatively persistently induced ironic (pro-smoking) effects on secondary aspects of implicit evaluating related to tobacco addiction.

By neglecting to contradict or thus reduce smokers' enjoyment-focused pro-smoking implicit evaluating of *Enjoy-Pos*, it appeared as though our focused thought suppression instructions inadvertently primed smokers to implicitly evaluate other more ambiguous topics from a default reward-focused pro-smoking perspective. Overall, therefore, the current research indicated that focused thought suppression may be a more viable means of temporarily eliminating particular aspects of implicit evaluating integral to tobacco addiction than the more commonly used ad hoc approach examined in Study 3. However, these findings also indicated that any such benefits come at the cost of cueing (and perhaps sensitizing) other aspects of smokers' pro-smoking implicit evaluating. And crucially, as such, smokers' attempts to control their tobacco cravings via thought suppression appeared to be an aspect of their tobacco addiction, rather than a potential cure for it.

Therefore, in order to explore this idea further, with greater ecological validity, our final study sought to examine how smokers' implicit evaluating of both the pros and cons of smoking (and of quitting smoking) changed as a function of classic contexts for relapse during suppression-oriented nicotine abstinence. Specifically, we examined how smokers' implicit evaluating of both smoking and quitting smoking changed as a function of 14-24 hours of suppression-oriented nicotine abstinence versus smoking as usual; and also as a function of acute stress just before the end of this period of abstinence versus having smoked as usual. Using this setup we found that unaided suppression-oriented nicotine abstinence by itself had little if any impact on smokers' implicit evaluating of smoking or quitting smoking – thus broadly confirming the lack of any consistent nicotine deprivation effects in the literature on smoking-related implicit evaluating. By contrast, however, acute stress during nicotine abstinence appeared to provoke substantial pro-smoking and anti-quitting changes in smokers' implicit evaluating as compared to acute stress while smoking as usual (i.e. which appeared to have little or no effect). Thus, crucially, the current findings systematically confirmed that smoker's usual efforts to engage in suppression-oriented management of their tobacco cravings during unaided nicotine abstinence appeared to be highly self-defeating when most needed – during acute stress.

Indeed, using a similar short-interval momentary analysis as we introduced in studies three and four, we found that the impact of acute stress on smokers' implicit evaluating of smoking was particularly pronounced for between four and five minutes after it passed – that is, before the smokers' pro-smoking implicit evaluating gradually diminished to pre-stress levels. Critically, not only did this finding provide additional support for the idea that both reward- and relief-focused motivational processes are central to tobacco addiction, but moreover it systematically modelled the progression of these processes with respect to relapse during smoking-cessation for the first time; and indeed, with a degree of temporal precision that was thoroughly unprecedented in the literature.

8.2. On the Potential of the IRAP to Measure Two New Dimensions of Implicit Evaluating Stability Related to Self-control

The literature on implicit cognition has traditionally viewed implicit measures that exhibit low internal reliability or trial block order effects, as a sign that they lack validity due to extraneous influences (e.g. see Fiedler, Messner & Bleumke, 2006; Klauer & Mierke, 2005; Teige-Mocigemba, Klauer, & Sherman, 2010; Levin, Hayes, & Waltz, 2010). In essence, this point of view is born from a common ontological and epistemological assumption that human cognitive processes are purely based upon hypothetical constructs that are immutable. Crucially, however, this position largely ignores the possibility that one's implicit evaluating of a given topic might be cued in opposing ways by various, sometimes co-occurring aspects of context. Indeed, in a broader sense, it completely ignores the possibility that one's cognitive processes might evolve in any given domain as a function of learning processes (see De Houwer, Barnes-Holmes & Moors, 2013). This is a potentially important oversight given that tobacco addiction is the epitome of conflicting patterns of (implicit) evaluating that often compete within a given context as a function of ongoing deliberation (e.g. as when a smoker struggles to deliberately suppress their tobacco cravings during smoking-cessation).

Crucially, the stability with which a smoker implicitly evaluates a given trial-type topic in a pro-smoking manner within a given context (implicit indecisiveness), or indeed between contexts (perspective-switching tendency), is likely by definition to have an important bearing upon how persistently and/or consistently that smoker would be able to deliberately sustain an alternative anti-smoking perspective (i.e. with all other variables remaining equal). And in particular, we anticipated that such properties might determine how accustomed and thus prepared a smoker was to sustain deliberate self-control over their tobacco cravings and/or their smoking (i.e. within and/or among relevant contexts). As such, a major secondary aim of the current programme of research was to explore, for the first time, the stability of (smoking-related) trial-type effects both on an approximately second-by-second momentary basis as per their respective internal reliabilities within a given measurement context; and also as a function of the alternative evaluative perspectives cued by the IRAP's trial block order instructions and sequencing (i.e. the latter's prime function as a methodological variable is literally to prioritize one overarching implicit evaluating perspective over its alternative across all four coordinated IRAP trial-types during any given IRAP session).

Overall, our results throughout the current research supported the idea that the internal reliability of trial-type effects, and their malleability in response to block order cueing, both successfully measured the stability one's implicit evaluating of a given topic from a given evaluative perspective. Namely, across multiple studies we confirmed that committed smokers produced IRAP trial-type effects with relatively high internal reliability whenever they addressed topics about which smokers characteristically lacked ambivalence (such as whether smoking makes them feel good). And that by contrast, committed smokers typically produced IRAP trial-type effects with low internal reliability whenever they addressed topics about which smokers are characteristically ambivalent (such as whether quitting smoking made them feel good). Indeed, not only did we confirm various other known-groups patterns of internal reliability in cross-section, including with non-smokers, but in our final study we also experimentally induced predictable implicit indecisiveness effects in terms of internal reliability, both with respect to nicotine deprivation and acute stress. In fact, our acute stress induction procedure during nicotine abstinence induced so much implicit indecisiveness among smokers' implicit evaluating of smoking and quitting that they consistently exhibited strong negative internal reliability on multiple IRAP trial-types. In other words, the relevant smokers were so ambivalent about smoking and/or quitting smoking in those cases, that they oscillated periodically on a trial by trial basis between being inclined to affirm versus deny each of the relevant trial-type topics. Moreover, *both* groups of smokers in Study 3 who attempted to deliberately evaluate from the perspective of a non-smoker exhibited precisely the same differential pattern of internal reliability across the four relevant trial-types as non-smokers exhibited on the same IRAP during Study 1.

Likewise, the current programme of research repeatedly confirmed that smokers' and never-smokers' implicit evaluating were cued by the block order variable in line with their known respective histories for persistently adopting pro- versus anti-smoking perspectives on various pivotal smoking-related topics. And as such, it appeared that trial-typed IRAP trial block order effects were promising as a means of measuring the extent to which a smoker was inclined to persistently switch implicit evaluating perspectives on a given trial-type topic due to pro- versus anti-smoking contextual priming (i.e. which we termed a smoker's perspective-switching tendency on a given trial-type topic within a given overarching context). For example, owing to their characteristic lack of interest in smoking, never-smokers were cued to implicitly evaluate in a pro-smoking manner by pro-smoking-first trial block orders, but also similarly cued to implicitly evaluate in an anti-smoking manner by anti-smoking-first trial block orders. And by contrast, we consistently found that committed smokers were relatively disinclined to be cued in an anti-smoking direction by anti-smoking-first trial block orders. In fact, not only did the IRAP trial block order variable fail to moderate smokers' implicit evaluating on any topic that they were characteristically accustomed to evaluating in an exclusively pro-smoking manner (i.e. including when they were instructed to engage in thought suppression during Studies 3 and 4). Moreover, anti-smoking-first block orders sometimes even ironically provoked stronger pro-smoking implicit evaluating on topics about which smokers were characteristically defensive about their smoking and/or their ongoing failure to quit smoking (see Studies 2 and 5).

Overall, therefore, the current findings indicated that the internal reliability of a trial-type effect, and the size of a block order effect on a trial-type effect, are both promising measures of the stability of one's implicit evaluating – the former within a given context; and the latter between contexts that respectively cue opposing perspectives on each relevant trial-type topic. Nonetheless, given that such findings are wholly unprecedented in the literature a cautious approach is of course warranted. In particular, it would be prudent to conduct experimental research dedicated to analysing how trial-type internal reliability, and trial-typed block order effects vary as a function of variables well known to affect one's tendency to switch evaluative perspectives as per their overt (smoking-related) behaviour (e.g. using visualisation instructions, and/or mood-induction tasks as in our last three studies; also for related findings, that view IAT block order effects as confounding see Klauer & Mierke, 2005; Klauer, Schmitz, Teige-Mocigemba, & Voss, 2010). Indeed, in particular, much more research is required before it will be possible to determine how and/or to what extent the stability of one's implicit evaluating restricts one's ability deliberately cultivate any new (anti-smoking) perspectives.

Accordingly, we recommend further experimental research examining how the current provisional measures of implicit evaluating stability relate to particular deliberate self-control strategies, such as thought suppression, on a topic by topic basis. Crucially, we anticipate that such research may have important benefits for developing techniques to support intentional behavioural change regarding those topics (see below). In any case, for the moment, what seems most pertinent here is the fact that no other measure of implicit evaluating besides the IRAP is capable of investigating such questions – because no other measure is currently able to measure implicit evaluating with respect to one topic as distinct from another.

8.3. On the Additional Construct Validity Provided by the IRAP over Corresponding Questionnaire-based Self-report Measures of Evaluating

One of the primary reasons why researchers have sought to develop measures of smoking-related implicit evaluating is, as reviewed in Chapter 1, because of the fundamentally limited capacity of standard, rationalistic questionnaire-based measures of smoking-related evaluating to disentangle the irrational nature of tobacco addiction (see for example Wiers, Houben, Roefs, de Jong, Hofmann, & Stacy, 2010). Confirming this rationale, we observed multiple instances of smokers' implicit evaluating on the IRAP directly comporting with criterion variables for tobacco addiction, but nonetheless being directly contradicted by corresponding aspects of explicit evaluating of smoking. For example, in Study 1 smokers' pro-smoking-ban explicit evaluations were at odds with their strongly anti-ban *Ban-Neg* D_{IRAP} effects (i.e. as per widespread social pressure for pro-ban explicit evaluating; see Chapter 4, Introduction). And this was despite the fact that the smokers evaluated smoking favourably on both explicit measures of smoking, and also on both *Smoking-trial-type* D_{IRAP}s. Moreover, by contrast, the corresponding IAT was like any other relative measure of implicit evaluating, unable to reveal any such (direct) contradictions between implicit versus explicit evaluating because of its inability to distinguish implicit evaluating of one topic from another.

Likewise, in Study 2, we observed a pattern of pro-smoking implicit evaluating among non-smokers at risk for smoking (i.e. undergraduate students) that comported very precisely with the pattern of reasons that early-career smokers typically give (versus not) for initiating smoking. And crucially, by contrast, the relevant non-smokers were unequivocally anti-smoking in their responses to all corresponding explicit measures – likely as a result of having to confirm that they had not smoked during the previous 12 months in order to gain admittance to the study. Moreover, on the basis that smokers are not generally stigmatized, but rather are more likely to be socially accepted for offering the relevant reward- and/or relief-focused reasons for their smoking, smokers' implicit versus explicit evaluating did not conflict in this regard (see Chassin et al., 2007; Fitz et al., 2015; Vahey et al., 2010). Rather, instead, correlation data implied that smokers' explicit evaluating of smoking in Studies 1 and 2 was largely driven by those aspects of implicit evaluating we found to be most integral to tobacco addiction.

Furthermore, in Study 5, the smokers' questionnaire-based evaluating implied that unaided nicotine abstinence, and/or the stress induction tasks made them more, not less inclined to quit smoking. Crucially, this was at odds with the fact that every one of the smokers who abstained from smoking for 14-24 hours smoked immediately after that period expired; and despite the fact that smokers' pro-smoking/anti-quitting implicit evaluating increased in response to acute stress during suppression-oriented nicotine abstinence. Indeed, incidentally, this discrepancy between implicit versus explicit evaluating tallied very well with the fact that in studies 3 and 4 smokers consistently reported imagining scenarios that stigmatized smoking as their primary means of relating to smoking from the perspective of a life-long non-smoker. Overall, therefore, the current programme of research repeatedly confirmed the IRAP's ability to reveal aspects of smoking-related evaluating that were simply not available to corresponding measures of explicit evaluating. And indeed, this is putting aside the fact that traditional questionnaires are not equipped to measure one's implicit indecisiveness or perspective-switching tendencies with regard to a given topic.

8.4. Methodological Insights and Issues Arising from the Current Research

The current programme of research involved developing multiple refinements of existing methods of analysing and interpreting IRAP data. For example, as we have already reviewed, the current research revealed the potential of the IRAP to estimate implicit indecisiveness and perspective-switching tendencies. Moreover, the current research was also the first to analyze trial-type effects in terms of both IRAP concept-type and target-type – crucially, this provided the first means of systematically estimating the extent to which all four trial-type effects operated independently of each other (with respect to any relevant experimental variables). By contrast, IRAP research to date has typically combined IRAP trial-type scores with little if any stated empirical or a priori rationale for doing so (e.g. see Hussey, Thompson, McEnteggart, Barnes-Holmes, & Barnes-Holmes, 2015, pp. 160-162). And as such, the current approach to analysing the independence of trial-type effects offered the IRAP literature a more objective, systematic and indeed precise means of determining whether or not it is functionally appropriate to combine IRAP trial-type scores in one's analysis or not.

Likewise, our analyses of the reliability of IRAP trial-type effects also highlighted for the first time, the importance of accounting for the fact that each IRAP trial-type effect is typically comprised of far fewer trials than many other measure of implicit cognition, such as the IAT, that are well known for exhibiting good internal reliability. In particular, by employing the algorithm that we improvised from Spearman-Brown's generalized prediction formula in Appendix 11, it became clear from Study 1 onwards that smoking-related IRAP trial-type effects were capable of exemplary levels of internal reliability (i.e. conditional upon variables related to implicit indecisiveness), that would otherwise have been severely underestimated using existing data-analytic techniques in the IRAP literature.

In addition, studies one and two also highlighted how bootstrapped meditational analyses of cross-section data has the potential to be a useful precursor to experimental research insofar as it demonstrated an ability to efficiently extract tentative causal information about the likely motivational importance of each given type of implicit evaluating. Specifically, such analyses proved useful in estimating the extent to which a given aspect of implicit evaluating is causally integral versus collateral to a given criterion variable – and crucially, this informed what aspects of smoking-related implicit evaluating we targeted as part of our subsequent experimental analyses (e.g. our choice to target relief- as opposed to reward-focused pro-smoking implicit evaluating in Study 4). Indeed, from the outset, the bootstrapped analyses we performed in Study 1 were instrumental in determining how to proceed with refining the precision with which we examined smokers' implicit reasons for smoking in Study 2 – and ultimately, this yielded the unprecedented criterion validity achieved by the relief-focused *Need-Neg* trial-type.

Furthermore, new distribution-based analytic techniques that were introduced in Study 1 for examining the homogeneity of IAT and IRAP effects, indicated that the former and but not the latter effects confounded multiple types of implicit evaluating not only within and among participants, but also among contexts. And indeed, this was despite the fact that the IAT in Study 1 was the first to be specifically designed to minimize the evaluative indeterminacy of its scores. For example, we developed a new type of chi-squared interval analysis (detailed in Appendix 8) which revealed that the smokers' IAT effects were distributed in a polarized, bimodal fashion around zero such that they tended either to favour pro-smoking/anti-smoking-ban IAT trials over anti-smoking/pro-smoking-ban IAT trials, or vice versa. As such, the IAT appeared to confound not just qualitatively different types of implicit evaluating among participants, but also contrasting types; and moreover it did so even among smokers who were specifically sampled for pro-smoking evaluating as opposed to anti-smoking evaluating. Crucially, by contrast, each of the corresponding IRAP trial-type effects were uniformly, and normally distributed across zero, thus indicating that they were respectively measuring implicit evaluating homogeneously from participant to participant.

In addition, we also found similarly sized IAT block order effects for smokers as for non-smokers – thus suggesting that the smokers' IAT effects were measuring different types of implicit evaluating in the pro- versus anti-smoking-first block order conditions (i.e. because otherwise, owing to smokers' characteristic tendency to evaluating in a pro- rather than anti-smoking manner, IAT block order should in principle have been able to differentially prime different IAT effects). In other words, crucially, the IAT from Study 1 appeared to confound implicit evaluating across contexts. And moreover, as already reviewed, all of the corresponding IRAP trial-type effects exhibited block order effects that closely conformed with smokers' and non-smokers' respective known-group behavioural histories in relation to smoking. On balance, therefore, the distribution-focused analytics we developed as part of Study 1 showed promise as a first means of estimating just how confounded (or not) a given implicit measure is with respect to implicit evaluating. Indeed, at the very least, these new analytics proved useful in confirming that the IRAP has greater potential than the IAT as a means of systematically guiding research toward a more differentiated understanding of tobacco addiction.

Another potentially important methodological insight provided by the current research was its revelation and indeed close replication of the fact that the IRAP practise phase appeared to serve as a buffer with respect to fleeting malleability effects (i.e. those lasting less than approximately 4-5 minutes). For example, during studies 3 and 4 we observed that thought suppression effects were often not apparent across the IRAP test phase when participants began suppressing just before they attempted to master the response latency and accuracy criteria for the relevant IRAPs (i.e. during their respective practise phases). Similarly, Study 5 revealed that the largest impact of acute stress induction during nicotine abstinence was during the criterion practise blocks of the relevant IRAP and diminished quickly thereafter across its test phase.

Crucially, this research was the first to incorporate an analysis of trial-type effects derived from the criterion practise blocks of an IRAP, and as such it has potentially important implications for future IRAP research. In particular, without a block-pair analysis of IRAP data that begins during the relevant IRAP's criterion practise blocks, then any such research is unlikely to accurately measure the onset and/or persistence of relatively fleeting experimental variables upon implicit evaluating. Indeed, by implication, one should require participants to master a given IRAP's response latency and accuracy criteria before using it to examine the progression of experimental effects that are likely to have a relatively momentary onset and/or offset. Otherwise, the relevant malleability effects may have passed before participants have reached the earliest point at which IRAP can measure such effects – namely, its criterion practise blocks.[87] However, by the same token, if one's aim is to capture relatively stable patterns of implicit evaluating irrespective of fleeting background variables, then conversely, it is perhaps best to restrict one's analysis to IRAP trial-type effects computed across the IRAP test phase (e.g. as we did for the first two IRAPs presented in Study 5).

In addition, on a somewhat related point, as part of studies 3 and 4 we also introduced a more precise means of determining the processes by which experimental treatments impacted one's IRAP trial-type effects (see Appendices 20-22; for a related approach to IAT data analysis see Webb, Sheeran, & Pepper, 2012). For example, in both studies 3 and 4 we revealed that smokers' attempts at deliberately suppressing their tobacco cravings were most likely to (temporarily) reduce their propensity for pro-smoking trial-type effects not by facilitating the anti-smoking/pro-abstinence responding recorded during anti-smoking blocks of the IRAP – but rather, by interfering with corresponding responses from pro-smoking IRAP trial blocks. As such, crucially, these latency-based supplementary analyses provided us with the means to directly confirm that smokers' attempts to suppress their cravings did not operate by affecting anti-smoking/pro-abstinence evaluating per se, but rather only by temporarily interfering and/or facilitating pro-smoking implicit responses. And this further confirmed our primary findings that thought suppression appeared to be an aspect of tobacco addiction, rather than an external process capable of ameliorating it.

Furthermore, perhaps one of the most important methodological issues raised by the current thesis was the idea that IRAP trial-type effects are prone to contextually

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[87] Note that this assumes that the relevant malleability effects are large enough and/or persistent enough to be registered across the approximately two-minute period it usually takes participants complete a pair of IRAP trial blocks. Nonetheless, alternatively, if one suspects that one is dealing with more fleeting effects they do have the option to employ an IRAP implementing less versions of each trial-type – thus resulting in IRAP trial blocks having fewer trials.

cued changes in the overarching perspectives (i.e. relational networks) within which they are embedded. As such, our opening chapters may have established that the IRAP is the only measure of implicit evaluating currently able to measure implicit evaluating of one topic as distinct from another. However, our systematic analyses of IRAP block order effects, and of thought suppression effects also made it clear that IRAP trial-type effects are nonetheless prone to confounding by variables external to the IRAP that cue changes in the overarching relational networks with which participants respond to those trial-types. And therefore, we strongly recommend that researchers conducting IRAP research take as much care as practicable to coordinate an IRAP's four trial-type topics in line with whatever overarching evaluative perspective is of interest; and moreover to control any variables that might cue different relational networks about the trial-type topics they are interested. Perhaps nowhere is this recommendation more pertinent, than when measuring implicit evaluating of topics about which participants are theoretically likely to exhibit various, context-dependent competing patterns of (implicit) evaluating in relation to given trial-type topic – as with addiction.

Of course, previous IRAP research has repeatedly demonstrated that even relatively established IRAP effects are prone to variability depending upon context (e.g. Cullen et al., 2008) – crucially, however, the current programme of research was the first to directly and experimentally link any such changes to the underlying issue of the stability of the relational networks within which a given trial-type effect is embedded. In fact, instead of recognising IRAP malleability effects as reflecting changes in the relational networks being assessed, the author of one recent set of IRAP studies actually dismissed such effects as mere error variance without considering the possibility that participants' evaluative perspectives might vary by context (O'Shea, Watson, & Brown, 2015). Thus, the current research offers a range of potentially important implications for the IRAP literature (indeed for indirectly related work in the evaluative conditioning literature, see Dijkstra & Menninga, 2015; Zanon, De Houwer, & Gast, 2011; Zanon, De Houwer, Gast, & Smith, 2014).

Finally, another important point related to the stability of one's overarching evaluative perspective during an IRAP, is in regard to the long-acknowledged possibility that individuals might be able to strategically induce temporary changes in the perspectives from which they implicitly evaluated as a means of faking measures of implicit evaluating (e.g. De Houwer, 2006, pp. 17-18; Gawronski, LeBel, & Peters, 2007, pp. 185-186). In particular, some existing findings do suggest that IRAP effects may be particularly prone to deception by strategic shifts in perspective switching when they address relatively unfamiliar topics (e.g. De Houwer, Beckers, & Moors, 2007; Hughes & Barnes-Holmes, 2011; Peters & Gawronski, 2011; Smyth et al., 2008; Zanon et al., 2014). Granted, the current thought suppression research, along with related research from other domains, suggests that deliberate perspective switching is much less feasible, or indeed concealable, as a means of faking an IRAP when one is characteristically accustomed to evaluating its trial-types in one way rather than the other – even when one is ambivalent about that topic (e.g. see Barnes-Holmes, Murphy, et al., 2011; Field et al., 2009; McKenna et al., 2007; Röhner, Schröder-Abé, & Schütz. 2011; Stacy & Wiers, 2010, p. 566-567; Wiers, Houben et al., 2010, p. 480; Wiers, Gladwin, et al., 2013, pp. 196-200; Van Dessel, De Houwer, Gast et al., 2015; Van Dessel, De Houwer, Roets, et al., 2015; Webb et al., 2012). However, owing the relative lack of process-based research on the issue (see Gawronski, LeBel, & Peters, 2007, pp. 185-186; Sayers & Sayers, 2013; Wiers, Gladwin, et al., 2013, pp. 196-200), on balance, it would be prudent for IRAP researchers to be vigilant against any variables that might cue extraneous perspective switching motivated by social desirability and/or other demand characteristics.

8.5. Main Implications of the Current Work for Future Research on Tobacco Addiction

The current programme of research consistently indicated that suppression-oriented strategies which seek to eliminate tobacco cravings are unlikely to succeed. In particular, we found that suppression-oriented methods of nicotine abstinence are self-defeating because they tend to provoke the very tobacco cravings they seek to eliminate. This is important from the point of view that the vast majority of the research that has so far examined how best to modify addiction-related (implicit) evaluating has focused upon doing so using suppression-oriented tactics. In fact, most cognitive theories view (tobacco) addiction as being primarily an issue of not being able to sustain pro-abstinence/anti-addiction rumination extensively enough to permanently contradict, or thus prevent pro-smoking/anti-abstinence implicit evaluating from interjecting to motivate addictive behaviour (see concepts such as *ego/cognitive depletion*, *cognitive efficiency*, and *dual processes* of cognitive control; Cox, Klinger, & Fadardi, 2015; Baumeister & Vonasch, 2015; Friese et al., 2011, p. 337; Hoffman, Vohs & Baumeister, 2012; Wiers, Houben et al., 2010; Spada et al., 2015; Stacy & Wiers, 2010; Wiers, Gladwin et al., 2013; Tiffany, 1990, 2008). In other words, most research in the area has to date assumed that one must first eliminate implicit evaluating related to tobacco cravings if one is to remain abstinent. Crucially, however, as we will now briefly review, most of this literature is in disarray owing to a variety of experimental confounds which the current research sought to begin to resolve (e.g. see Cox et al., 2015; Sayers & Sayette, 2013; Wiers, Gladwin, et al., 2013).

For example, despite the fact that cognitive training has not yet been tested as a means of modifying smoking-related IAT effects, it is generally the most popularly tested method of modifying (i.e. as opposed to cueing) implicit evaluating both in relation to other measures of smoking-related implicit evaluation (see Chapters 1-3; Note 7, Appendix 1); and in relation to other IAT-domains more broadly (see Roefs et al., 2011; Rooke et al., 2008, p. 1324; Smith et al., 2013, p. 194; Wiers, Gladwin, et al., 2013; Wiers, Houben et al., 2010; Wiers & Stacy, 2006a). However, rather than instructing individuals to deliberatively contradict problematic instances of (implicit) evaluating as narrative-based techniques typically do, cognitive training techniques generally focus upon training individuals to automatically sustain novel implicit evaluating perspectives designed to (implicitly) repress pre-established pro-addiction/anti-abstinence implicit evaluating (Moss et al., 2013; Stacy & Wiers, 2010; Wiers, Gladwin, et al., 2013; Wiers, Houben et al., 2010). Crucially, however, such methods have been relatively unsuccessful in reducing the occurrence of established aspects of addiction-related implicit evaluating, much less the problem behaviours they were supposed to motivate.

Despite much early promise, based upon the ability of the relevant training methods to temporarily reduce pro-addiction implicit evaluating on various implicit measures, such effects have often failed to replicate and/or to generalize (see Bowley et al., 2013; Kerst & Waters, 2014; Vandenbosch & De Houwer, 2011; Woud et al., 2013). Moreover, on the relatively few occasions where cognitive training appeared to be effective in modifying established implicit evaluating and/or criterion behaviours, it was usually integrated within a wider package of narrative-based treatment-as-usual (see Note 7, Appendix 1; Eberl, Wiers, Pawelczack, Rinck, Becker, & Linden, 2013; Wiers, Houben et al., 2010, p. 480; Wiers, Gladwin, et al., 2013, p. 196-200). And by contrast, cognitive training has usually failed to generalize in its effects when used in isolation (i.e. except trivially to implicit measures that employed similar procedures to those implemented by the relevant cognitive training; for reviews see Note 7, Appendix 1; Stacy & Wiers, 2010, p. 566-567; Wiers, Houben et al., 2010, p. 480; Wiers, Gladwin, et al., 2013, pp. 196-200). Overall, this suggests that the impact of cognitive training is very much a function of the (implicit) evaluative perspectives that participants engage in relating to this training.

Indeed, during recent years, a small number of researchers have repeatedly demonstrated that participants respond differently to cognitive training depending upon how they relate to and/or are instructed to relate to it (i.e. as per concepts such as *contingency awareness*, *cognitive construal* and *implementation intentions*; see Cesario, 2014; De Houwer, 2014; Hoffman et al., 2010; Hogarth et al., 2006; Hogarth & Duka, 2006; Loersch & Payne, 2011; Pleyers et al., 2007; Smyth et al., 2008; Van Dessel et al., 2014, 2015; Webb et al., 2012). Thus, given that the manner in which participants relate to cognitive training has generally not been experimentally controlled in testing, there is currently only a relatively rudimentary technical understanding of how cognitive training operates with respect to particular types of the implicit evaluating or the criterion behaviours they presumably motivate (see Vandenbosch & De Houwer, 2011). And as such, crucially, the literature remains highly equivocal about whether or not cognitive training is feasible as a means of eliminating (tobacco) cravings, and thus addiction.

Indeed, the literature on contradiction-based narrative techniques of modifying implicit evaluating suffers from similar, if not worse ambiguities as that on contradiction-based cognitive training techniques. Although narrative-based methods have on occasion ameliorated implicit evaluating supporting other types of addictions, such changes have rarely generalized to changes in the relevant addictive behaviours in question (e.g. Thush, Wiers, Moerbeek, Ames, Grenard, Sussman, & Stacy, 2009; for reviews see Roefs et al., 2011, pp. 167-184; Rooke et al., 2008, p. 1324). Rather, in such cases (as in the smoking-related literature), the relevant narrative techniques were usually an unidentified amalgamation of various different treatment strategies in addition to those instructing addicted individuals to deliberatively dispute and thus suppress any cravings they might experience. Indeed, more broadly again, researchers have only just begun to decompose how different types of instructions impact established implicit evaluating in any domain, much less in relation to (tobacco) addiction specifically (see Smith, De Houwer, & Nosek, 2013; Smith & De Houwer, 2015). Crucially, without experimentally distinguishing among different types of narrative techniques, it will not be possible to systematically determine which (combinations of) techniques are most effective in changing particular types of (established) implicit evaluating (e.g. see Kazdin, 2007; Gifford & Humphreys, 2007; Peterson et al., 2011, pp. 49-57; Roefs et al., 2011; Rooke et al., 2008, p. 1324; Wiers, Houben et al., 2010, p. 481).

Overall, therefore, by experimentally isolating the most common suppression-oriented strategies used by smokers during smoking-cessation, the current research made an important contribution to the nascent literature on how one might modify smoking-related implicit evaluating. In particular, the current findings implied that rather than modifying smoking-related implicit evaluating per se, deliberate thought suppression merely cues temporary variability in it – and moreover, ultimately, it typically does so in a counterproductive fashion (see De Houwer, Barnes-Holmes & Moors, 2013). Thus, crucially, future research should pursue alternatives to the adversarial, suppression-oriented strategies for managing tobacco cravings which currently lie at the heart of many smoking-cessation (and -prevention) treatments. Indeed, there may even be strong ethical reasons for pursuing alternatives to suppression-oriented self-control strategies (see Kjærsgaard, 2015). For example, apart from confirming that deliberate suppression of one's tobacco cravings is counterproductive, the current research consistently found that tobacco addiction is implicitly motivated by perceptions that smoking provides relief from craving-related negative affect, and/or increases rewarding feelings. And as such, insofar as suppression-oriented smoking-cessation tactics predicate smoking-cessation on eliminating tobacco cravings from one's experiences they are also likely to tacitly compound the very implicit motivational processes which govern tobacco addiction in the first place.

Certainly, there is now a large questionnaire-based research literature suggesting that rather than suppressing and thus stigmatizing cravings as a means of quitting smoking, one should learn to deliberately accommodate and thus *accept* those cravings in terms of one's most overarching reasons for wanting to stop smoking (see Wegner, 2011; see also Brandt et al., 2015; Brown et al., 2005; Cameron et al., 2013; Erlblich & Montgomery, 2012; Erskine et al., 2010, 2015; Farris et al., 2015; Farris, Zvolensky, & Schmidt, 2015; Forman et al., 2007; Hayes, Strosahl & Wilson, 2011, p. 82; Gifford & Humphreys, 2007, p. 359; Lee et al., 2015; Levin, Luoma, & Haeger, 2015; McCallion & Zvolensky, 2015; Moss et al., 2015; Spada et al., 2015; Stewart, Barnes-Holmes, Barnes-Holmes, Bond, & Hayes, 2006, p. 81; Toll et al., 2001; Wiers, Gladwin, et al., 2013). Indeed, by confirming that tobacco addiction is differentially motivated by reward- and relief-focused implicit evaluating depending upon mood-context, the current research was in essence the first to confirm that the so-called experiential avoidance processes typically targeted by acceptance-based approaches operate implicitly (e.g. see Aldao & Nolen-Hoeksema, 2012; Cameron et al., 2013; McCallion

& Zvolensky, 2015; Schloss & Haaga, 2011). Overall, therefore, future research on smoking-cessation should prioritize examining the potential of acceptance-based techniques for motivationally decoupling one's smoking from any experientially-avoidant implicit evaluating driving it addictively. However, before doing so we recommend that like Carpenter et al. (2012), researchers first establish how well baseline IRAP trial-type scores targeting smoking-related psychological avoidance are able to prospectively predict relapse during smoking cessation – in particular, doing so would provide preliminary causal evidence as to whether the research involving resource-intensive acceptance-based treatments is warranted.

8.6. Final Conclusions

Overall, the current programme of research consistently supported the IRAP as a useful means of quantifying tobacco addiction in terms of implicit evaluating. In particular, by revealing motivational distinctions that were simply not available using other existing measures of implicit cognition, the IRAP's experimental precision allowed us to identify aspects of implicit evaluating with correspondingly unprecedented levels of criterion validity in relation to tobacco addiction. In particular, our findings confirmed that the more precisely one coordinates the phrasing of IRAP trial-type topics with well known conditional aspects of relevant criterion variables (e.g. as per the heuristic descriptions available from prevailing cognitive theory) the more one is likely to obtain criterion validity with any resulting trial-type effects. Thus, together our findings provided a preliminary functional model of tobacco addiction wherein implicit evaluating motivates smoking in complex, coordinated and mood-dependent networks which collectively insist that one should regulate one's ongoing emotional experiences – particularly negative craving-related affect – by smoking. Moreover, in a related vein, the current research also provided an experimental analysis of smokers' most popular, and also least successful, method of managing their tobacco cravings. Not only did this research systematically confirm the self-defeating nature of the suppression-oriented tactics that smokers (and therapists) typically use as a means of smoking-cessation. Moreover, it implicated such dynamics as being an important facet of tobacco addiction itself, and thus recommended research on acceptance-based tactics as an alternative means of motivating smoking-cessation. Indeed, perhaps most importantly, regardless of how one might best treat it, the current research was successful in quantifying tobacco addiction in such a way as to begin clarifying the motivational problem(s) that its treatments must ultimately tackle – and crucially, without the IRAP this type of process-based analysis of implicit evaluating would simply not have been possible.

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LIST OF APPENDICES

APPENDIX 1

Collateral Controversies resulting from the Topic-indeterminacy of most (Addiction-related) Measures of Implicit Cognition

Note 1

Although most evaluative priming procedures instruct participants to respond to targets as quickly and as accurately as possible, this of course does not oblige participants to do so. Likewise, although most researchers exclude extremely slow evaluations when scoring evaluative priming effects (i.e. target responses that had statistically outlying latencies within the relevant dataset; see Wentura & Degner, 2010, pp. 109-110) this only excludes evaluations that may have involved extreme amounts of deliberation. Thus, in practice evaluative priming effects still include many target responses that are slow enough to have involved the complexities of deliberation (i.e. a recent meta-analysis indicated mean latencies ranging from 798-2709 *ms* with standard deviations ranging from 908-2281 *ms*; see Cameron et al., 2012, pp. 342-343). Granted, a very small minority of evaluative priming studies have attempted to *actively* minimize participants' ability to deliberate while responding to trial targets. However all did so in such an extreme manner that they obscured the relevant evaluative processes even more than conventional evaluative priming effects. Specifically, the time given participants to respond to the targets was designed to become adaptively briefer and briefer (e.g. 300-500 ms) until it heavily interfered with each participant's ability to 'correctly' evaluate targets as positive versus negative (i.e. the researcher judged correctness in terms of his/her opinion about people's normal evaluations of the relevant target). The basic assumption here is that very brief response deadlines will cause evaluative priming effects to emerge primarily from automatic cognitive processes in terms of response 'accuracy' (i.e. at very brief response latencies participants should be more versus less able to 'correctly' classify targets depending respectively upon whether the preceding prime prompts them to respond in ways that agree versus disagree with the relevant 'correct' evaluation; see Wentura & Degner, 2010, pp. 103, 107-108). However, the key problem in setting out to interfere with the participant's ability to evaluate prime and/or target stimuli as instructed is that it is tantamount to encouraging evaluating in other, unspecified terms when responding to the relevant trial (as is tacitly acknowledged by the common practice of excluding extremely brief evaluations when scoring priming effects; see Wentura & Degner, 2010, pp. 109-110). Thus, as compared to conventional evaluative priming effects that allow participants enough time to evaluate trial stimuli according to the researcher's expectations of normal, by definition priming effects based upon decrements in response 'accuracy' make it less clear to the researcher what cognitions they involve (i.e. because these decrements indicate that participants have evaluated the relevant targets with unusual connotations that the researcher neither anticipated nor recorded).

Note 2

Admittedly, most evaluative priming procedures do make some attempt to control for extraneous influences when scoring latency-based evaluative priming effects. Specifically, researchers typically exclude any target evaluation post hoc that contradicts the researcher's beliefs about how that target is normally evaluated by most people (e.g. if a participant evaluated the target 'fabulous' negatively that self-report would usually be excluded from scoring; see Wentura & Degner, 2010, pp. 109-110; Wittenbrink, 2007, p. 29). However, aside from the fact that these exclusion criteria are not possible with one major variant of evaluative priming (i.e. the AMP; see Payne et al., 2007, p. 404), even when they are possible they are not a solution to the original problem of cognitive indeterminacy but rather merely a compromise. Such practices not only tacitly acknowledge that participants might perceive their task in many different but unrecorded ways (i.e. even when they appear to evaluate targets as 'normal' and are therefore not excluded), but they also diminish the size of each priming effect's dataset and therefore, in principle, that effect's reliability (see Wentura & Degner, 2010, pp. 106, 111; Wittenbrink, 2007, p. 29).

Note 3

Although Degner (2009) prevented participants from strategically distorting evaluative priming effects, this was achieved only by using response deadlines that were specifically designed to interfere with participants' ability to evaluate target stimuli consistently. Thus, Degner (2009) exchanged one methodological ambiguity for another (i.e. as per Notes 1 and 2, Appendix 1, above). Moreover, Teige-Mocigemba and Klauer (2013) recently demonstrated that participants can strategically distort their evaluative priming effects even under similar response deadline conditions as those that Degner (2009) claimed prevented this. Likewise, although there is some evidence that procedures designed to mask participants' awareness of primes can reduce participants' ability to strategically control evaluative priming effects, in such cases the resulting priming effects were smaller and/or more unstable than unmasked evaluative priming effects (see Van den Bussche et al, 2009; Wentura & Degner, 2010, pp. 99-100). This is unsurprising given that evaluative priming was originally inspired by theoretical traditions that are generally unconcerned with participants' awareness of stimuli and much more interested in intentionality, efficiency and/or speed as facets of implicitness (see Payne & Gawronski, 2010, pp. 2-10).

Note 4

One recent variant of evaluative priming, the AMP, has been widely promoted in terms of its exemplary internal reliability (e.g. $.70 \leq \alpha s \leq .90$ Payne et al., 2005; Wentura & Degner, 2010). However, when considered across a broad range of psychometric criteria AMP scores perform rather disappointingly (see Bar-Anan & Nosek, 2014b). Although the AMP improves upon other evaluative priming methods in that participants tend to respond more consistently across its trials, nevertheless its tasks cannot identify the particular evaluating they induce for each participant (see Bar-Anan & Nosek, 2014a; Deutsch & Gawronski, 2009; Loersch & Payne, 2011; Oikawa, Aarts, & Oikawa, 2011; Ruys, Aarts, Papies, Oikawa & Oikawa, 2012; Spruyt et al., 2014). In other words, even if AMP scores are internally consistent the specific evaluating they measure remains undetermined and thus free to vary in an unrecorded, confounded fashion from participant to participant and from occasion to occasion.

Furthermore, AMP scores are very susceptible to strategic distortion (see Bar-Anan & Nosek, 2012, 2014b; Blaison et al., 2012; De Houwer & Smith, 2013). In response to such findings, some authors have attempted to defend the implicitness of AMP scores by claiming that so-called strategic distortions of AMP scores operate indirectly via non-deliberative, implicit processes; however, none have yet tested the relevant processes directly (see Gawronski & Ye, 2014; Payne, Brown-Iannuzzi, Burkley, Arbuckle, et al., 2013; Payne et al., 2005; Payne, Govorun & Arbuckle, 2008). In fact, to the contrary, the latest evidence suggests that participants must be consciously aware of prime stimuli in order for AMP effects to emerge (i.e. AMP effects usually only emerge when participants correctly detected and/or recognized its primes as being relevant to their completion of AMP trials; see Cameron et al., 2011, pp. 341-344; Gast, De Houwer & De Schryver, 2012; Oikawa et al., 2011; Rohr, Degner & Wentura, 2015, pp. 13-15; Ruys et al., 2012).

In any case, whether or not the AMP is deemed implicit, the key point is that it does not determine the specific type(s) of evaluating involved. Indeed, even the reliability of all but the strongest of AMP scores is questionable as per the recent finding that AMP scores rapidly lose internal reliability as their magnitude reduces (see Bar-Anan & Nosek, 2014b; De Houwer & Smith, 2013, pp. 300-301). This finding makes sense from the point of view that the misattributions upon which AMP scores are based are likely to become more unstable whenever AMP tasks lose relevance for participants (see Notes 1 and 2 above about how ambiguous tasks promote unstable evaluating; also see Bar-Anan & Nosek, 2014b; Loersch & Payne, 2011; Oikawa et al., 2011; Ruys et al., 2012). Crucially, whatever the reason for the diminishing reliability of AMP scores' as they become weaker, it means that such scores are bound to be limited in their ability to measure individual differences. And indeed, this is confirmed in practice by the limited ability of AMP scores to detect many known-groups type behavioral criteria despite initial claims that AMP scores achieved average criterion effects of $r = .53$ (see Payne et al., 2005, p. 290; Payne, Govorun, & Arbuckle, 2008).

For example, Bar-Anan and Nosek's (2014b) large scale comparison of implicit measures involving 23,413 participants revealed that AMP scores bore erratic and relatively weak relationships with corresponding validity criteria across the three domains tested. Specifically, AMP scores correlated with validity criteria in the range $-.13 \leq r \leq .37$, with the respective weighted average criterion correlations for race, political orientation and self-esteem being $r = .09$, $r = .36$, and $r = .07$ (i.e. we calculated these and the following r effects by first converting all relevant criterion statistical effects in terms of r before then using degrees of freedom to weight the relevant averages; see Rosnow, Rosenthal & Rubin, 2000). Indeed, these criterion correlations diminished even more when disregarding extreme AMP scores and/or when AMP scores were derived from one topic at a time (i.e. an average $r = .09$ across the three domains tested between the middle 90% of AMP scores and corresponding criterion variables; and an average $r = .11$ between single category AMP scores and corresponding criterion variables; see Bar-Anan and Nosek, 2014b; see also Bar-Anan & Nosek, 2012, 2014a; Cameron et al., 2012, p. 342; Herring et al., 2013, p. 1074; Rooke et al., 2008).

Note 5

Just as precise measures of implicit evaluating are prerequisite to examine how contextual cues moderate evaluating, a very similar point applies to the core methods employed by the research literature examining the neurobiology and/or psychopharmacology of addiction (see Berridge & Robinson, 2006; Everitt & Robins, 2005; Greenberg & Altman, 1976; Koob & Le Moal, 2008; Perkins et al, 1997; Robinson & Berridge, 1993, 2003, 2008). The various cued conditioning techniques (i.e. operant, respondent, habituation, etc.) that form the backbone of this literature are generally used to measure how motivating a given object of addiction is in terms of how effectively it can condition overt behaviours to occur more or less often with particular contextual cues (i.e. in contrast to the cue-reactivity paradigm which measures how pre-existing behavioural cues function). The key problem with such research is that it cannot determine or therefore measure how the behavioural contingencies it sets up are being motivationally transformed on an ongoing basis by evaluating (see Barnes-

Holmes et al., 2010; Berridge & Robinson, 2006, 2011; Field & Duka, 2001; Hayes & Brownstein, 1986; Heyman, 2011, 2013; Moore, 2013; Perkins, 2009; Perkins et al., 1997; Rose et al., 2013; Skinner, 1945, 1989; Steele & Hayes, 1991). Therefore, to the extent that researchers wish to determine how neurobiological and/or pharmacological variables interact with motivational processes in humans capable of evaluating, they will need to incorporate precise measures of implicit evaluating within their analyses. Indeed, this point has important implications for psychological research in general. To understand how contextual variables influence the motivational processes governing the behaviour of people capable of evaluating it follows that we must account precisely for implicit evaluating. Otherwise, researchers will lack the practical ability to replicate any particular neurobiological and/or psychopharmacological findings about the impact of context upon motivational processes (for a review of how this has already occurred in the literature on social- and goal-priming see Cesario, 2014; Kahneman, 2012; Loersch & Payne, 2011; Yong, 2012).

Note 6

The creators of the unipolar ST-IAT and the FAST each argued from the outset that they had effectively achieved a non-relative measure by targeting implicit associations between an attitude object and some attribute *relative to* neutrality for both associates (i.e. neutrality as the intersection of *both* the scale for the attitude object and for the attribute of interest). And indeed, such arguments have some face validity insofar as comparing a category with its notional zero point constitutes a non-relative, albeit hypothetical, scale by definition. Crucially, however, it is highly questionable whether the ‘neutral’ stimuli employed by either measure actually function as such for either the attitude object and attribute of interest.

The unipolar ST-IAT and the FAST (and incidentally, the unipolar IAT and the Brief IAT) are all designed to make relative comparisons with stimuli that were chosen to be universally neutral, in the sense of irrelevant, to the topic and attribute category of interest. Crucially, the unipolar ST-IAT and the FAST (and indeed the unipolar IAT and the Brief IAT) are specifically designed to incorporate particular neutral stimuli on the basis of irrelevance to the attitude object and attribute of interest. Thus, rather than operationalising neutrality in terms of stimuli that are neutral with respect to the relevant attitude object *and* attribute, all such measures make the tacit compromise of equating irrelevancy with neutrality. Given that it is a contradiction in terms to speak of comparing things that are irrelevant to each other, therefore, by inadvertent design, such measures make comparisons that are an even less interpretable than a standard IAT effect.

To illustrate, the unipolar ST-IAT (Thush & Wiers, 2007) was designed as a hybrid of the unipolar IAT and the ST-IAT. Admittedly, this hybrid does avoid comparisons between an attitude object of interest and some contrasting topic. Specifically, the only difference between a unipolar ST-IAT and an ST-IAT is that the former replaces one of the latter’s attribute categories with a group of stimuli that were each chosen to be universally neutral, in the sense of irrelevant, to its accompanying IAT stimulus categories. Thus, for example, Thush and Wiers (2007) employed words such as flat, daily, usual, wide and steep as ‘neutral’ stimuli when attempting to measure implicit associations between alcohol-related words and positive attribute words such as likeable or pleasant. The key point here is that there are many unintended and unrecorded ways in which participants might have associated alcohol-related words with the ‘neutral’ words employed by Thush and Wiers (2007). Participants might have associated the supposedly neutral word ‘flat’ in negative terms with one of the beers named among the alcohol-related stimuli, but in contrast have also associated the supposedly neutral word ‘daily’ in positive terms with alcohol (e.g. that daily alcohol consumption is benign and/or customary).

Taking a somewhat different approach to achieving comparisons with neutrality, the FAST uses nonsense words as neutral comparison stimuli. In broad terms, the FAST compares how quickly participants learn to classify a target attitude object’s exemplars with a target attribute’s exemplars together, versus with nonsense stimuli, while under time pressure (i.e. < 3000ms per trial). The basic idea here is that participants should require less learning trials to correctly match exemplars from an attitude object with exemplars from attribute and neutral categories (and vice-versa) to the extent that participants have a behavioural history of similarly relating the attitude object and attribute categories together. However, like the unipolar ST-IAT the core, albeit tacit, assumption here is that participants will always respond neutrally to nonsense stimuli because they are fundamentally irrelevant to all topics and attributes.

Therefore, the FAST is tacitly based on the untenable dual assumption that participants will only ever respond to trials involving neutral/nonsense stimuli in terms of (a) the categories about which those stimuli were specifically designed to be irrelevant (i.e. this is needed to achieve a scale in terms of the attitude object and attribute of interest), *and* yet (b) without associating those neutral/nonsense stimuli in any way with those categories (i.e. this is needed to achieve a true zero point on the relevant scale object-attribute associative scale). In other words, it seems very contradictory to expect a participant to respond to neutral/nonsense stimuli in terms of an attitude object and attribute of interest during tasks that specifically legislate against this (e.g. as in a unipolar IAT when neutral stimuli must be classified as distinct from the attribute of interest, or as when nonsense stimuli must be paired with nonsense stimuli during a FAST). By incorporating comparisons with stimuli that are designed to be irrelevant to the topic of interest, therefore as we have already argued with respect to unipolar (ST-)IATs, the FAST is a relative measure of implicit evaluating and moreover it is one that is even less interpretable than the original format IAT (i.e. especially given that when faced with ambigious stimuli people usually respond to those stimuli in ideographic terms; cf. *apophonia*; see also Bordieri, Kellum, & Wilson, 2013; Bordieri, Kellum, Wilson, Whiteman, in press).

Incidentally, similar criticisms also apply to the Brief IAT. The Brief IAT instructs participants to classify contrasting attribute stimuli by ignoring their category label, and instead responding relative to the category label for the attribute of interest. Thus, in addition to measuring one attitude object relative to another, Brief IATs also compare responses to some attribute category of interest with responses to an irrelevant attribute category (i.e. in order to achieve a neutral comparison for the implicit evaluative association of interest; see Sririam & Greenwald, 2009; Teige-Mocigemba et al., 2010, p. 134). Therefore, much like the unipolar IAT, the unipolar ST-IAT, and the FAST, the Brief IAT also fails to disambiguate one attribute category from another despite its aspirations to the contrary.

Granted, there is an IAT variant that can disambiguate one attribute category from another without resorting to comparisons with irrelevant stimuli. However, that variant, the so-called single attribute IAT, has only been used once in the literature (Penke, Eichsteadt, & Asendorpf, 2006), and it is still a relative measure with respect to attitude objects (i.e. it is designed to measure the implicit association between a given attitude object and a given attribute relative to some contrasting attitude object). Overall, therefore, neither the original format IAT nor any of its variants succeed in providing non-relative measures of implicit evaluating.

Note 7

Wittekind et al. (2015) recently reported evidence that a zAAT designed to train avoidance of smoking-related cues (and approach to smoking-neutral cues) brought about significant reductions in a number of indicators for tobacco addiction four weeks later. However, Wittekind et al. did not implement a measurement version of the relevant zAAT to test whether the relevant response biases had been successfully trained, and thus it is speculative whether or not any such changes brought about changes in tobacco addiction at follow-up. Moreover, even ignoring this issue, smokers receiving a ‘slight variant’ of the relevant zAAT training unexpectedly exhibited no such changes in tobacco addiction at follow-up. This raises important questions about whether the relevant changes in smoking-related behaviour, which were relatively minor, were merely an instance of regression to the mean among the heavy smokers included in this study. On average the relevant smokers who received the standard zAAT training had been smoking for 22-25 years, smoking an average of 20 CPD at the outset of the study and reported reductions in their cigarette consumption that was of a similarly small magnitude as the accompanying reductions in CPD observed among those receiving the variant zAAT or those in the control group (i.e. respective average reductions of 1.85, 0.75 and 0.40 CPD from an average of 20 CPD).

Granted, two studies that are commonly cited as having treated alcohol addiction using AATs did employ alcohol-related zAATs as measures (i.e. Wiers et al., 2011; Wiers, Rinck, Kordts, Houben, & Strack, 2010). And yet, neither study succeeded in demonstrating that the small changes in alcohol-related zAAT scores they obtained mediated the relatively minor changes in alcohol-related behaviour they also obtained (i.e. despite third party claims to the contrary; e.g. Earp et al., 2013, p. 2159; Watson et al., 2012, p. 6-7; Woud, Becker & Rinck, 2011, pp. 1331-1332). Indeed, even putting aside the question of whether zAAT training impacts addiction-related behaviour, follow-up research now suggests that any such changes in behaviour might not occur via changes in implicit evaluating but rather as a deliberative artefact of the zAAT instructions involved (see Vandenbosch and De Houwer, 2011; Van Dessel, De Houwer, Gast, & Smith, 2014; Van Dessel, De Houwer, Roets, & Gast, 2015; Wiers, Gladwin, et al., 2013).

APPENDIX 2

The Demographic and Behavioural History Questionnaire (DBHQ)

Participant Code: __________

Brief Questionnaire

Please try to answer the following questions as ***accurately*** as possible:

1. What is your age and sex?

2. Briefly, what is your current occupation and current highest level of education?

3. Have you ever lived in a home where people smoked indoors? Give brief details (e.g., did your parents smoke?).

4. Do you or have you ever smoked on a daily basis? If not have you ever smoked a cigarette? Please give details of your smoking history as appropriate (e.g., social smoker? how long has it been since you smoked on a daily basis? Etc…)

5. Indicate as accurately as possible how long it has been since you first began smoking cigarettes on a daily basis? Also, for approximately how much of that time did you refrain from smoking.

6. During the PAST 30 DAYS on how many days did you smoke a cigarette?

0-10 days   10-20 days   20-30 days

30 days

7. *Exactly* how many cigarettes do you smoke per day?

8. How long can you go without smoking before you first feel like you need a cigarette?

Less than an hour   1-3 hours   More than 3 hours, but less than a day

A full day   1-6 days   A week or more   Forever

7. Do you think you will smoke a cigarette when this study is finished?

Definitely not ______ Possibly ______ Probably ______ Definitely

8. Are you or have you ever been addicted to alcohol or a regular user of illicit drugs (including cannabis)?

Yes ______ No

9. Do you want to completely stop smoking cigarettes?*

Yes ______ No

* If you answered yes to the final question, and would like to participate in a free workshop designed to help people quit smoking please ask the researcher for further details.

This study is now complete; please report to the researcher for debriefing.

Thank you very much for your participation.

APPENDIX 3

The D_{IAT}-algorithm

The D_{IAT}-algorithm was computed as follows: (i) latencies above 10,000 ms from the dataset were eliminated; (ii) all data for a participant were removed if he or she produced more than 10% of trials with latencies less than 300 ms; (iii) means were computed for trials in each of the four blocks, 3, 4, 6, and 7; (iv) one standard deviation was calculated for all trials in blocks 3 and 6, and another for blocks 4 and 7; (v) difference scores were computed between blocks 3 and 6, and between blocks 4 and 7, taking the pro-smoking from the anti-smoking blocks; (vi) each mean difference score was divided by its associated standard deviation; and (v) these two scores were added together and divided by two. Accordingly, the only difference between the D_{IAT} score and Cohen's *d* is that whereas D_{IAT} is computed using the *common* standard deviation computed from all of the trials across both treatment conditions being compared, *d* uses a *pooled* standard deviation (i.e. the square root of a weighted average, according to relative sample size, of the sample variances from each individual condition being compared).

APPENDIX 4

The D_{IRAP}-algorithm

The IRAP response latencies were transformed into D_{IRAP} scores using an adaptation of the D_{IAT}-algorithm called the D_{IRAP}-algorithm. The details of the D_{IRAP}-algorithm are as follows: (i) only response-latency data from test blocks are used (unlike the D_{IAT}-algorithm which incorporates IAT practise blocks 3 and 6); (ii) latencies above 10 000 *ms* are eliminated from the dataset; (iii) the data are eliminated for a participant for whom more than 10% of test-block trials have latencies less than 300 *ms*; (iv) twelve standard deviations are computed, one for the response latencies belonging to each trial-type in each of the three pairs of test blocks (*i.e.* four *common* standard deviations for the response-latencies from test blocks 1 and 2, four from the latencies from test-blocks 3 and 4, and a further four from test-blocks 5 and 6); (v) 24 mean response latencies are calculated, one for each of the four trial-types in each test block; (vi) 12 difference scores are computed for each trial-type in each pair of test blocks, by subtracting the mean latency of each trial-type's trials in a pro-smoking test-block from the mean latency of that trial-type's trials in its corresponding anti-smoking test-block; (vii) each difference score is then divided by its corresponding standard deviation from step iv, yielding 12 *block-pair* D_{IRAP} scores; one score for each trial-type for each pair of test blocks (viii) finally, four *overall trial-type* D_{IRAP} scores are computed by averaging each trial-type's three *block-pair* D_{IRAP}s from step vii.

APPENDIX 5

The Accuracy, Speed and Synchronicity Characteristics of the Trial Responses Underlying D_{IAT}s versus D_{IRAP}s

Although the average accuracy of participant responding on the IAT data trials was less than that on the IRAP test trials, $t(85) = 2.64$, $p = .01$, $\eta^2 = .08$, this difference likely lacks practical significance. In both cases the participants responded with an average accuracy of at least 80%: 91.6% of the IAT data trials were accurate, *standard deviation* (*SD*) = 6.4%, *inter-quartile range* (*IQR*) = 7.5%, and similarly, the average percentage accuracy for the IRAP test trials was 94%, *SD* = 3.9%, *IQR* = 4.67%.

In contrast, there was a substantial difference between the average speed with which participants completed the IAT test trials versus the IRAP test trials, $t(85) = 16.93$, $p < .0001$, such that on average, participants completed the IAT trials 1254 *ms* faster than the IRAP trials. Specifically, although participants completed IAT data trials with a mean latency of just 1134 *ms*, *SD* = 355 *ms*, *IQR* = 434 *ms*, on average participants took 2388 *ms* to complete each IRAP test trial, *SD* = 410 *ms*, *IQR* = 595 *ms*.

Crucially, however, the critical response property underlying the IAT effect, the relative (a) synchrony of pro- versus anti-smoking trials, did not differ from those underlying the *trial-type* D_{IRAP}s, $t(85) = .24$, $p = .81$, $\eta^2 = .0007$. The average pro- versus anti-smoking response latency difference underpinning each IAT effect was just 18 *ms*, *SD* = 424 *ms*, *IQR* = 530 *ms*, and similarly the average pro- versus anti-smoking response latency difference underpinning each trial-type effect from the IRAP was just 36 *ms*, *SD* = 237 *ms*, *IQR* = 182 *ms*.

APPENDIX 6

Major Rationales for Analysing IRAP data in terms of its Nested Stimulus Variables

All IRAP research to date has analysed the four *trial-type* D_{IRAP}s as a single four-level 'trial-type' variable. By minimizing the complexity among the four *trial-type* D_{IRAP}s to one 'trial-type' dimension, this approach maximises one's statistical power to detect any experimental main or interaction effects that affect all four *trial-type* D_{IRAP}s collectively in the same way. This is a legitimate strategy if we are convinced that the four *trial-type* D_{IRAP}s do not meaningfully differ from each other in a given analytic context (i.e. if we are convinced they will not interact with any of the other experimental variables involved). However, many IRAPs are specifically designed to permit drawing clear functional distinctions among all four trial-types and thus it seems likely, as in the present case, that the forgoing assumptions will often be untenable. Under such circumstances, if one is to avoid confounding an IRAP's four trial-types, one must at least begin by analysing all four *trial-types* D_{IRAP}s collectively in terms of the two nested IRAP variables which ultimately distinguish them (i.e. in the present study these variables are called *Concept Label* versus *Attribute Target Stimulus Class*; see Method section). To do otherwise, would not just neglect important information about how an IRAP's four *trial-type* D_{IRAP}s interact with each other, but also about how they interact with external experimental variables. For example, it would mean that we would not be able to statistically test whether smokers differed from non-smokers more on the smoking-related *trial-type* D_{IRAP}s versus on the smoking-ban-related *trial-type* D_{IRAP}s. Admittedly, one could compute all of the eight simple effect comparisons between smokers versus non-smokers on each of the four *trial-type* D_{IRAP}s, and then informally interpret trend(s) about which *trial-type* D_{IRAP}s were most sensitive to smoking-status. However, that is all it would be; a subjective interpretation vulnerable to illusions about how the two neglected trial-type variables interacted with each other. Therefore, to exclude subjective illusions about the many meaningful interactions that are possible among an IRAP's four *trial-type* D_{IRAP}s (i.e. they grow exponentially when we use the IRAP to examine an increasing number of experimental manipulations), it requires an omnibus statistical test incorporating smoking-status with the two 'internal' IRAP variables which distinguished those four trial-types from each other. Finally, given that in the present case the concept label variable defines the topic to which each attribute target stimulus class applies, we therefore nested the attribute target stimulus class variable within the concept label variable throughout all proceeding analyses.

APPENDIX 7

The Technical Rationale of Receiver Operating Characteristic (ROC) Curve Statistics

A *ROC curve* describes the dynamic trade-off between the true positive rate (i.e. *sensitivity*) versus the false positive rate (i.e. 1 – *specificity* = 1 – true negative rate) with which a given measure classifies cases into two diagnostic categories. Thus, the area bounded by the receiver operating characteristic curve, quantified in the *Area Under the Curve* (AUC) statistic, is a direct indicator of a measure's ability to dichotomise cases in the relevant way. When the *AUC* has a value of .5, corresponding to a positively sloped diagonal *ROC* curve, it indicates that the measure in question cannot distinguish between the two classifications being considered beyond chance; but when the *AUC* of a curve is 1 it indicates that the measure in questions achieves a perfect separation of the two classifications being considered (i.e. the closer the trajectory of the *ROC* curve to the top of the sensitivity axis throughout the false positive axis the more effective the relevant measure is at discriminating between the two relevant classifications). Once an *AUC* has been calculated for a given dichotomous classification problem its statistical significance is usually calculated in comparison to the asymptotically normal population distribution of *AUC* about 0.5 (i.e. where the two classification groups have equal sampling likelihoods; IBM Corp., 2012, pp. 839-844).

APPENDIX 8

On the Distribution of D_{IAT}s versus *trial-type* D_{IRAP}s about Zero

Figure A8.1 reveals that unlike the smokers' D_{IAT}s which appear to have polarized on either side of the neighbourhood of zero, the smokers' four *trial-type* D_{IRAP}s were continuously and (approximately) normally distributed across that neighbourhood. Indeed, not one smoker scored within $-.1 < D_{IAT} < .1$, even though almost a third of IAT smokers had a $D_{IAT} < -.1$, with the other two thirds scoring $D_{IAT} > .1$. What is more, only three smokers scored within the relatively large interval $-.3 < D_{IAT} < .3$ (i.e. corresponding to $-.59 < d < .59$ in the present sample), and all three scores were decidedly non-zero: one smoker scored $D_{IAT} = -.23$; another scored $D_{IAT} = .13$; and the third scored $D_{IAT} = .20$.

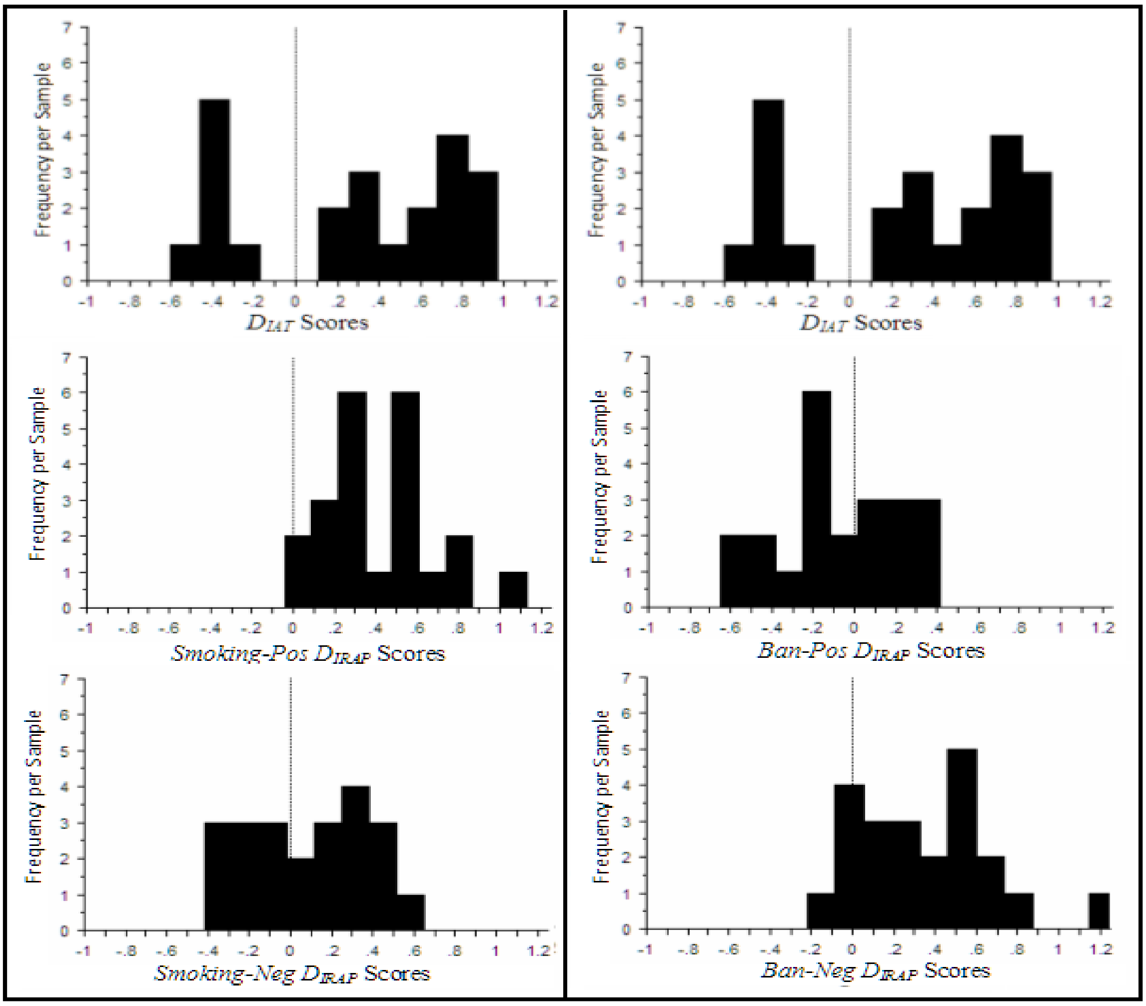


Figure A8.1. Histograms of smokers' four *trial-type* D_{IRAP}s juxtaposed with histogram of matched smokers' D_{IAT}s. Most notably, smokers' *trial-type* D_{IRAP}s appear to be continuously and approximately normally distributed across the region of zero, but there is a clear discontinuity in smokers' D_{IAT}s around zero.

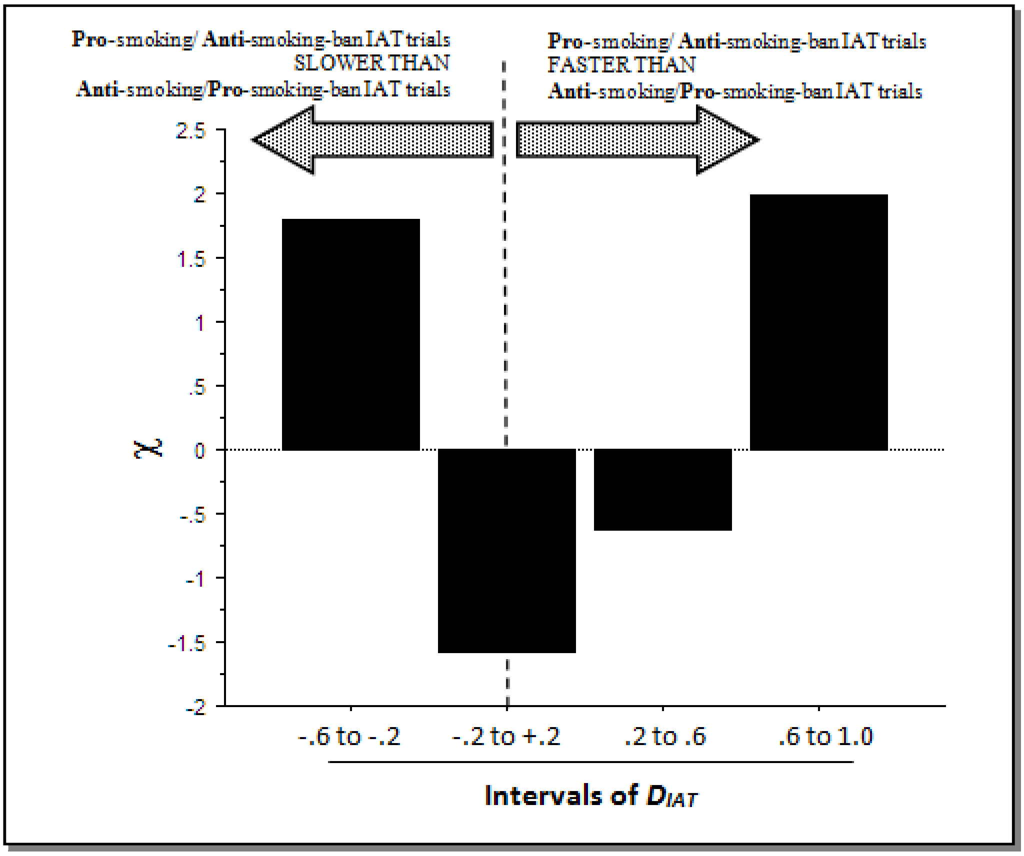


Figure A8.2. A *Yates*-corrected chi-gram illustrating the dramatic polarization of the smokers' D_{IAT} scores. In essence, the chi-gram displays the differences between the observed patterns with which smokers' D_{IAT} scores occurred in four intervals of D_{IAT}, versus the expected patterns with which they would have occurred in these intervals if they were sampled from a normal population distribution. The vertical axis graduates *chi* (χ), which for each interval of D_{IAT} on the horizontal axis is the observed D_{IAT} count minus the expected D_{IAT} count divided by the square root of the expected D_{IAT} count. Thus a zero χ score indicates that the number of smokers scoring within the relevant interval of D_{IAT} conforms to the standard normal model; and in contrast, a positive versus negative χ score respectively indicates that the smokers collectively produced more versus less D_{iAT}s in the relevant interval than is predicted by the standard normal model.

Furthermore, as illustrated in Figure A8.2, we conducted a *chi-square goodness-of-fit test* on four adjacent intervals of smokers' D_{IAT}s to quantify the extent to which the respective frequencies observed in each interval was different from what would be expected if it were normally distributed. The four intervals of D_{IAT}, $-.6 < D_{IAT} < -.2$, $-.2 < D_{IAT} < +.2$, $.2 < D_{IAT} < .6$, $.6 < D_{IAT} < 1.0$, were chosen based upon three criteria: (a) the inclusion of all observed D_{IAT}s in terms of one-decimal of *D* (b) adjacency to each other; (c) and thirdly, being of sufficient width to at least approximate the minimum expected count per cell of five needed to conduct a *chi-squared goodness-of-fit test*. Overall, the *chi-squared* test indicated that the smokers produced much fewer scores in the middle of their D_{IAT} distribution (i.e. around $D_{IAT} = 0$), and proportionately more at either end of their D_{IAT} distribution, than would be expected if these scores were in fact sampled from a homogenous normal population of D_{IAT}s, $\chi^2(3, 22) = 9.98$, $p = .019$, *Cramer's* $V = .38$

(i.e. a *Cramer's V* of .29 with *df* = 3 conventionally qualifies as large; Cohen, 1988). Using the appropriate *Z*-transformations with $M = .275$ and $SD = .51$, $-.6 < D_{IAT} < -.2$ would be expected to contain 13.4% of the sample (i.e. 2.9 smokers) versus the 27% observed (i.e. 6 smokers); $-.2 < D_{IAT} < +.2$ would be expected to contain 26.4% of the sample (i.e. 5.8 smokers) versus the 9% observed (i.e. 2 smokers); $.2 < D_{IAT} < .6$ would be expected to contain 29.9% of the sample (i.e. 6.6 smokers) versus the 23% observed (i.e. 5 smokers); $.6 < D_{IAT} < 1.0$ would be expected to contain 18.3% (i.e. 4 smokers) versus the 36% observed (i.e. 8 smokers).[88]

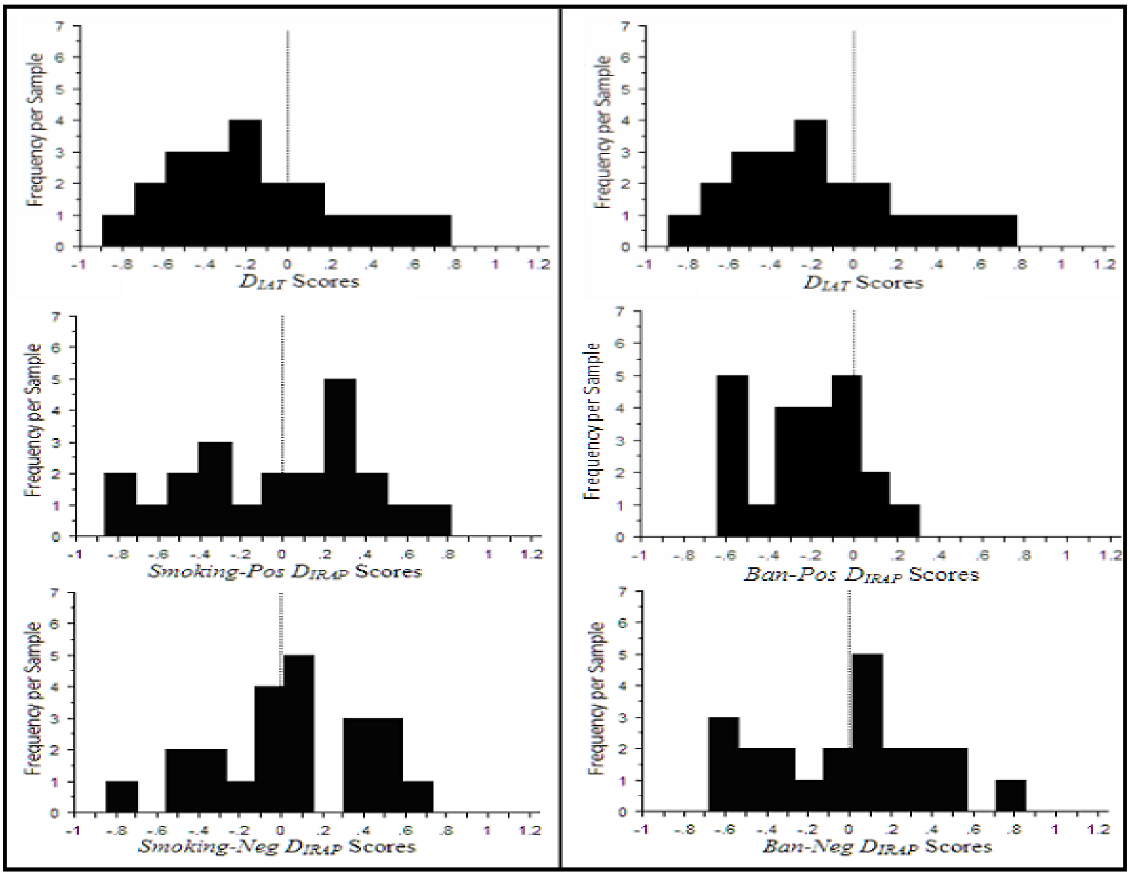


Figure A8.3. Histograms of non-smokers' four *trial-type* D_{IRAP}s juxtaposed with histogram of matched non-smokers' D_{IAT}s. Both non-smokers' *trial-type* D_{IRAP}s and matched non-smokers' D_{IAT}s appear to be continuously and approximately normally distributed across the region of zero.

In contrast, Figure A8.3 suggests that non-smokers' D_{IAT}s were continuously and normally distributed across the neighbourhood of zero much as non-smokers' *trial-type* D_{IRAP}s. Indeed, *normal Q-Q* plots and *Shapiro-Wilks tests* confirmed that all of the smokers' and non-smokers' *trial-type* D_{IRAP}s were approximately normally distributed, Ws $\leq .95$, ps $\geq .36$; as were the IAT non-smokers' D_{IAT}s, $W(21) = .97$, $p = .69$; but that

[88] On balance, the expected frequencies observed here are sufficiently high particularly given that there are no discrepancies among them (see Cochran, 1954; Hogg & Tanis, 1996; Howell, 2012b; Kirkman, 1996; Yarnold, 1970).

in contrast the IAT smokers' D_{IAT}s did somewhat violate normality due to its narrow and polarized tails, $W(22) = .90$, $p = .03$. In summary, therefore, the smokers' D_{IAT}s were unique among the other samples of *D*s obtained in the present study, insofar as they were bimodally distributed into sub-varieties on either side of zero.

APPENDIX 9

Plots of Smoking-status Sensitivity versus Specificity for *Smoking-Pos D_{IRAP}* and *Ban-Neg D_{IRAP}*

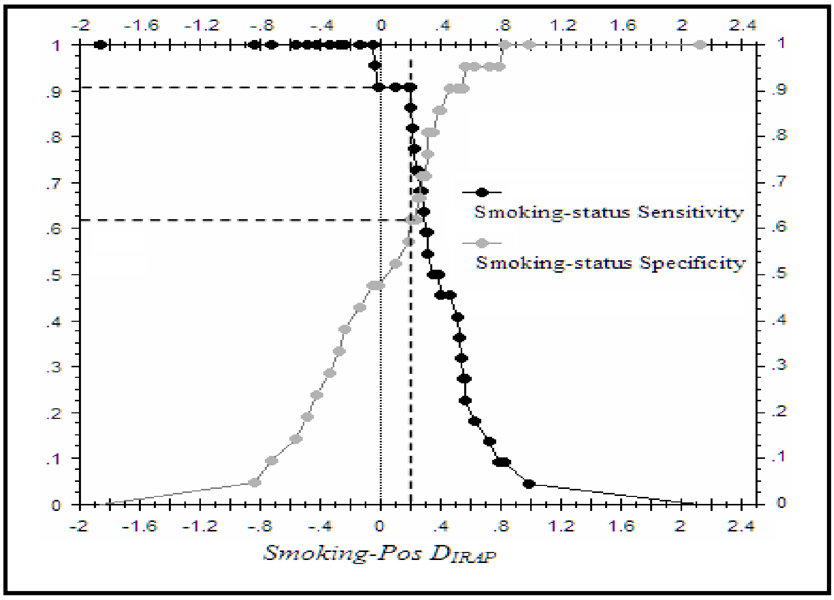


Figure A9.1. The sensitivity versus specificity of the *Smoking-Pos D_{IRAP}* to smoking-status across relevant *ROC curve* coordinates. The intersection between the dashed vertical line and the two dashed horizontal lines at *Smoking-Pos D_{IRAP}* = .196 represents the point at which *Smoking-Pos D_{IRAP}* has maximal combined sensitivity and specificity for identifying smokers from the present sample (i.e. 91% sensitivity, and 62% specificity).

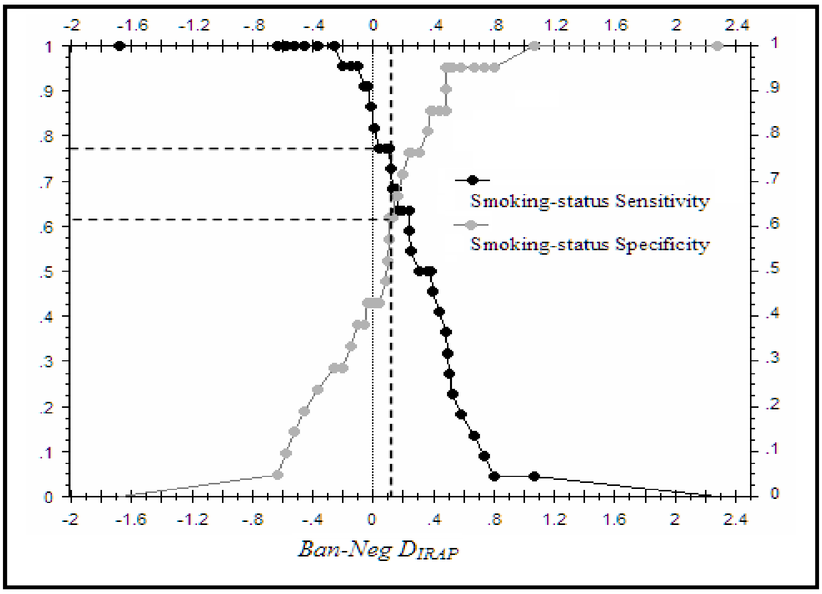


Figure A9.2. The sensitivity versus specificity of the *Ban-Neg D_{IRAP}* to smoking-status across relevant *ROC curve* coordinates. The intersection between the dashed vertical line and the two dashed horizontal lines at *Ban-Neg D_{IRAP}* = .11 represents the point at which *Ban-Neg D_{IRAP}* has maximal combined sensitivity and specificity for identifying smokers from the present sample (i.e. 77% sensitivity, and 62% specificity).

APPENDIX 10

Statistical Details for the Bootstrapped Models of the Various *D*s in Terms of Years Smoking and Trait Smoking Compulsion Intensity

We used the Hayes (2012) '*PROCESS*' software for casual path analysis to individually model the causal provenance of each of the various *D*s in terms of years smoking (YS) and trait smoking compulsion intensity (i.e. CPD, mFTQ or HONC). Essentially, the *PROCESS* software uses *SPSS* macros to implement an innovative bootstrapping procedure which is designed to estimate the sampling distribution of the *indirect causal path* (i.e. between the independent variable and the relevant dependent variable via its purported mediator), thereby providing a non-parametric estimate of the indirect effect *95% Confidence Interval (CI)*; and if the *CI* excludes zero this means that the indirect path is statistically significant and otherwise not (see also Preacher & Hayes, 2004; 2008; Hayes, 2009). CPD, mFTQ and HONC were tested *separately* as candidate mediators between YS and each of the *D*s because of the high degree of collinearity and conceptual reciprocity among them (i.e. both here and in the literature they constitute indeterminately overlapping aspects of the trait intensity of smoking compulsions; see DiFranza, Ursprung, & Biller, 2012; O'Loughlin et al., 2002, *p*. 359; Wellman et al., 2006).

Modelling Some Causes of D_{IAT}

Our three mediation models respectively indicated that the relationship between YS and D_{IAT} was dominantly mediated by CPD and mFTQ, κ^2s = .34, .26 (i.e. *p*s ≤ .05 for both κ^2s and the corresponding mediation paths), and less so by HONC, $\kappa^2 = .13$, $p > .05$. In particular, as illustrated in Figure A10.1 below, the mediation path (*ab*) from YS to D_{IAT} via CPD was a very large proportion of the total effect (path c') of YS on D_{IAT} (i.e. $\kappa^2 = .34$ means that path *ab* was 34% of the maximum mediation effect possible given the observed patterns of variation between YS and D_{IAT}). Overall, YS and the respective smoking-compulsion intensity traits accounted for approximately 10-19% of the observed variation in D_{IAT}, of which the *indirect path ab* constituted approximately 1.7% of D_{IAT} via CPD, 0.6% of D_{IAT} via mFTQ, and 0.2% of D_{IAT} via HONC (i.e. the latter three percentages were obtained by multiplying the R^2 for *path c* between YS and D_{IAT} with each corresponding κ^2 in Figure A10.1 and Table A10.1).

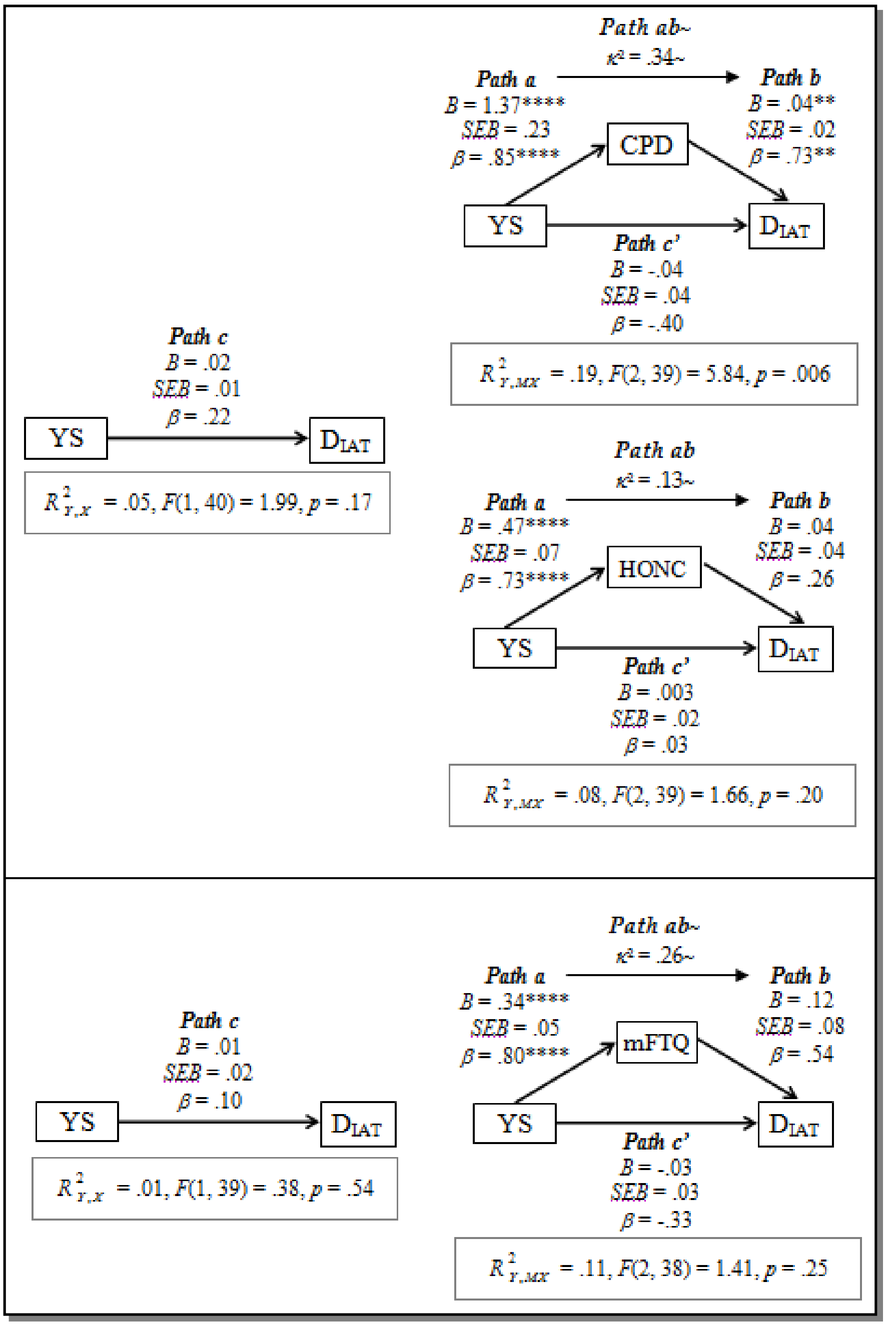


Figure A10.1. Path diagrams of the total effects model from years smoking (YS) to D_{IAT} versus the corresponding mediation model via CPD. A tilde (~) indicates that the relevant mediation statistic was found to be significant at $p < .05$ using 5000 bias corrected (BC) bootstrapped resamples of $n = 42$ with replacement. *SEB* signifies the standard error of a path's unstandardised OLS regression coefficients, *B*; the βs are the standardized versions of these *B*s. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$, **** $p \leq .0001$.

Table A10.1
Statistical details of the total effects model from YS to D_{IAT}, along with the statistical details of the bootstrapped mediation models from YS to D_{IAT} via CPD, HONC or mFTQ.

	Point Estimate	*SE*	*p*	95% Confidence Intervals (*CI*) Lower CI	Upper CI
Models w/out CPD or HONC					
Intercept	-.04	.09	.69	-.19	.12
YS → D_{IAT} (path c)	.02	.02	.33	-.01	.05
$R^2_{Y,X}$ (path c)	.05	.05	.33	-.05	.15
Model with CPD					
Intercept	-.13	.09	.14	-.28	.02
YS → CPD (path a)	1.37	.23	< .0001	.99	1.75
CPD → D_{IAT} (path b)	.04	.02	.02	.01	.07
YS → D_{IAT} (path c')	-.04	.04	.68	-.09	.03
Indirect effect (path ab)	.06	.03	--[a]	.02	.10
$R^2_{M,X}$ (path a)	.72	.07	< .0001	.61	.83
$R^2_{Y,MX}$ (paths b and c')	.19	.07	.006	.03	.35
κ^2	.34	.13	--[a]	.11	.54
Model with HONC					
Intercept	-.09	.10	.70	-.25	.07
YS → mFTQ (path a)	.47	.07	< .0001	.33	.61
mFTQ → D_{IAT} (path b)	.04	.04	.33	-.03	.10
YS → D_{IAT} (path c')	.003	.02	.93	-.05	.06
Indirect effect (path ab)	.02	.02	--[a]	-.01	.05
$R^2_{M,X}$ (path a)	.53	.12	< .0001	.33	.73
$R^2_{Y,MX}$ (paths b and c')	.08	.16	.27	-.04	.20
κ^2	.13	.11	--[a]	.01	.34
Model w/out mFTQ					
Intercept	-.02	.09	.85	-.17	.14
YS → D_{IAT} (path c)	.01	.02	.54	-.03	.04
$R^2_{Y,X}$ (path c)	.01	.03	.54	-.04	.06
Model with mFTQ					
Intercept	-.10	.09	.56	-.26	.05
YS → mFTQ (path a)	.34	.05	< .0001	.26	.42
mFTQ → D_{IAT} (path b)	.12	.08	.13	-.01	.26
YS → D_{IAT} (path c')	-.03	.03	.72	-.09	.03
Indirect effect (path ab)	.04	.03	--[b]	.01	.09
$R^2_{M,X}$ (path a)	.65	.09	< .0001	.51	.79
$R^2_{Y,MX}$ (paths b and c')	.11	.20	.25	-.21	.43
κ^2	.26	.13	--[b]	.05	.48

Note. All 95% *CI*s are one-tailed as per Steiger (2004, p. 174) and all were bootstrapped except those for the R^2 estimates which weren't provided for in the *PROCESS* software.
[a] 20,000 bias corrected (BC) bootstrap resamples with replacement of $n = 42$; one multivariate outlier was found using the Mahalanobis distance method and removed from both the CPD and HONC models.
[b] 20,000 BC bootstrap resamples with replacement of $n = 41$; two multivariate outliers were found using the Mahalanobis distance method and removed.

Modelling Some Causes of the four trial-type D_{IRAP}s

The Smoking-Pos D_{IRAP}. As illustrated in Figure A10.2 below CPD, mFTQ and HONC all dominated the relationship between YS and *Smoking-Pos D_{IRAP}* as very strong mediators (i.e. $.17 \leq \kappa^2$s $\leq .30$, *p*s $\leq .05$). Specifically, as per Hayes (2013, *pp*. 158-164), although path c was large in all cases, βs = .44, in contradiction path c' was relatively small in each of the three mediation models, $\beta = -.03$. Thus, for example, approximately (κ^2 =) 30% of the 20% shared *total effects* variance between YS and *Smoking-Pos D_{IRAP}* could, in principle, have developed in tandem with HONC; which approximately amounts to a moderate 6% of *Smoking-Pos D_{IRAP}* overall (or an $r \approx .24$). And likewise, a small-to-moderate 4.4% and 3.4% of *Smoking-Pos D_{IRAP}* (or *r*s ≈ .21, .18) could in principle have developed in tandem with CPD and mFTQ, respectively. In addition, our analysis indicated that between 5%-10% of *Smoking-Pos D_{IRAP}* was likely to have resulted collaterally from each of the three smoking-compulsion intensity traits (i.e. as compared to the 5% of *Smoking-Pos D_{IRAP}* which on average could have developed in tandem with tobacco addiction). Specifically, CPD, mFTQ and HONC all accounted for *Smoking-Pos D_{IRAP}* even while controlling for YS, respective R^2*-changes* = .09, .05, .10, $F(1, 41)$s = 5.30, 2.91, 6.08, *p*s = .03, .10, .02 (see Table A10.2 for model details).

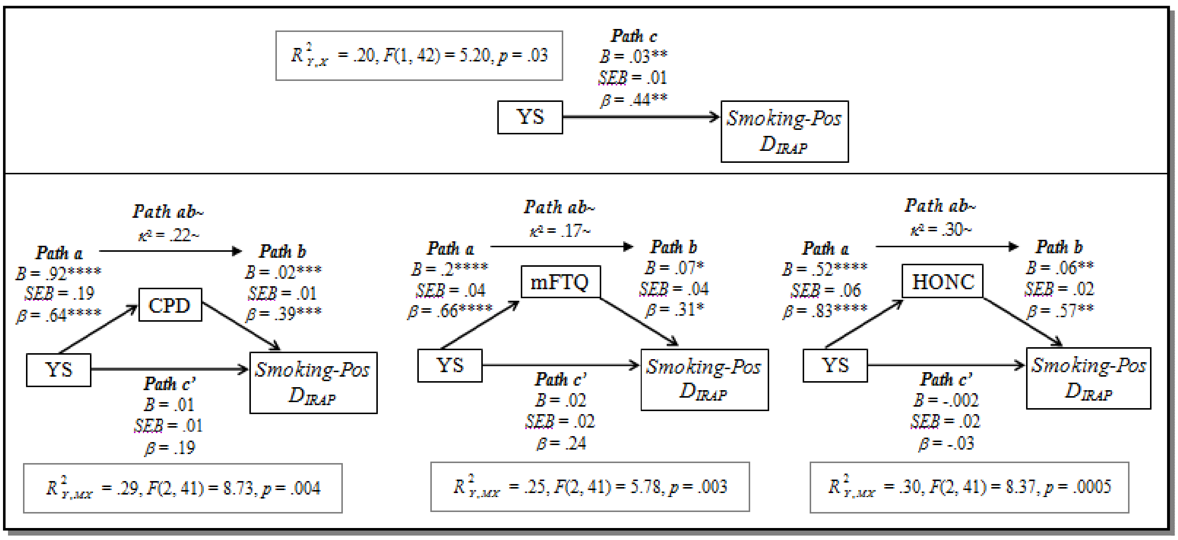


Figure A10.2. Path diagram on top of the total effects model from years smoking (YS) to *Smoking-Pos D_{IRAP}*, followed underneath by the corresponding path diagrams for mediation models via CPD, mFTQ or HONC. A tilde (~) indicates that the relevant mediation statistic was significant at $p < .05$ using 5000 BC bootstrapped resamples of $n = 44$ with replacement; and otherwise not. *SEB* signifies the standard error of a path's unstandardised OLS regression coefficients (i.e. *B*); the βs are the standardized versions of these *B*s. * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$, **** $p \leq .0001$, with all *p*s one-tailed.

Table A10.2
Statistical details of the bootstrapped mediation and total effects models from YS to Smoking-Pos D_{IRAP} via CPD, mFTQ or HONC.

	Point Estimate	*SE*	*P*	95% Confidence Intervals (*CI*) Lower CI	Upper CI
Model w/out CPD, mFTQ or HONC					
Intercept	.06	.08	.45	-.08	.20
YS → D_{IRAP} (path c)	.03	.01	.03	.01	.05
$R^2_{Y,X}$ (path c)	.20	.08	.03	.02	.38
Model with CPD					
Intercept	-.004	.09	.97	-.15	.15
YS → CPD (path a)	.92	.19	< .0001	.61	1.23
CPD → D_{IRAP} (path b)	.02	.01	.001	.01	.03
YS → D_{IRAP} (path c')	.01	.01	.17	-.01	.04
Indirect effect (path ab)	.02	.01	--[a]	.01	.03
$R^2_{M,X}$ (path a)	.41	.11	< .0001	.23	.59
$R^2_{Y,MX}$ (paths b and c')	.29	.10	.0007	.12	.46
κ^2	.22	.07	--[a]	.09	.32
Model with mFTQ					
Intercept	-.004	.09	.97	-.16	.15
YS → mFTQ (path a)	.20	.02	< .0001	.13	.27
mFTQ → D_{IRAP} (path b)	.07	.04	.04	.04	.14
YS → D_{IRAP} (path c')	.02	.02	.30	-.01	.04
Indirect effect (path ab)	.01	.01	--[a]	.003	.03
$R^2_{M,X}$ (path a)	.44	.10	< .0001	.27	.61
$R^2_{Y,MX}$ (paths b and c')	.25	.10	.006	.09	.41
κ^2	.17	.09	--[a]	.04	.32
Model with HONC					
Intercept	-.02	09	.87	-.17	.14
YS → HONC (path a)	.52	.06	< .0001	.42	.62
HONC → D_{IRAP} (path b)	.06	.02	.004	.03	.10
YS → D_{IRAP} (path c')	-.002	.02	.55	-.03	.03
Indirect effect (path ab)	.03	.01	--[a]	.02	.05
$R^2_{M,X}$ (path a)	.68	.07	< .0001	.56	.80
$R^2_{Y,MX}$ (paths b and c')	.30	.10	.0009	.13	.47
κ^2	.30	.09	--[a]	.16	.44

Note. All 95% *CI*s are one-tailed as per Steiger (2004, p. 174) and all were bootstrapped except those for the R^2 estimates which weren't provided for in the *PROCESS* software.
[a] 20,000 BC bootstrap resamples of $n = 44$ with replacement; we found one multivariate outlier using the Mahalanobis distance method (in the CPD model) but did not exclude it because upon closer examination it was an extension of the predicted effect.

The Ban-Neg D_{IRAP}. The relationship between YS and *Ban-Neg D_{IRAP}* appeared to be dominantly mediated by CPD, mFTQ and HONC, respectively, κ^2s = .19, .19, .19

(for relevant model Figure A10.3 and Table A10.3). However, given that the relationship between YS and *Ban-Neg* D_{IRAP} was only moderate to begin with, $F(1, 42) = 5.23$, $R^2 = .11$, $p = .03$, therefore only a null 2% of *Ban-Neg* D_{IRAP} could have developed in tandem with CPD, mFTQ or HONC across YS (i.e. $r = .14$). In contrast, a further 5-8% of *Ban-Neg* D_{IRAP} appeared to be an artefact of the three smoking-compulsion intensity traits, R^2*-changes* = .08, .07, .05, $F(1, 41)$s = 3.92, 3.52, 2.34, *ps* = .05, .07, .13.

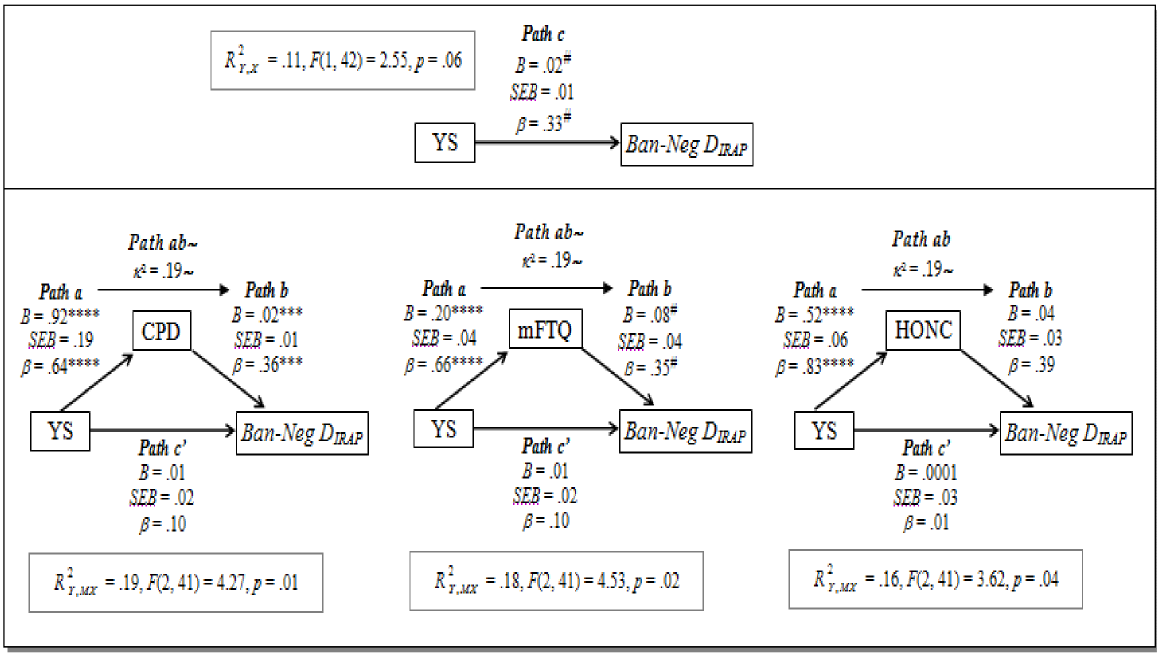


Figure A10.3. Path diagrams for the total effect model from years smoking (YS) to *Ban-Neg* D_{IRAP}, for the corresponding mediation models via cigarettes smoked per day (CPD), mFTQ and HONC. All models involved $n = 44$. A tilde (~) indicates that the relevant mediation statistic was significant at $p < .05$ using 5000 BC bootstrapped resamples of n with replacement; and otherwise not. *SEB* signifies the standard error of a path's unstandardised OLS regression coefficients (i.e. B); the βs are the standardized versions of these Bs. # $p \leq .10$, * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$, **** $p \leq .0001$, with all *ps* one-tailed.

Table A10.3

Statistical details of the bootstrapped mediation and total effects models from YS to Ban-Neg D_{IRAP} via CPD, mFTQ or HONC.

	Point Estimate	*SE*	*p*	*95% Confidence Intervals (CI)* Lower CI	Upper CI
Model w/out CPD, mFTQ or HONC					
Intercept	.07	.08	.36	-.06	.20
YS → D_{IRAP} (path c)	.02	.01	.12	-.001	.04
$R^2_{Y,X}$ (path c)	.11	.08	.12	-.03	.25
Model with CPD					
Intercept	.01	.08	.87	-.12	.15
YS → CPD (path a)	.92	.19	< .0001	.61	1.23
CPD → D_{IRAP} (path b)	.02	.01	.02	.005	.03
YS → D_{IRAP} (path c')	.01	.02	.68	-.02	.04
Indirect effect (path ab)	.01	.01	--[a]	.01	.03
$R^2_{M,X}$ (path a)	.41	.11	< .0001	.23	.59
$R^2_{Y,MX}$ (paths b and c')	.19	.10	.02	.03	.35
κ^2	.19	.07	--[a]	.07	.30
Model with mFTQ					
Intercept	-.002	.08	.99	-.14	.14
YS → mFTQ (path a)	.20	.04	< .0001	.13	.27
mFTQ → D_{IRAP} (path b)	.08	.04	.07	.01	.14
YS → D_{IRAP} (path c')	.01	.02	.75	-.03	.04
Indirect effect (path ab)	.02	.01	--[a]	.0004	.09
$R^2_{M,X}$ (path a)	.44	.05	< .0001	.27	.61
$R^2_{Y,MX}$ (paths b and c')	.18	.10	.02	.01	.35
κ^2	.19	.10	--[a]	.03	.35
Model with HONC					
Intercept	.02	.08	.80	-.12	.16
YS → HONC (path a)	.52	.06	< .0001	.42	.62
HONC → D_{IRAP} (path b)	.04	.03	.16	-.01	.09
YS → D_{IRAP} (path c')	.0001	.03	.98	-.04	.04
Indirect effect (path ab)	.02	.01	--[a]	-.002	.04
$R^2_{M,X}$ (path a)	.68	.07	< .0001	.56	.80
$R^2_{Y,MX}$ (paths b and c')	.16	.06	.04	.07	.25
κ^2	.19	.11	--[a]	.02	.38

Note. All 95% *CI*s are one-tailed as per Steiger (2004, p. 174) and all were bootstrapped except those for the R^2 estimates which weren't provided for in the *PROCESS* software.

[a] 20,000 BC bootstrap resamples of n = 44 with replacement; we found one multivariate outlier using the Mahalanobis distance method (in the CPD model) but did not exclude it because upon closer examination it was an extension of the predicted effect.

APPENDIX 11

Compensation Algorithm for Comparisons of *trial-type* D_{IRAP} r_{sb}s with D_{IAT} r_{sb}s

The *Spearman-Brown's generalized prediction formula* is:

$$\rho'_{XX} = \frac{k\rho_{xx}}{1 + (k-1)\rho_{xx}}$$

where ρ_{xx} is the internal reliability of the current test, and where ρ'_{xx} is the predicted internal reliability of current test if all things being equal it had *k* times as many test items as the current test (i.e. this can include numbers between zero and one if we wish to hypothetically shorten the current test; Brown, 1910; Spearman, 1910; Wainer & Thissen, 2001; Webb et al., 2007, p. 87).

Thus,

$$r_{sb} = \frac{2|r|}{1 + (2-1)|r|} = \frac{2|r|}{1 + |r|}$$

where r_{sb} is the Spearman-Brown corrected split-half reliability of a given test, and where *r* is the split-half reliability of that test.

Thus, the formula for the '*compensated* r_{sb}s' which predict the internal reliability of a given *trial-type* D_{IRAP}s with four times as many of the same trials as they have (i.e. as many as the D_{IAT}),

$$\textit{Compensated } r_{sb} = \frac{4|r_{sb}|}{1 + (4-1)|r_{sb}|} = \frac{4|r_{sb}|}{1 + 3|r_{sb}|}$$

where r_{sb} is the Spearman-Brown corrected split-half correlation of the original *trial-type* D_{IRAP} in question.

APPENDIX 12

The Demographic and Behavioural History Questionnaire Version 2 (DBHQ-2)

Participant Code: ___________

Background Questionnaire

Please try to answer the following questions as ***accurately*** as possible:

1. What is your age, sex, current occupation, and highest level of education?

2. Have you ever lived in a home where people smoked indoors? Give brief details about when and for how long.

3. Do you buy cigarettes on a regular basis or do you smoke only socially? If not please describe how often you do smoke:

4. If you don't currently smoke please describe any previous experience (when and for how long) you have with smoking no matter how long ago. Do you or have you smoked other substances besides tobacco; have you had difficulties with alcohol or drugs? Please give details as appropriate:

5. Please describe your smoking history in terms of when you first started smoking AND when you first started buying cigarettes regularly.

ONLY if you have never EVER smoked then skip questions 6-18

6. It is important that you now describe when & for how long you have quit smoking on previous occasions. Approximately how long in total have you refrained from smoking since you first began smoking on a regular basis?

7. Have thoughts about stopping smoking been more frequent recently THAN IS USUAL FOR YOU? If so please rate the following scale AND give further details about how long this has been happening and why?

Less Quitting Thoughts than Usual — **As Usual** — **More Quitting Thoughts than Usual**

0 1 2 3 4 5 6

8. Have you made plans to quit smoking soon? If so do you believe that your quitting-intentions are the same, weaker or stronger than previous quit attempts? Please rate:

Weakest Intentions — **Strongest Intentions**

0 1 2 3 4 5 6

Give details of background to these plans:

9. On how many days of the PAST 30 DAYS did you smoke a cigarette?

0-10 days   10-20 days   20-30 days

30 days

Participant Code: ___________

10. Have you made an attempt to quit smoking during the past 30 days? If yes then please give details of when and for how long:

Please rate how much exertion you put into this quit attempt on the following scale:

Least Effort 0 — 1 — 2 — 3 — 4 — 5 — 6 **Most Effort**

11. Are you currently smoking more or less cigarettes per day than is usual for you?

Less than Usual 0 — 1 — 2 — **As Usual** 3 — 4 — 5 — 6 **More than Usual**

Please give details:

12. *Exactly* how many cigarettes have you USUALLY smoked per day during the past 30 days?

13. What treatments have you previously used to help you stop smoking? Please give details of type and length of treatment and how long ago:

14. How long without smoking before you first feel like you would LIKE a cigarette?

Less than an hour 1-3 hours More than 3 hours, but less than a day

A full day 1-6 days A week or more Forever

15. How long without smoking before you first feel like you NEED a cigarette?

Less than an hour 1-3 hours More than 3 hours, but less than a day

A full day 1-6 days A week or more Forever

17. Do you think you will smoke a cigarette when this study is finished?

Definitely not ______ Possibly ______ Probably ______ Definitely

18. Please indicate when you WANT to completely stop smoking cigarettes forever?

NOW ------ SOON ------ LATER -------- NEVER

This study is now complete; please report to the researcher for debriefing.

Thank you very much for your participation.

APPENDIX 13

The Avoidance and Inflexibility Scale (AIS)

AIS

Assessment________________ Date__________________ ID___________________

Below are three sections. In each section you will find a statement followed by a list of questions. Please rate your response to each question by circling the appropriate number on the scale beneath the question. There are no right or wrong answers.

Section One

Sometimes people have thoughts that encourage them to smoke, for example, "I need a cigarette!" or "I wish I could have a cigarette now!"

1. How likely is it that these thoughts will lead you to smoke?

1------------------2-------------------3------------------4--------------------5
not at all a little somewhat considerably very likely

2. How much are you struggling to control these thoughts?

1------------------2-------------------3------------------4--------------------5
not at all a little somewhat considerably very much

3. To what degree must you reduce how often you have these thoughts in order not to smoke?

1------------------2-------------------3------------------4--------------------5
not at all a little somewhat considerably extensively

4. To what degree must you reduce the intensity of these thoughts in order not to smoke?

1------------------2-------------------3------------------4--------------------5
not at all a little somewhat considerably extensively

Section Two

Sometimes people have feelings that encourage them to smoke, for example, they may really feel like having a cigarette, and/or they may have feelings such as stress, enjoyment, fatigue, boredom, satisfaction, etc. that encourage them to smoke.

5. How likely is it that these feelings will lead you to smoke?

1-----------------2-----------------3-----------------4------------------5
not at all a little somewhat considerably very likely

6. How important is getting rid of these feelings?

1-----------------2-----------------3-----------------4------------------5
not at all a little somewhat considerably very important

7. How much are you struggling to control these feelings?

1-----------------2-----------------3-----------------4------------------5
not at all a little somewhat considerably very much

8. To what degree must you reduce how often you have these feelings in order not to smoke?

1-----------------2-----------------3-----------------4------------------5
not at all a little somewhat considerably extensively

9. To what degree must you reduce the intensity of these feelings in order not to smoke?

1-----------------2-----------------3-----------------4------------------5
not at all a little somewhat considerably extensively

Section Three

Sometimes people have bodily sensations that encourage them to smoke. For example, physical cravings or withdrawal symptoms may encourage people to smoke.

10. How likely is it that these bodily sensations will lead you to smoke?

1-----------------2-----------------3-----------------4-------------------5
not at all a little somewhat considerably very likely

11. How much are you struggling to get rid of these bodily sensations?

1-----------------2-----------------3-----------------4-------------------5
not at all a little somewhat considerably very much

12. To what degree must you reduce how often you have these bodily sensations in order not to smoke?

1-----------------2-----------------3-----------------4-------------------5
not at all a little somewhat considerably extensively

13. To what degree must you reduce the intensity of these bodily sensations in order not to smoke?

1-----------------2-----------------3-----------------4-------------------5
not at all a little somewhat considerably extensively

APPENDIX 14

Bootstrapped Mediaton Models of the four IRAP trial-type effects in Terms of Trait Tobacco Addiction Intensity across Years Smoking

Table A14.1

Statistical details of the bootstrapped mediation and total effects models from YS to Enjoy-Pos D_{IRAP} via CPD, mFTQ, HONC or *AIS.*

	Point Estimate	*SE*	*p*	*95% Confidence Intervals (CI)* Lower CI	Upper CI
Model w/out CPD[a]					
Intercept	.24	.08	.01	.10	.37
YS → D_{IRAP} (path c)	.05	.03	.05	.01	.10
$R^2_{Y,X}$ (path c)	.11	.08	.05	-.05	.27
Model w/out mFTQ or HONC[b]					
Intercept	.22	.08	.01	.08	.36
YS → D_{IRAP} (path c)	.06	.03	.08	.004	.11
$R^2_{Y,X}$ (path c)	.12	.08	.06	-.04	.28
Model w/out AIS[c]					
Intercept	.22	.08	.01	.08	.36
YS → D_{IRAP} (path c)	.05	.04	.14	-.01	.11
$R^2_{Y,X}$ (path c)	.09	.08	.14	-.06	.24
Model with CPD[a]					
Intercept	.24	.08	.01	.10	.38
YS → CPD (path a)	2.78	.14	< .0001	2.54	3.02
CPD → D_{IRAP} (path b)	-.03	.04	.53	-.10	.04
YS → D_{IRAP} (path c')	.12	.11	.28	-.07	.32
Indirect effect (path ab)	-.07	.13	--[a]	-.24	.15
$R^2_{M,X}$ (path a)	.95	.02	< .0001	.92	.98
$R^2_{Y,MX}$ (paths b and c')	.12	.08	.10	-.04	.28
κ^2	.11	.11	--[a]	.005	.29
Model with mFTQ[b]					
Intercept	.22	.08	.01	.08	.35
YS → mFTQ (path a)	2.80	.22	< .0001	2.43	3.17
mFTQ → D_{IRAP} (path b)	.005	.04	.90	-.06	.07
YS → D_{IRAP} (path c')	.04	.11	.71	-.15	.23
Indirect effect (path ab)	.01	.10	--[b]	-.17	.14
$R^2_{M,X}$ (path a)	.89	.03	< .0001	.83	.95
$R^2_{Y,MX}$ (paths b and c')	.11	.08	.18	-.05	.26
κ^2	.03	.11	--[b]	.00	.06

Table A14.1 (continued)

Statistical details of the bootstrapped mediation and total effects models from YS to Enjoy-Pos D_{IRAP} *via CPD, mFTQ, HONC* or *AIS.*

	Point Estimate	*SE*	*p*	95% Confidence Intervals (*CI*) *Lower CI*	*Upper CI*
Model with HONC[b]					
Intercept	.22	08	.01	.08	.35
YS → HONC (path a)	.68	.13	< .0001	.46	.90
HONC → D_{IRAP} (path b)	.06	.06	.32	-.04	.16
YS → D_{IRAP} (path c')	.02	.05	.76	-.07	.10
Indirect effect (path ab)	.04	.04	--[b]	-.02	.10
$R^2_{M,X}$ (path a)	.56	.09	< .0001	.38	.74
$R^2_{Y,MX}$ (paths b and c')	.16	.09	.08	-.01	. 33
κ^2	.17	.11	--[b]	.01	.36
Model with AIS[c]					
Intercept	.16	.19	.41	-.16	.48
YS → AIS (path a)	5.51	.79	< .0001	4.16	6.86
AIS → D_{IRAP} (path b)	.004	.01	.76	-.02	.03
YS → D_{IRAP} (path c')	.03	.09	.73	-.12	.18
Indirect effect (path ab)	.02	.07	--[c]	-.10	.13
$R^2_{M,X}$ (path a)	.74	.06	< .0001	.62	.86
$R^2_{Y,MX}$ (paths b and c')	.10	.08	.22	-.05	. 25
κ^2	.07	.13	--[c]	.0003	.19

Note. All 95% *CI*s are one-tailed as per Steiger (2004, p. 174) and all were bootstrapped except those for the R^2 estimates which weren't provided for in the *PROCESS* software.

[a] 20,000 BC bootstrap resamples of n = 30 with replacement (i.e. two multivariate outliers were found using the Mahalanobis distance method). [b] 20,000 BC bootstrap resamples of n = 32 with replacement (i.e. no multivariate outliers). [c] 20,000 BC bootstrap resamples of n = 31 (i.e. one multivariate outlier).

Table A14.2

Statistical details of the bootstrapped mediation and total effects models from YS to Enjoy-Neg D_{IRAP} via CPD, mFTQ, HONC or *AIS.*

	Point Estimate	*SE*	*p*	*95% Confidence Intervals (CI)* Lower CI	Upper CI
Model w/out CPD[a]					
Intercept	.17	.08	.04	.04	.31
YS → D_{IRAP} (path c)	.05	.02	.02	.02	.09
$R^2_{Y,X}$ (path c)	.14	.09	.02	-.04	.32
Model w/out mFTQ or HONC[b]					
Intercept	.16	.08	.05	.03	.30
YS → D_{IRAP} (path c)	.05	.02	.03	.01	.10
$R^2_{Y,X}$ (path c)	.15	.09	.03	-.03	.31
Model w/out AIS[c]					
Intercept	.16	.08	.06	.02	.29
YS → D_{IRAP} (path c)	.06	.03	.03	.02	.11
$R^2_{Y,X}$ (path c)	.15	.09	.03	-.03	.33
Model with CPD[a]					
Intercept	.17	.08	.04	.03	.31
YS → CPD (path a)	2.78	.14	< .0001	2.54	3.02
CPD → D_{IRAP} (path b)	.03	.03	.36	-.02	.08
YS → D_{IRAP} (path c')	-.03	.09	.75	-.18	.12
Indirect effect (path ab)	.08	.12	--[a]	-.05	.29
$R^2_{M,X}$ (path a)	.95	.02	< .0001	.92	.98
$R^2_{Y,MX}$ (paths b and c')	.15	.09	.07	-.03	.33
κ^2	.13	.11	--[a]	.01	.34
Model with mFTQ[b]					
Intercept	.16	.08	.07	.02	.29
YS → mFTQ (path a)	2.80	.22	< .0001	2.43	3.17
mFTQ → D_{IRAP} (path b)	.01	.03	.68	-.03	.06
YS → D_{IRAP} (path c')	.02	.08	.78	-.11	.16
Indirect effect (path ab)	.03	.08	--[b]	-.11	.15
$R^2_{M,X}$ (path a)	.89	.03	< .0001	.83	.95
$R^2_{Y,MX}$ (paths b and c')	.14	.09	.12	-.03	.31
κ^2	.08	.11	--[b]	.001	.21

Table A14.2 (continued)

Statistical details of the bootstrapped mediation and total effects models from YS to Enjoy-Neg D_{IRAP} via CPD, mFTQ, HONC or *AIS.*

	Point Estimate	*SE*	*p*	95% Confidence Intervals (*CI*) Lower CI	Upper CI
Model with HONC[b]					
Intercept	.16	.08	.06	.02	.29
YS → HONC (path a)	.68	.13	< .0001	.46	.90
HONC → D_{IRAP} (path b)	.07	.03	.04	.02	.13
YS → D_{IRAP} (path c')	.004	.03	.91	-.05	.06
Indirect effect (path ab)	.05	.02	--[b]	.02	.09
$R^2_{M,X}$ (path a)	.56	.06	< .0001	.38	.74
$R^2_{Y,MX}$ (paths b and c')	.23	.10	.009	.04	.42
κ^2	.24	.10	--[b]	.07	.40
Model with AIS[c]					
Intercept	.07	.16	.69	-.21	.34
YS → AIS (path a)	5.51	.79	< .0001	4.16	6.86
AIS → D_{IRAP} (path b)	.01	.01	.54	-.01	.02
YS → D_{IRAP} (path c')	.03	.06	.67	-.08	.14
Indirect effect (path ab)	.03	.07	--[c]	-.07	.13
$R^2_{M,X}$ (path a)	.74	.06	< .0001	.62	.86
$R^2_{Y,MX}$ (paths b and c')	.17	.09	.13	-.01	.35
κ^2	.12	.13	--[c]	.003	.31

Note. All 95% *CI*s are one-tailed as per Steiger (2004, p. 174) and all were bootstrapped except those for the R^2 estimates which weren't provided for in the *PROCESS* software.

[a] 20,000 BC bootstrap resamples of n = 30 with replacement (i.e. two multivariate outliers were found using the Mahalanobis distance method). [b] 20,000 BC bootstrap resamples of n = 32 with replacement (i.e. no multivariate outliers). [c] 20,000 BC bootstrap resamples of n = 31 (i.e. one multivariate outlier).

Table A14.3

Statistical details of the bootstrapped mediation and total effects models from YS to Need-Pos D_{IRAP} via CPD, mFTQ, HONC or *AIS.*

	Point Estimate	*SE*	*P*	*95% Confidence Intervals (CI)* Lower CI	Upper CI
Model w/out CPD[a]					
Intercept	.16	.12	.18	-.04	.36
YS → D_{IRAP} (path c)	.07	.04	.049	.01	.13
$R^2_{Y,X}$ (path c)	.15	.09	.049	-.03	.33
Model w/out mFTQ or HONC[b]					
Intercept	.16	.12	.19	-.04	.35
YS → D_{IRAP} (path c)	.06	.03	.07	-.007	.12
$R^2_{Y,X}$ (path c)	.12	.08	.07	-.04	.28
Model w/out AIS[c]					
Intercept	.17	.12	.15	-.02	.37
YS → D_{IRAP} (path c)	.05	.03	.16	-.01	.10
$R^2_{Y,X}$ (path c)	.07	.07	.16	-.06	.20
Model with CPD[a]					
Intercept	.16	.12	.18	-.04	.36
YS → CPD (path a)	2.78	.14	< .0001	2.54	3.02
CPD → D_{IRAP} (path b)	.009	.05	.86	-.07	.09
YS → D_{IRAP} (path c')	.05	.13	.71	-.18	.28
Indirect effect (path ab)	.02	.10	--[a]	-.19	.20
$R^2_{M,X}$ (path a)	.95	.02	< .0001	.92	.98
$R^2_{Y,MX}$ (paths b and c')	.15	.09	.16	-.03	.33
κ^2	.03	.14	--[a]	.00	.07
Model with mFTQ[b]					
Intercept	.14	.12	.26	-.07	.34
YS → mFTQ (path a)	2.80	.22	< .0001	2.43	3.17
mFTQ → D_{IRAP} (path b)	.02	.02	.27	-.01	.06
YS → D_{IRAP} (path c')	-.0003	.07	.97	-.12	.11
Indirect effect (path ab)	.07	.07	--[b]	-.06	.16
$R^2_{M,X}$ (path a)	.89	.03	< .0001	.83	.95
$R^2_{Y,MX}$ (paths b and c')	.14	.09	.12	-.03	.31
κ^2	.13	.10	--[b]	.009	.30

Table A14.3 (continued)
Statistical details of the bootstrapped mediation and total effects models from YS to Need-Pos D_{IRAP} via CPD, mFTQ, HONC or *AIS.*

	Point Estimate	*SE*	*p*	95% Confidence Intervals (*CI*) Lower *CI*	Upper *CI*
Model with HONC[b]					
Intercept	.15	.12	.20	-.05	.35
YS → HONC (path a)	.68	.13	< .0001	.46	.90
HONC → D_{IRAP} (path b)	.06	.05	.25	-.03	.15
YS → D_{IRAP} (path c')	.02	.06	.72	-.09	.13
Indirect effect (path ab)	.04	.04	--[b]	-.01	.11
$R^2_{M,X}$ (path a)	.56	.09	< .0001	.38	.74
$R^2_{Y,MX}$ (paths b and c')	.16	.09	.08	-.01	. 33
κ^2	.16	.11	--[b]	.02	.35
Model with AIS[c]					
Intercept	.01	.17	.93	-.27	.30
YS → AIS (path a)	5.51	.79	< .0001	4.16	6.86
AIS → D_{IRAP} (path b)	.01	.01	.15	-.002	.02
YS → D_{IRAP} (path c')	-.01	.05	.85	-.09	.08
Indirect effect (path ab)	.06	.05	--[c]	-.01	.14
$R^2_{M,X}$ (path a)	.74	.06	< .0001	.62	.86
$R^2_{Y,MX}$ (paths b and c')	.10	.08	.18	-.05	. 25
κ^2	.17	.10	--[c]	.02	.33

Note. All 95% *CI*s are one-tailed as per Steiger (2004, p. 174) and all were bootstrapped except those for the R^2 estimates which weren't provided for in the *PROCESS* software.
[a] 20,000 BC bootstrap resamples of *n* = 30 with replacement (i.e. two multivariate outliers were found using the Mahalanobis distance method). [b] 20,000 BC bootstrap resamples of *n* = 32 with replacement (i.e. no multivariate outliers). [c] 20,000 BC bootstrap resamples of *n* = 31 (i.e. one multivariate outlier).

Table A14.4

Statistical details of the bootstrapped mediation and total effects models from YS to Need-Neg D_{IRAP} via CPD, mFTQ, HONC or *AIS.*

	Point Estimate	*SE*	*p*	95% Confidence Intervals (CI) Lower CI	Upper CI
Model w/out CPD[a]					
Intercept	.09	.09	.33	-.06	.24
YS → D_{IRAP} (path c)	.10	.03	.002	.05	.16
$R^2_{Y,X}$ (path c)	.29	.11	.002	.08	.50
Model w/out mFTQ or HONC[b]					
Intercept	.07	.09	.40	-.07	.22
YS → D_{IRAP} (path c)	.11	.03	.0001	.06	.16
$R^2_{Y,X}$ (path c)	.35	.11	.0001	.14	.56
Model w/out AIS[c]					
Intercept	.07	.09	.43	-.08	.22
YS → D_{IRAP} (path c)	.12	.03	.001	.06	.18
$R^2_{Y,X}$ (path c)	.34	.11	.001	.13	.55
Model with CPD[a]					
Intercept	.08	.09	.37	-.07	.23
YS → CPD (path a)	2.78	.14	< .0001	2.54	3.02
CPD → D_{IRAP} (path b)	.09	.05	.10	-.0001	.18
YS → D_{IRAP} (path c')	-.15	.16	.35	-.43	.12
Indirect effect (path ab)	.26	.15	--[a]	.06	.50
$R^2_{M,X}$ (path a)	.95	.02	< .0001	.92	.98
$R^2_{Y,MX}$ (paths b and c')	.38	.10	.001	.18	.58
κ^2	.35	.15	--[a]	.08	.58
Model with mFTQ[b]					
Intercept	.03	.08	.73	-.11	.17
YS → mFTQ (path a)	2.80	.22	< .0001	2.43	3.17
mFTQ → D_{IRAP} (path b)	.06	.03	.08	.003	.11
YS → D_{IRAP} (path c')	-.05	.10	.62	-.22	.12
Indirect effect (path ab)	.16	.09	--[b]	-.02	.27
$R^2_{M,X}$ (path a)	.89	.03	< .0001	.83	.95
$R^2_{Y,MX}$ (paths b and c')	.44	.10	.0009	.25	.63
κ^2	.36	.17	--[b]	.05	.60

Table A14.4 (continued)
Statistical details of the bootstrapped mediation and total effects models from YS to Need-Neg D_{IRAP} via CPD, mFTQ, HONC or *AIS.*

	Point Estimate	*SE*	*p*	*95% Confidence Intervals (CI)* Lower CI	Upper CI
Model with HONC[b]					
Intercept	.07	.09	.43	-.08	.22
YS → HONC (path a)	.68	.13	< .0001	.46	.90
HONC → D_{IRAP} (path b)	.07	.05	.19	-.02	.15
YS → D_{IRAP} (path c')	.07	.04	.13	-.01	.14
Indirect effect (path ab)	.04	.03	--[b]	-.02	.09
$R^2_{M,X}$ (path a)	.56	.09	< .0001	.38	.74
$R^2_{Y,MX}$ (paths b and c')	.39	.12	.003	.15	.63
κ^2	.19	.11	--[b]	.02	.36
Model with AIS[c]					
Intercept	-.20	.18	.26	-.50	.10
YS → AIS (path a)	5.51	.79	< .0001	4.16	6.86
AIS → D_{IRAP} (path b)	.02	.01	.13	-.002	.04
YS → D_{IRAP} (path c')	.02	.08	.81	-.12	.16
Indirect effect (path ab)	.10	.06	--[c]	-.01	.19
$R^2_{M,X}$ (path a)	.74	.06	< .0001	.62	.86
$R^2_{Y,MX}$ (paths b and c')	.43	.10	.001	.24	.62
κ^2	.31	.15	--[c]	.03	.54

Note. All 95% *CI*s are one-tailed as per Steiger (2004, p. 174) and all were bootstrapped except those for the R^2 estimates which weren't provided for in the *PROCESS* software.
[a] 20,000 BC bootstrap resamples of n = 30 with replacement; two multivariate outliers were found using the Mahalanobis distance method. [b] 20,000 BC bootstrap resamples of n = 32 with replacement (i.e. no multivariate outliers). [c] 20,000 BC bootstrap resamples of n = 31 (i.e. one multivariate outlier).

APPENDIX 15

The ‘Extended’ D_{IRAP}-algorithm

The details of the extended D_{IRAP}-algorithm are as follows: (i) only response-latency data from the three pairs of test blocks and the preceeding pair of practise blocks (i.e. the *criterion practise blocks*) are used; (ii) latencies above 10 000 *ms* are eliminated from the dataset; (iii) the entire IRAP dataset is eliminated for any participant for whom more than 10% of test-block trials have latencies less than 300*ms*; (iv) sixteen standard deviations are computed, one for the response latencies belonging to each trial-type across the two criterion practise blocks, and one for the response latencies belonging to each trial-type in each of the three pairs of test blocks; (v) 32 mean response latencies are calculated, one for each of the four trial-types in each criterion practise block and test block; (vi) 16 difference scores are computed, one per trial-type per pair of qualifying trial blocks, by subtracting the mean latency of each trial-type’s trials in each pro-smoking block from the mean latency of that trial-type’s trials in the paired anti-smoking block; (vii) each difference score is then divided by its corresponding standard deviation from step iv, yielding 16 *block-pair* D_{IRAP} scores; one score for each trial-type across the criterion practise blocks and one score for each trial-type across each pair of test blocks; (viii) four *standard trial-type* D_{IRAP} scores are computed by averaging each trial-type’s three *block-pair* D_{IRAP}s from step vii relating to test blocks; (ix) and finally, four *extended trial-type* D_{IRAP} scores may optionally be computed by averaging all four of each trial-type’s *block-pair* D_{IRAP}s from step vii.

APPENDIX 16

The Instructions for IRAP-based Perspective Switching Task in Study 3

<u>Participant Code:</u>

In this study we wish to investigate the extent to which smokers are able to pretend they are non-smokers.

Accordingly, I would like you to do your very best to **imagine yourself as a lifelong NON-SMOKER** while you are completing the following tasks. Remember, however, that to complete the following tasks properly your response-times must be as accurate and fast as possible (i.e., go fast and avoid the red X).

<u>In other words, think of yourself as a non-smoker and avoid the appearance of the red X's while also responding quickly</u>.

In your own words, what do you think you are now required to do and what strategy do you intend to use to accomplish it (if you don't know inform the researcher):

APPENDIX 17

Instructions for the Questionnaire-based Perspective Switching Task in Study 3

Participant Code:

In this study we wish to investigate the extent to which smokers are able to pretend they are non-smokers.

Accordingly, during the questionnaire to follow, I would like you to try your very best to imagine you are a **lifelong NON-SMOKER**.

In your own words, what do you think you are now required to do and what strategy do you intend to use to accomplish it (if you don't know inform the researcher):

APPENDIX 18

The Manipulation Check Questionnaires for IRAP-based Perspective Swtiching Task in Study 3

Participant Code:

Thank you for persisting this long with the task.
You are now more than half way through the study with just some written questions to complete.

To what extent do you think that you were successful in pretending to be a lifelong non-smoker during the computerised tasks? Please circle a number as appropriate on the scale directly underneath.

Not At All			Moderately			Very Much
0	1	2	3	4	5	6

Please rate below how motivated you felt to pretend that you were a lifelong non-smoker during the computerised tasks you have just completed.

Not At All			Moderately			Very Much
0	1	2	3	4	5	6

In your own words: Please fully describe HOW on the computerised tasks you attempted to adopt the perspective of a person who had never smoked.

APPENDIX 19

The Manipulation Check Questionnaires for Questionnaire-based Perspective Swtiching Task in Study 3

Participant Code:

To what extent do you think that you were successful in intentionally portraying yourself as a non-smoker on the paper questionnaires? Please circle a number as appropriate.

Not At All — **Moderately** (3) — **Very Much**

0 1 2 3 4 5 6

Please rate how motivated you felt to pretend that you were a lifelong non-smoker on the paper questionnaires you have just completed.

Not At All — **Moderately** (3) — **Very Much**

0 1 2 3 4 5 6

In your own words: Please fully describe HOW on the paper questionnaires you attempted to adopt the perspective of a person who had never smoked.

APPENDIX 20

Response Latency by Thought Suppression Analyses for Study 3

The *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} were the only two (standard) *trial-type* D_{IRAP}s in Study 3a to be impacted by the thought suppression instructions. This might have occured either by the relevant instructions leading to a speeding up of anti-smoking IRAP responses relative to pro-smoking IRAP responses; and/or by the relevant instructions leading to a slowing down of pro-smoking IRAP responses relative to anti-smoking IRAP responses. Crucially, the former eventuality would have implied that the *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} became less pro-smoking as a result of the thought suppression instructions facilitating anti-smoking responding (i.e. facilitating smokers' pre-existing anti-smoking implicit evaluating perspectives). And the latter eventuality would imply that these trial-type effects became less pro-smoking as a result of the thought suppression instructions interfering with pro-smoking responding (i.e. with smokers' pre-existing implicit evaluating perspectives). Therefore, in order to determine the extent to which the thought suppresion instructions impacted *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} via pro- versus anti-smoking IRAP response latencies we conducted two corresponding 2x2 mixed ANOVAs. Specifically, these ANOVAs respectively examined *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} in terms of crossing the IRAP effect variable (i.e. pro- versus anti-smoking IRAP response latencies) with a contrast between the control group and both perspective switching groups considered collectively.

The first ANOVA indicated that there was a moderate-to-small interaction effect between thought suppression and the IRAP effect variable on *Smoking-Pos* response latency, $F(1, 54) = 3.33$, $p = .07$, $\eta_p^2 = .06$ (i.e. $r \approx .24$), which qualified a main effect for IRAP effect $F(1, 54) = 26.98$, $p = .0001$, $\eta_p^2 = .33$, $r \approx .58$, and a null main effect for the thought suppression contrast, $F(1, 54) = 1.19$, $p = .28$, $\eta_p^2 = .02$, $r \approx .15$. Crucially, planned comparisons indicated that the thought suppressors' *Smoking-Pos* affirmation response latencies were a moderate degree slower than the control group's *Smoking-Pos* affirmation response latencies (i.e. $t(54) = -1.82$, $p = .07$, $\eta^2 = .06$, $r \approx .24$); and that by contrast, there was no such difference for the *Smoking-Pos* denial response latencies (i.e. $t(54) = -.13$, $p = .90$, $\eta^2 = .0003$, $r \approx .02$).

The second ANOVA indicated that there was a moderate-to-small interaction effect between the control versus thought suppression contrast and the IRAP effect variable on *Ban-Neg* response latency, $F(1, 54) = 1.91$, $p = .17$, $\eta_p^2 = .04$ (i.e. $r \approx .19$), which qualified main effects for both variables (i.e. respectively, *F*s = 2.89, 30.93; *p*s = .09, .0001; η_p^2s = .05, .36; *r*s ≈ .23, .60). Crucially, planned comparisons indicated that the thought suppressors' *Ban-Neg* affirmation response latencies were a moderate degree slower than the control group's *Ban-Neg* affirmation response latencies (i.e. $t(54) = -2.33$, $p = .02$, $\eta^2 = .09$, $r \approx .30$); and that by contrast, there was no such difference for the *Ban-Neg* denial response latencies (i.e. $t(54) = -1.01$, $p = .32$, $\eta^2 = .02$, $r \approx .14$). Overall, therefore, it appeared as though the thought suppression instructions made smokers' *Smoking-Pos* D_{IRAP} and *Ban-Neg* D_{IRAP} less pro-smoking only by interfering with their pro-smoking responding on these trial-types (i.e. with smokers' pre-existing implicit evaluating perspectives), rather than by facilitating their anti-smoking responding to these trial-types (e.g. by stimulating smokers' pre-existing anti-smoking implicit evaluating perspectives).

APPENDIX 21

Response Latency by Thought Suppression and Block Order Analyses for Study 3

The IRAP block order variable interacted with the pre-practise thought suppression instructions but not with the post-practise thought suppression instructions. We therefore analysed only the pre-practise group as a means of determining how their thought suppression instructions interacted with IRAP block order in terms of facilitating versus interfering with pro- versus anti-smoking responses across the four IRAP trial-types. Applying the appropriate ANOVA contrasts revealed that there was a moderately-sized interaction effect between the pre-practise thought suppression instructions and the IRAP effect variable, $F(1, 14) = .92$, $p = .35$, $\eta_p^2 = .06$ (i.e. $r \approx .25$), which qualified a large main effect for IRAP effect, $F(1, 14) = 3.69$, $p = .07$, $\eta_p^2 = .21$, $r \approx .46$, and a moderately-sized main effect for the thought suppression contrast, $F(1, 14) = 1.50$, $p = .24$, $\eta_p^2 = .10$, $r \approx .31$. Crucially, planned comparisons indicated that the pre-practise group's anti-smoking response latencies were a moderate-to-large degree faster in the pro-smoking-first block order than the anti-smoking-first block order (i.e. $t(14) = -1.52$, $p = .15$, $\eta^2 = .14$, $r \approx .38$). Indeed, the pre-practise group's pro-smoking response latencies were also moderately faster in the pro-smoking-first block order than the anti-smoking-first block order (i.e. $t(14) = -.90$, $p = .38$, $\eta^2 = .05$, $r \approx .23$). As such, given that the latter effect was actually contrary to the thought suppression instructions (i.e. and more importantly, the block order effect), it implies that the pre-practise block order effect across four IRAP trial-types was exclusively due to the pro-smoking-first block order facilitating anti-smoking IRAP responses relative to the anti-smoking-first block order.

APPENDIX 22

Response Latency by Thought Suppression and Block Order Analyses for Study 4

When the thought suppression instructions reduced the current *trial-type* D_{IRAP}s, in each case it might have occured by the relevant instructions leading to a speeding up of anti-smoking IRAP responses relative to pro-smoking IRAP responses; and/or by the relevant instructions leading to a slowing down of pro-smoking IRAP resposnes relative to anti-smoking IRAP responses. Crucially, the former eventuality would imply that the affected *trial-type* D_{IRAP}s became less mood-consistent as a result of the thought suppression instructions facilitating mood-inconsistent responding (i.e. facilitating smokers' pre-existing mood-inconsistent implicit evaluating perspectives). And the latter eventuality would imply that these trial-type effects became less mood-consistent as a result of the thought suppression instructions interfering with mood-consistent responding (i.e. with smokers' pre-existing implicit evaluating perspectives). Therefore, in order to determine whether the perspective switching instructions most impacted the relevant *trial-type* D_{IRAP}s via mood-consistent versus -inconsistent IRAP response latencies we conducted the following ANOVAs, accounting for block order wherever it made a difference.

The first ANOVA indicated that there was a moderate-to-small interaction effect between perspective switching and the IRAP effect variable on *Need-Neg* response latency, $F(1, 46) = 2.65$, $p = .11$, $\eta_p^2 = .05$ (i.e. $r \approx .23$), which qualified a large main effect for IRAP effect, $F(1, 46) = 18.37$, $p < .0001$, $\eta_p^2 = .29$, $r \approx .53$, and also a large main effect for the perspective switching contrast, $F(1, 46) = 24.10$, $p < .0001$, $\eta_p^2 = .34$, $r \approx .59$. Crucially, planned comparisons indicated that the perspective switchers' consistent *Need-Neg* response latencies were a moderate degree slower than the control group's *Need-Neg* response latencies (i.e. $t(46) = -4.70$, $p < .0001$, $\eta^2 = .32$, $r \approx .57$); and that by contrast, the relevant difference was smaller for the inconsistent *Need-Neg* response latencies (i.e. $t(46) = -3.22$, $p = .002$, $\eta^2 = .18$, $r \approx .43$).

The second ANOVA indicated that there was a moderate-to-large three-way interaction on *Need-Pos* response latencies among IRAP effect, perspective switching, and block order, $F(2, 42) = 3.45$, $p = .04$, $\eta_p^2 = .14$ (i.e. $r \approx .38$). This interaction qualified a large main effect of perspective switching, $F(2, 42) = 9.08$, $p = .0001$, $\eta_p^2 = .30$ (i.e. $r \approx .55$), and a two-way interaction between IRAP effect and perspective switching, $F(2, 42) = 4.32$, $p = .02$, $\eta_p^2 = .17$ (i.e. $r \approx .41$), but all other omnibus effects were null, Fs $\leq .84$, ps $\geq .36$, η_p^2s $\leq .02$ (i.e. rs $\leq .14$). Crucially, planned comparisons indicated that the pre- and post-practise groups' consistent *Need-Pos* response latencies were a large degree slower than the control group's *Need-Pos* response latencies in both block order conditions (i.e. respectively, $t(15)$s = -3.70, -2.60, -2.60, -5.40; ps = .002, .02, .02, .0001; η^2s = .48, .31, .31, .66; rs ≈ .69, .56, .56, .81); and that by contrast, the corresponding pre- and post-practise differences were typically moderately-sized for the inconsistent *Need-Neg* response latencies in both block orders (i.e. respectively, $t(15)$s = -.92, -2.15, -.81, -1.15; ps = .37, .05, .44, .27; η^2s = .05, .24, .04, .08; rs ≈ .23, .49, .20, .28). In other words, the perspective switching instructions appeared to impact *Need-Pos* D_{IRAP} by slowing mood-consistent (i.e. 'False') *Need-Pos* responses more than mood-inconsistent (i.e. 'True') *Need-Pos* responses (i.e. even when this impact was moderated by block order).

By contrast, the mood-consistent-first block order helped make the pre-practise group's *Need-Pos* D_{IRAP} less mood-consistent (and more pro-smoking) mainly by speeding up their mood-inconsistent IRAP responding, $t(14) = -.88$, $p = .39$; $\eta^2 = .05$, $r \approx .22$, rather than by interfering with their mood-consistent IRAP responding, $t(14) = .57$, $p = .58$; $\eta^2 = .02$, $r \approx .15$ (i.e. the relevant interaction was moderately-sized, $F(1, 14) = 2.02$, $p = .18$, $\eta_p^2 = .13$, $r = .36$). However, conversely, the mood-inconsistent-first block order helped make the post-practise group's *Need-Pos* D_{IRAP} less mood-consistent (and more pro-smoking) mainly by interfering with their mood-consistent IRAP responding, $t(14) = -1.78$, $p = .10$; $\eta^2 = .18$, $r \approx .43$, rather than by speeding up their mood-inconsistent IRAP responding, $t(14) = .62$, $p = .54$; $\eta^2 = .03$, $r \approx .16$ (i.e. the relevant interaction was moderately-sized, $F(1, 14) = 4.00$, $p = .07$, $\eta_p^2 = .22$, $r = .47$).

The third and final ANOVA indicated that there was a moderate two-way interaction on *Enjoy-Neg* response latencies both between IRAP effect and perspective switching, $F(2, 42) = 2.08$, $p = .14$, $\eta_p^2 = .09$ (i.e. $r \approx .30$); and also between perspective switching and block order, $F(2, 42) = 1.68$, $p = .20$, $\eta_p^2 = .07$ (i.e. $r \approx .27$). These interactions qualified a main effect of perspective switching, $F(2, 42) = 14.09$, $p < .0001$, $\eta_p^2 = .40$ (i.e. $r \approx .63$), but all other main and interaction effects were null, Fs $\leq .64$, ps $\geq .43$, η_p^2s $\leq .01$ (i.e. rs $\leq .12$). Crucially, planned comparisons indicated that the pre- and post-practise groups' consistent *Enjoy-Neg* response latencies tended to be slower than than the control group's *Enjoy-Neg* response latencies, (i.e. respectively, $t(15)$s = -2.66, -5.23, -.87, -4.62; ps = .02, .0001, .40, .0003; η^2s = .32, .65, .04, .58; rs ≈

.57, .80, .20, .77), to a consistently greater degree than the consistent *Enjoy-Neg* response latencies were (i.e. respectively, $t(15)$s = -2.83, -3.04, -.05, -2.53; ps = .01, .01, .96, .02; η^2s = .35, .38, .0002, .30; rs ≈ .59, .62, .01, .55). In other words, crucially, the perspective switching instructions appeared to impact *Enjoy-Neg* D_{IRAP} by slowing mood-consistent *Enjoy-Neg* responses more than mood-inconsistent *Enjoy-Neg* responses (i.e. even when this impact was moderated by block order).

Indeed, likewise (and much like the post-practise mood-inconsistent-first block order for *Need-Pos* D_{IRAP}), the post-practise mood-consistent-first block order helped make *Enjoy-Neg* D_{IRAP} less mood-consistent (and more pro-smoking) mainly by interfering with their mood-consistent IRAP responding, $t(14) = -2.01$, $p = .06$; $\eta^2 = .22$, $r \approx .47$, rather than by speeding up their mood-inconsistent IRAP responding which were actually moderately slower than the mood-consistent-first pre-practise group, $t(14) = .93$, $p = .37$; $\eta^2 = .06$, $r \approx .24$ (i.e. the relevant interaction was moderately-sized, $F(1, 14) = 1.34$, $p = .27$, $\eta_p^2 = .25$, $r = .50$). Overall, therefore, it appeared as though the perspective switching instructions made their impact on the current *trial-type* D_{IRAP}s by interfering with mood-consistent responding (i.e. with smokers' pre-existing implicit evaluating perspectives), rather than by facilitating mood-inconsistent responding. And that, block order operated differently depending upon not just perspective switching, but also the particular *trial-type* D_{IRAP} involved. Namely, whereas the pre-practise block order effect on *Need-Pos* D_{IRAP} occurred by facilitating mood-inconsistent IRAP responding, both post-practise block order effects (i.e. on *Need-Pos* D_{IRAP} and *Enjoy-Neg* D_{IRAP}) occurred by interfering with mood-consistent IRAP responding.

APPENDIX 23

A Thought-listing Questionnaire for Musical Induction of Sad Moods

Participant Code (5): __________

You are about to watch a series of pictures of landscapes while Alblnoni's Adagio in G Minor plays in the background. This will last for seven minutes. **Your task** is to reflect upon events and things in your life that have made you feel *"sad, frustrated, anxious, worried or even angry."*

During this time you should write about your reflections in detail. Please list each of the *thoughts*, *opinions*, *memories* or *feelings* that passed through your mind as you looked at the landscapes and listened to the music. You should list them one-at-a-time in each of boxes provided for you below; each time please rate the experience using a number from the scale below. If for example a thought passes through you mind, then something else, and then later the original thought again, you should record the second occurrence of the original thought *again* in the sequence you had it in. Simply write down the first thing that passes through your mind in the first box, the second in the second box, etc. If you run out of paper please ask the researcher for more.

You can ignore spelling, grammar, and punctuation as long as it is clear what you mean – the main thing is to briefly summarise your experiences as they arrive. Please be completely honest. Remember that everything you write is anonymous – it is important for the success of the study that you summarise all of the mental experiences that arise for you while listening to the music *whether you think them relevant or not*.

For each experience that you list, please choose a number from the following scale that best describes the experience; write the number in the "Feeling rating" column beside where you listed your experience.

Negative feeling *Neutral* *Positive feeling*

-3 -2 -1 0 1 2 3

Memory descriptions	**Feeling rating**

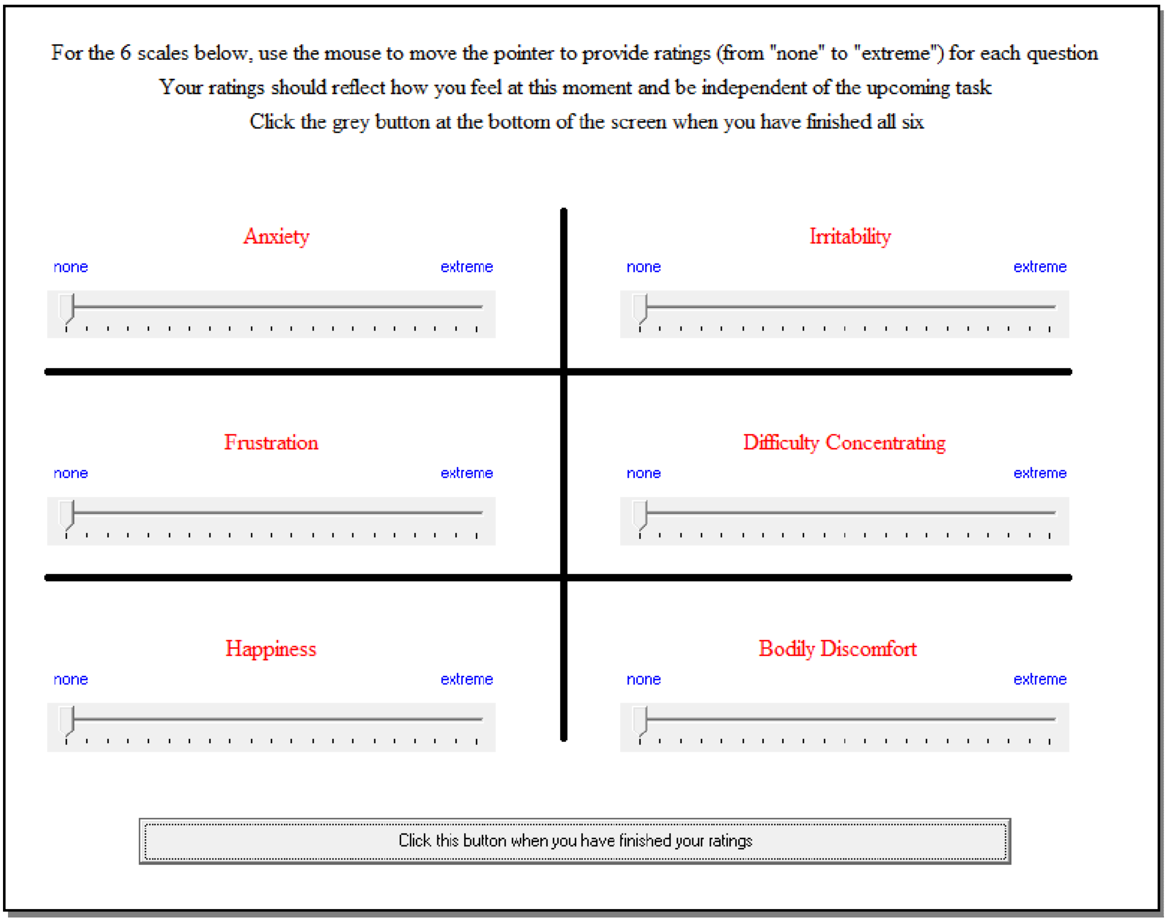
Participant Code (5): __________

Participant Code (5): __________

If you would like more paper please ask the researcher.
As long as you continue to re-read the message it is important to continue recording your experiences.

APPENDIX 24

PASAT-C Mood Induction Manipulation Check Rating Scales



APPENDIX 25

The Demographic and Behavioural History Questionnaire Version 3 (DBHQ-3)

Background Questionnaire

To be filled out by the researcher – Take notes to supplement answers where it would provide more information regarding smoking-status.

1. What is your cultural ethnicity, date of birth, sex, current occupation, and highest level of education (second-level; third-level, etc.)?

2. What time was your last cigarette at? (take note of time at intake & also current time & how long slept during that period)

3. How old were you when you first started smoking cigarettes FAIRLY REGULARLY? (enter "X" if never smoked regularly)

4. How old were you when you first started to buy cigarettes FAIRLY REGULARLY? (enter "X" if never smoked regularly)

5. Do you *buy* cigarettes on a regular basis (on more days than not)?

6. On how many of the past 30 days did you smoke cigarettes?

   0-10 days 10-20 days 20-30 days 30 days

7. During the past 30 days, on the days you smoked how many cigarettes do you think you smoked on average each day?

   Between ___ and ___ per day.

8. Are you currently smoking as usual, or more or less cigarettes per day than is usual for you?

Much Less than Usual			As Usual			Much more than Usual
-3	-2	-1	0	1	2	3

Please give details of daily cigarette consumption *CURRENTLY* versus *USUALLY* (get ranges as before) and reasons for any differences (e.g. just money or because of quitting concerns?; extrinsic versus intrinsic):

9. How long ago did your most recent attempt to QUIT smoking *START*?
   (Get participant to provide their best recall of date when they relapsed and calculate how long ago this was)

   During this quit attempt how long were you able to quit for?

10. When did you last try to REDUCE how much you smoked?
(Get participant to provide their best recall how long ago it was when they started this attempt)

During this attempt to reduce the number of cigarettes you smoke, how long were you able to maintain the reduction in your cigarette consumption?

11. How many times in the past 12 months have you made what you would consider a "serious" attempt to quit smoking?

Between _______ and _______ times

12. How many times in your life have you made what you would consider a "serious" attempt to quit smoking?

Between _______ and _______ times

13. In the past 12 months, how many times have you quit smoking for at least 24 hours?

Between _______ and _______ times

14. How many times in your life have you quit smoking for at least 24 hours?

Between _______ and _______ times

15. What information resource or professional support have you previously used to help you stop smoking? Please give details of type and length of support, how well it worked, and how long ago