Nudging, transparency and rationality: An experimental investigation

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Abstract

This thesis is made up of three Chapters. In the first chapter, "Happy, healthy, wealthy and rational: Are biases harmful?" I conduct a quasi-experimental survey to investigate whether or not five biases and fallacies (present bias, sunk cost fallacy, loss aversion, gambler's fallacy and impact bias) affect the likelihood of depression, of an individual participating in socially (un)desirable behaviors and whether or not they are associated with lower incomes. Out of the five biases investigated three are linked to lower incomes, but only one to a higher likelihood of depression.

In the second chapter, "The Marginal Benefit of Manipulation: Investigating paternalistic interventions in the context of intertemporal choice", I conduct an experiment to determine to what degree a traditional libertarian paternalist (LP) intervention, popularly known as a nudge, can outperform an autonomy-enhancing paternalist intervention (AEP). I introduce the term Marginal Benefit of Manipulation, MBoM, defined as the difference in treatment effect between an LP and AEP intervention. I find that the AEP intervention completely failed to alter behavior, but while the LP intervention fares better at first, it tapers off towards the end of the survey and the treatment effect becomes insignificant.

In the third chapter, "The Marginal Cost of Transparency: Do honest nudges work?", I conduct another experiment, this time to determine the effect that transparency has on the efficacy of a nudge. I introduce the term Marginal Cost of Transparency, MCoT, defined as the difference in treatment effect between a libertarian paternalist intervention (LP) and what I call a transparent libertarian paternalist (TLP) intervention, a type of LP intervention where consumers are made aware of the nudge and why it is there. My results indicate that the MCoT is, with few exceptions,

not statistically different from zero and that the answer to the question “Do honest nudges work?” is Yes. Furthermore, the results indicate that autonomy-enhancing paternalism fares at least as well provided participants are paying full attention.

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This dissertation is dedicated to my son Timothy Gustavsson. May you always retain your curiosity and your stubbornness, and may you always speak truth to power.

Introduction

1. Motivation

Behavioral economics has moved from being outré to being accepted and mainstream. However, there are features of behavioral economics that have perhaps been accepted all too readily and need to be critically examined. Among these are the use of behavioral nudges to alter consumer behavior to match what the choice architect views as more consistent with individual maximization and the identification of behavioral biases that, purportedly, reduce individual welfare. In this thesis, using survey analysis, the impact of behavioral biases on individual wellbeing is explored in the first chapter and the ethics and efficacy of nudging are explored in subsequent chapters.

A central assumption of economics is that agents are rational and that the neoclassical model of individual utility maximization is the gold standard for evaluating individual behavior. Any behavior deviating from this standard is deemed a bias and is thus irrational. Such biased behaviors should lead to a reduction in individual wellbeing, and, as such, should be corrected. However, as discussed by Berg and Gigerenzer (2010), very little evidence exists that irrationality, as defined by neoclassical economics, does in fact reduce utility. This is a critical shortcoming, as behavioral economists frequently justify their models by promising, or at least aspiring to, improved empirical realism (Camerer 1999, 2003; Rabin, 1998, 2002 and Thaler, 1991). Truly realistic models of consumer behavior should be expected to identify not just how consumers differ from the neoclassical assumptions, but whether and to what extent these differences make them worse off. Parker and Fischhoff (2005) did find a negative relationship between Decision-Making Capacity

(DMC) and anti-social, high-risk behavior, and as those with high DMC scores on average were less biased, this may seem to vindicate the neoclassical model. However, the study suffered from several shortcomings. First, the sample size was very small with only 110 participants. Second, the sample was made up only of 18- and 19-year old men, putting the external validity of the study into doubt, as there is no way to know whether results can be extrapolated to other age groups or to women. Finally, as economics was not the focus of the study, no proxy for utility was used and the study did not investigate the link between biases and income. As debiasing techniques become more sophisticated and able to permanently change behavior (Morewedge et al., 2015), it is critical to understand which biases actually reduce utility. Otherwise, behavioral economists run the risk of "correcting" biases which may be harmless or even beneficial, something that would undoubtedly undermine trust in the field among policy-makers as well as the general public. In Chapter 1, "Happy, healthy, wealthy and rational: Are biases harmful?", a quasi-experimental online survey is developed and implemented specifically to determine the link, should it exist, between biases and depression, income and unemployment, as well as socially destructive behaviors such as alcohol overconsumption, drug use, smoking, obesity/Type II diabetes and socially desirable behaviors such as budgeting and saving.

Behavioral biases also provide the rational for nudging. Nudging, also referred to as libertarian paternalism (LP), is an application of behavioral economics developed by Richard Thaler and Cass Sunstein that has become more and more common in recent years. Local and national governments as well as private businesses have used nudges to reach their policy, regulatory or profit goals. Thaler and Sunstein (2008) defined a nudge as "any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives.”

Despite promising results, many have raised ethical concerns with nudging. Critics allege that nudging is a short-term solution that may make the problem of poor decision-making worse in the long run as consumers come to rely on nudges to make good decisions thus spend less time and energy on obtaining information (Klick and Mitchell, 2006). Nudging has been criticized for the lack of transparency surrounding its use (Binder and Lades, 2015) and the inability for consumers to hold choice architects to account. As nudging has become more widespread, these ethical concerns are no longer just hypothetical, but have become real world issues.

More prosaically, nudging can be criticized for not raising revenue, unlike sin taxes. Even if a nudge is relatively effective at preventing an undesirable consumer behavior, it will not raise revenue, as do sin taxes, that can be used to treat the negative effects stemming from those consumers who still do engage in that behavior.

Nudges do little to deal with the underlying causes of faulty consumer behavior (O’Brien, 2011). Whereas proponents of libertarian paternalism tend to view bad decisions as resulting from psychological biases and heuristics, many of these decisions may stem from poor socioeconomic backgrounds, discrimination and environments that reinforce racial stereotypes. For example, high school students belonging to an ethnic minority may choose not to apply for college, not because they are suffering from a present bias that prevents them from valuing the long-term benefits that they will gain from higher education, but rather due to lack of educated role models, encouragement from teachers and career guidance counselors, or fear of not being evaluated fairly by the university admission offices. Nudging students in this situation would “camouflage” rather than deal with the underlying issues that they face.

Autonomy-enhancing paternalism (AEP) is an attempt to rectify several of the issues associated with nudging: The goal of these interventions is to make consumers more informed and/or provide them with more time to make a decision, or to reverse a decision they have made, which will improve their ability to make right decision. The effects of these interventions may have a greater potential of permanently altering consumer behavior by augmenting the information set (Rogers and Frey, 2014). Since AEP interventions happen in the open, there is little issue with transparency and the ability of consumers to hold policymakers to account for these interventions.

AEP, however, is not itself immune to criticism: AEP can be opposed on the basis that it still is a form of government intervention that assumes that policymakers know what information consumers need to make decisions or how much time they need to consider their decisions before they make them. AEP interventions have also been criticized for promoting stigmas. In particular, calorie labels on restaurant menus may promote the shaming of overweight and obese individuals and may possibly worsen the condition of those struggling with eating disorders, such as anorexia (Maughan, 2018).

Transparent nudges (TLP: Transparent libertarian paternalism) are an alternative to both traditional, hidden nudges and AEP interventions. TLP utilizes normal nudges but includes disclaimers to consumers letting them know that they are being nudged, how, by whom, and for what purpose. TLP resolves many of the ethical issues with nudging, in particular the lack of transparency and accountability of responsible policymakers. This makes it a valid option, in particular in cases where AEP interventions are unfeasible or proven to be ineffective. It should be noted, however, that TLP interventions do not mitigate the issue of nudges having only a temporary effect, and by extension the issue of nudges causing learned helplessness (Klick and Mitchell, 2006).

In “The Marginal Benefit of Manipulation: Investigating paternalistic interventions in the context of intertemporal choice”, intertemporal choice and nudging to alter it is examined. Understanding intertemporal choice and how to alter intertemporal choices is vital in situations where consumers must focus on the long-term effects of their decisions rather than just considering the short-term benefits. This is true with respect to global warming, but it also the case with respect to diet choices leading to obesity (Komlos, Smith and Bogin, 2004), smoking (Fersterer and Winter-Ebmer, 2003), alcoholism (Petry, 2001), drug use (Kirby and Petry, 2004) and cognitive ability (Shamosh and Gray, 2008). While it is far from certain that a causal link exists between high discount rates and each of these conditions and behaviors, enough evidence exists implicating high discount rates that it is worthwhile to consider how they can be lowered. To do this and to investigate if there is a more ethical way to nudge, an experimental online survey is developed and implemented to determine the “marginal benefit of manipulation”, defined as the difference in treatment effect between a libertarian paternalist (LP) intervention (popularly known as a nudge) and an autonomy-enhancing paternalist (AEP) intervention. This is the first study to directly compare these two types of interventions in an experimental setting and is one of relatively few studies to use a mixed sample rather than exclusively students or exclusively non-students.

In “The Marginal Cost of Transparency: Do honest nudges work?”, an experimental online survey is developed and implemented to determine the effect that transparency has on the efficacy of a nudge. This is the “marginal cost of transparency”, defined as the difference in treatment between a hidden nudge and a transparent nudge.

Whereas Chapter 2 focuses on the difference between a nudge and another type of behavioral intervention, Chapter 3 focuses on the effect that transparency has on the efficacy of a nudge. Few previous studies had investigated this, and none had done so in the context of intertemporal choice. This study also employed a much stricter definition of transparency than previous studies, in which the nudged participant must be aware of not just that they are being nudged, but how, for what end and by whom.

By varying the size and form of the payoff, vouchers or money, the experiments captured the magnitude effect documented by Thaler (1981) and investigated how the form of the payoff interacts with the interventions. This difference would prove important as the third Chapter found that the efficacy of a treatment is partially dependent on the size of a reward (AEP interventions work better when rewards are large).

The interventions tested in the second and third chapters, while similar, are distinct. In the second chapter, the AEP intervention was “neutral”, consisting of a list of arguments in favor of the larger-later *and* a list of arguments in favor of the smaller-sooner options. In the third chapter the list was made up only of arguments in favor of the larger-later option. This change had a great impact on the efficacy of the AEP intervention which frequently turned out to be significant in the third chapter but not in the second. Taken together, this suggests that AEP can work, but also that there are limits to how “neutral” a policymaker can be and still expect results from an intervention.

In the third chapter, with its large sample size, it was possible to differentiate who paid full attention and those who paid only some attention to the experiment. This was determined by their answers to the attention-measuring questions. Substantial differences in the treatment effects were found between these two samples, with the AEP intervention being drastically more effective among those who paid full attention. Differences in treatment effect were also found between those participants with only a high school diploma or less, and those with higher education. No previous studies have found, or even investigated, either of these areas.

2. Methodology

To elicit preferences and determine the effect of various interventions on consumer behavior, as well as investigate the link between biases and financial well-being and mental health, this thesis uses online experiments. This raises three overarching methodological issues: First, the use and abuse of student-only subjects in experiments. Second, the design and reliability of online surveys. Thirdly, the use of incentives in experimental economics and in particular the impact of real incentives in intertemporal choice experiments.

2.1 The use of student subjects in social scientific research

The experiments that are the basis of this thesis included both student and non-student participants. This differs from most economic experiments which rely entirely on student subjects. This is a rather controversial topic in behavioral economics, with Bardsley et al. (2009) arguing in their book that, since economic theories make no exception for students, it is acceptable to test economic theories using students. Thus, if students display discount rates of a different structure than predicted by the discounted utility model (Samuelson, 1936), then the discounted utility model can be considered not to have behavioral validity.

Gächter (2010) argues on a similar basis that using students is sufficient to test economic theories, further arguing that students are the perfect subject pool as they are on average intelligent and used to learning. While this characterization of students may be correct, it is debatable whether this makes students a better subject pool, as these things distinguish them from the overall adult population. Gächter does concede that the optimal subject pool depends on the specific research question but argues that students should be used as a benchmark.

For the purposes of testing the assumptions of neoclassical economic theory, student samples are technically sufficient, but this is far from the only type of experimental economic research where student samples are used. As behavioral economics moves on from testing neoclassical assumptions to testing the feasibility of policy, the methodology needs to adapt and move from students to the general adult population.

Gächter further argues that replicability is improved when student-only samples are used, as students are easily available and other researchers can run the same experiment with their own universities' students to verify the results. In the pre-Internet world, this argument certainly held merit, but today adults can be reached and allowed to participate in experiments over the internet. Even experiments that cannot be conducted online can none the less utilize the internet for recruitment, making it easier to reach non-student adults.

In Chapter 1, while a student-only sample would have been possible, it would not have allowed the investigation of a link between income and biases. While students do have income, their income is less likely to depend on their cognitive abilities or lack thereof, and more likely to depend on the wealth of their parents, their ability to get stipends and the student loan and grant system in the country where they live.

Hooghe et al. (2010) noted further that in addition to student-only samples not including older individuals, such samples also do not include young people who are not attending college. As they show through three different experiments, interventions that are effective with undergraduate students may not be effective at same-aged peers who are working or unemployed.

Experiments in economics which have included students alongside other adults have had mixed results. Fehr and List (2004) found that CEOs, when playing two versions of a trust game, exhibited greater degree of trust, and a greater degree of trustworthiness, than students playing the same games. Cadsby and Maynes (1998), in a public goods game, found that nurses acted with a greater degree of co-operation than students.

On the other hand, DeJong, Forsythe and Uecker (1988), in a principal-agent experiment, found no difference in prices, quality of service or average profits between students and professionals. Dyer, Kagel and Levin (1989) found that professionals exhibit similar levels of winner's curse as students in an auction game. Fréchette (2015), in reviewing 13 experimental economic studies that used mixed samples (students and professionals) found that the difference between students and professionals was statistically insignificant in nine of these studies.

Unfortunately, none of the experiments comparing students and professionals have dealt with either nudges or intertemporal choice. It is known, however, that discount rates are negatively correlated with age and continue to decline into adulthood (Green, Fry and Myerson, 1994). It therefore seems intuitively reasonable that college students may be less patient than older adults. Whether or not this means that paternalistic interventions aimed at reducing the discount rate may affect students differently cannot be inferred, but as an act of caution, all adults, rather than just students, were invited to take part in the experiments.

2.2 On the use and reliability of online surveys

Since the advent of the internet, social scientists have debated how to best conduct online surveys, and the advantages and disadvantages of conducting surveys and experiments online.

Couper, Traugott and Lamias (2001) conducted three experiments on the effects of survey design on the responses and the response rate of a survey. They found that having multiple questions on the same screen significantly reduced the time it took to complete the survey and reduced cases of missing data, but that this did not significantly affect the responses to the questions themselves. Because of this, the surveys that form the foundations of this thesis generally grouped several questions together on the same page. Furthermore, they found that allowing participants to answer a web survey by only using their computer mouses, instead of having to type in their responses, significantly reduced the number of participants answering “Don’t know” to the questions. In the context of this thesis, this supports the choice of choice tasks in which the participant simply clicks on a button to choose an option (larger-later or smaller-sooner), as opposed to matching tasks which would have required participants to type the larger-later reward that would be the least they would be willing to accept in exchange for forgoing the smaller-sooner reward. While a drop-down menu with options could be used to allow participants to solve matching tasks without typing, these options could have had an anchoring effect which would have biased the responses.

Galesic and Bosnjak (2009) conducted an experiment investigating the impact that the length of an online survey has on participation and completion rates as well as data quality. They found a negative relationship between the length of the survey and the participation and completion rates. They also found that questions asked later in a survey receive shorter and more uniform answers. For this reason, the surveys in this thesis were kept relatively short, with a comparatively low number of tasks compared to most other economic experiments. Furthermore, the intertemporal choice tasks in the second and third chapter were placed at the beginning of the survey, to improve chances that participants would pay attention and not be tired or lack concentration due to having already answered other questions. In the first chapter, the tasks intended to discern biases were intermingled with demographic and attention-measuring questions, but all tasks were none the less in the first part of the survey. The relationship between the length of a survey and the response rate has also been documented in studies on mailed surveys (Burchell and Marsh, 1992; Jepson et al., 2005).

All three surveys were designed to allow as much anonymity as possible. This was particularly the case for the survey that formed the basis for the first chapter as it asked highly sensitive questions about depression, drug and alcohol consumption and several other taboo topics. Anonymity has been shown to affect the responses given to surveys on alcohol, tobacco and drugs at least among teenagers (Bjarnason and Adalbjarnardottir, 2000), and it is intuitively likely that adults too may answer such questions differently unless anonymity could be guaranteed. For this reason, this survey did not offer any real incentives, as real incentives would have required participants to disclose their contact information in order to be able to receive the incentive. While there are advantages associated with using real incentives (see below for a longer discussion on the topic), the disadvantage of not being able to offer fully anonymous participation was deemed to be greater.

Baatard (2012) and Kaczmirek (2005) recommend that longer surveys allow participants to save and continue where they left off. In the context of this thesis, this would apply mainly to the survey detailed in the first Chapter, which was significantly longer than the surveys in Chapters 2 and 3. There were two reasons why this option was not pursued. First, just like real incentives, offering participants a chance to save and come back to complete the survey later would almost certainly involve violating the anonymity of the participants in question, as they would have to disclose their email address to which a link to the part of the survey where they left off could be sent. Secondly and most importantly, the external validity of the survey would have suffered if participants had had the opportunity to close down the survey and look for information about the questions (to ensure that they gave the “right” answers) before proceeding.

Online surveys are generally accepted to reduce the experimenter effect. The term, coined by Kintz et al. (1965), refers to the influence that the presence of and interactions with an experimenter has on the effect of participants in a study. The experimenter effect can bias participants into acting unnaturally, reducing the external validity of the experiment, and has been observed in economic experiments (Weiss, O’Mahony and Wichchukit, 2010). The anonymity of online surveys and experiments is believed to reduce this effect (Denissen, Neumann and Van Zalk, 2010). No economic experiments have investigated whether the experimenter effect is reduced when experiments are conducted online. However, studies from other behavioral fields have shown that individuals are more prone to reveal sensitive information to computers (“virtual humans”) than to human beings (Pickard, Roster and Chen, 2016; Lucas et al., 2014). Another study (Pickard et al., 2019) found that employees are more likely to admit to violating internal controls when interviewed by a virtual interviewer as opposed to a human, even when the human in question has significant interviewing experience.

Online surveys also allow for significantly greater (in theory unlimited) sample sizes. Whereas sample sizes in lab experiments are limited by the size of the room where the experiment takes place, online experiments face no such limitation. This is a particularly great advantage when conducting research where the dependent variable is binary, as binary variables generally require greater sample sizes to analyze than continuous variables. Field experiments also allow for big sample sizes, but often require the experimenter to be physically present at the place where the experiment takes place in order to collect data, and as discussed by List (2011) many field experiments fail due to insufficient sample sizes. Larger sample sizes allow for comparison of subgroups which can yield greater insights into economic behavior in particular groups (such as people between a certain age). As noted by Zhang and Ortmann (2013), economic experiments typically utilize small sample sizes that only allow for non-parametric testing of the data. Small sample sizes are also a likely reason behind the low replication rates in experimental economics. A review of 85 replications found that only 42.3 % successfully replicated the results of another experiment (Maniadis, Tufano and List, 2017).

These advantages, however, mean little unless the data collected through online surveys is valid. While there are no experimental economic studies comparing the behavior of participants in online surveys to those in a traditional lab experiment, several such studies do exist in psychology and other behavioral sciences. Meyerson and Tryon (2003), using an online survey with a demographically identical sample as a previous, in-person study on the Sexual Boredom Scale and five validation scales, found extremely similar results to the previous study for all six scales. In a literature review, Gosling et al. (2004) found that results from surveys and experiments conducted on the internet were consistent with results from lab experiments and in-person surveys.

Other researchers, rather than attempting to replicate the results of past lab experiments using online experiments, have conducted experiments in which one part of the sample took part online, and the other in a lab. Doing this allows for the researcher to compare to what extent the behavior of participants in the online group differ from the "baseline" group of participants who are taking part in the experiment in person. A study on syllogistic reasoning (Musch and Klauer, 2002) found very similar results between the online and lab experiment participants, as did a study on self-trust (Pasveer and Ellard, 1998).

In conclusion, it is very likely that internet surveys and experiments induce behavior at least as natural as that induced by lab experiments, while simultaneously allowing for greater sample sizes.

2.3 On the use of real incentives

In experimental economics over the past several decades, the use of real incentives has become the norm. The use of real incentives is generally agreed to cause participants in experiments to act as they would have in real life, rather than to make choices according to what they believe the experimenter wants them to do or what they believe will make them look good. In addition, real incentives make participating in experiments more attractive, making it easier for experimenters to find participants.

One issue with real incentives is that participants' decision-making may shift as they earn money through the course of the experiment. That is, a participant on the third task of an experiment who has already earned $20 on the first two tasks may act differently than a participant who has not earned anything on the first two tasks. Random lottery incentives are an attempt to solve this problem. Instead of participants being paid based on every task in the experiment, they are paid based on one, randomly selected task. Since participants do not know which task their pay will be based on, they have no way of estimating how much they have earned mid-way through an experiment, meaning this cannot influence their behavior.

Random lottery incentive experiments are, however, not without critics. Smith (1982) argues that valid economic experiments must have incentives high enough to dominate any subjective costs and benefits to the subject that stem from participating in the experiment. Harrison (1994) further argues that since the average payment per task is typically very low in such experiments, that participants are likely to commit errors when solving tasks, and that results from random lottery incentive experiments thus will be biased towards the option that is most likely to result from errors. Wilcox (1993), in a similar vein, hypothesized that the greater the dilution of rewards, the less accurate would the heuristics used by participants in an experiment be. Cubitt, Starmer and Sugden (1998) defended random lottery incentives. In a series of three experiments, they found no evidence of bias in random lottery incentive experiments compared to single choice incentives in which a participant faces only one task and knows that the reward for this single task is real. Violations of expected utility theory were not found to be more common in random lottery incentive experiments than under single choice conditions.

In the specific context of intertemporal choice, which is the focus of two of the three Chapters of this thesis, there is little-to-no evidence suggesting that incentives matter at all. Frederick, Loewenstein and O’Donoghue (2002) in reviewing the literature on intertemporal choice experiments found no clear evidence that results from experiments which utilized real incentives differed from those that did not. Coller and Williams (1999) conducted an intertemporal choice experiment in which real incentives were used for one group, and hypothetical incentives for the other. The results were the same. Abdellaoui, Bleichrodt and l'Haridon (2013) conducted a similar experiment, and likewise found that results were overall similar between hypothetical and real participant groups. Chapter 2 of this thesis also compared the choices of participants who had provided their email address with those who had not. Participants needed to provide their email address in order to have a chance to be paid. If real incentives had an impact on intertemporal choices, one would expect there to be a difference between the choices of participants who provided their email addresses and thus had a chance to be paid, and those who did not. No such difference was found for any task.

In what is perhaps the most important study on this topic, Bickel et al. (2009) used neuroimaging to test the response of participants solving intertemporal choice tasks involving real and hypothetical gains, as well as hypothetical losses. They found no within-subject difference between these conditions, again reinforcing the idea that real incentives are not strictly necessary when conducting experiments on intertemporal choice.

Finally, exiting an online experiment is rather easy and does not involve any type of confrontation, or even interaction, with the experimenter. Thus, if subjects feel that the experiment is not worth their time in light of the lack of incentives, or that the lack of incentives makes it impossible for them to take the tasks seriously and know how they would act, they are likely to simply quit the experiment rather than stay and complete the tasks in an unnatural manner.

3. Key findings

This thesis uses experimental and quasi-experimental online surveys to examine key issues in behavioral economics: whether biases are indeed harmful and whether nudges can be both ethical and effective. The findings are important and improve our knowledge and understanding of behavioral economics and the role it plays in practical policy making.

In Chapter 1, “Happy, healthy, wealthy and rational: Are biases harmful?”, a quasi-experimental online survey is implemented to investigate which, if any, of the five biases tested are harmful to consumers financial well-being or mental health. Only one of the biases is associated with a higher risk of depression, while three are associated with a lower income.

In Chapter 2, “The Marginal Benefit of Manipulation: Investigating paternalistic interventions in the context of intertemporal choice”, an experimental online survey is carried out to investigate whether a libertarian paternalist intervention (nudge) is superior to an autonomy-enhancing paternalist intervention. A follow-up survey investigates whether either intervention has any permanent effect on behavior. The LP intervention is initially effective, but the treatment effect tapers off later in the survey. The AEP intervention turns out to be ineffective. Neither intervention has any permanent effect.

In Chapter 3, “The Marginal Cost of Transparency: Do honest nudges work?”, an experimental online survey is carried out to investigate the effect that transparency has on the efficacy of nudges. Parallel to this, the experiment investigates the effect of a stronger AEP intervention than the one investigated in the second Chapter. The results indicate that nudges can be effective even when transparent, and may, in fact, be more effective than hidden nudges provided that consumers exposed to them are paying full attention. The AEP intervention also proves to be effective among participants paying full attention.

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Happy, healthy, wealthy and rational: Are biases harmful?

Abstract

Behavioral economists have over the years discovered a great number of cognitive biases and fallacies in human decision-making that violate the neoclassical model of consumer behavior (popularly known as Homo Economicus). However, behavioral economists have, largely, failed to provide evidence that these violations actually reduce utility in any significant way. This paper presents the results of a quasi-experimental survey designed to determine whether non-neoclassical ("irrational") consumer behavior can be linked to a reduction in income, a higher risk of depression, or, possibly, to unhealthy risky behaviors such as smoking or drug use. Out of the five biases investigated, only one is associated with a higher risk of depression, while three biases are associated with lower earnings. Biases are not, in general, linked to unhealthy or risky behaviors. Overall, biases, even when significant, explain little and regressions across the board have very low explanatory power.

1. Introduction

Since its introduction as a sub-field of economics, behavioral economics has identified a great number of biases, heuristics and other deviations from the neoclassical model of consumer behavior. Experimental and empirical data have challenged everything from the neoclassical model of time discounting (Frederick, O'Donoghue and Loewenstein, 2002) to more basic ideas such as transitive preferences (Loomes and Taylor, 1992).

These discoveries have been followed by various proposals for policy interventions and "debiasing" measures. Historically most debiasing measures that have been attempted have focused on one bias in one setting and have failed to permanently alter behavior. Recently, this has begun to change: Morewedge et al. (2015) used a set of computer games and instructional videos to reduce six biases with an immediate reduction in erroneous decision-making of 30 percent and a reduction of 20 percent three months after the intervention took place.

Nudging, another well-known form of debiasing, whereby a 'choice architect' (typically a government official) changes the framing and/or presentation of a choice in a manner intended to induce the consumer to make choices that the architect deems to be better for the consumer (Thaler and Sunstein, 2003). For example, a choice architect may attempt to combat obesity by redesigning cafeterias to make the unhealthy foods less visible and/or accessible. Proponents of nudging defend this type of "manipulation" by arguing that they are merely undoing the harms caused by biases. The present bias for instance may cause an individual to overconsume unhealthy foods, and the nudge simply undoes the evil influence of the present bias. Thus, the nudge is justified as it, in effect, leads to consumers behaving in a manner closer to how they should behave if biases did not exist.

However, as Berg and Gigerenzer (2010) point out, by prescribing policy interventions to 'cure' irrational behavior, behavioral economists implicitly admit that while the neoclassical model of human behavior may be deeply flawed, it is none the less a model worth striving for. The neoclassical model, then, becomes something of a perhaps unachievable yet still admirable ideal. However, very little data supports the idea that consumers whose behavior is closer to the neoclassical model are happier, wealthier or less prone to socially undesirable behaviors such as drug or alcohol abuse. In fact, some studies even indicate the opposite may be true. In an experiment, Berg, Eckel and Johnson (2009) found time-inconsistent consumers and consumers who violated the Expected Utility model to have higher expected earnings.

Since debiasing was developed as a concept, researchers have looked for ways to permanently reduce biases broadly as opposed to only temporarily in specific settings (Nisbett et al., 1987). However, as debiasing measures become more effective at broadly reducing biases permanently, it becomes more important to understand whether such reductions do in fact benefit individuals, and if not whether they at least benefit society as a whole. If in the future we were to develop a button that if pushed would eliminate every bias and other deviation from the neoclassical model, should we choose to push it?

Parker and Fischhoff (2005) conducted a longitudinal study which tested the relationship between decision-making capacity (DMC) and a range of social factors. Participants were asked questions that tested for among other things sensitivity to framing, the sunk cost fallacy and present bias, from which a decision-making capacity score was then calculated with a higher score indicating a better ability to make decisions. The authors found a negative relationship between DMC scores and anti-social, high-risk behaviors (such as drug and alcohol abuse), although notably the ability to resist falling victim to the sunk cost fallacy was found to be irrelevant. As those with high DMC scores were, at least on average, less biased, this may be seen as a vindication of the neoclassical model. However, this study suffered from several shortfalls. First, the sample size was only 110. Second, all participants were males, aged between 18 and 19, which makes the external validity highly questionable. Behavior, including high-risk behavior, may still change after the age of 18, and 'flawed' decision-making processes that cause harm at the age of 18 may still be beneficial further down the line. Third, as the study did not mainly focus on economics, the authors did not include any proxy for utility (no depression/happiness score) nor did they look at income.

In this paper I conduct an experiment with the goal of establishing whether or not deviating from the behavior predicted by the neoclassical model hurts consumers individually and/or society as a whole. An important distinction needs to be made between these: It is virtually impossible to know whether a behavior is in fact irrational for each individual consumer who engages in it. As suggested Becker and Murphy (1988), addiction can in fact be modeled as a rational behavior, where consuming more and more of an addictive good may maximize a consumer's discounted utility. None the less, there exists no literature suggesting that addiction, alcohol or drug consumption is a net benefit to society as a large. Even if an individual rationally engages in behavior that will cause (or risk) addiction, this does not mean that society as a whole will not suffer from the effects of that rational choice (in the form of increased health care costs, costs associated with adopting or placing children of addicts into foster care etc.).

As it would not be practically feasible to test for every possible bias and fallacy, I focus on five well-established deviations from the neoclassical model, loss aversion, the sunk cost fallacy, impact bias, present bias and the gambler's fallacy. The paper is organized as follows. In Section 2 I discuss the five biases, both putting forward the economic argument that the biases reduce utility and counterarguments that they may be utility enhancing. In Section 3 I develop my methodology and discuss its limitations. In Section 4 I present my results and discuss their meaning, and finally In Section 5 I draw my conclusions.

2. Biases

Loss aversion

Loss aversion, first identified by Kahneman and Tversky (1979), refers to consumers' and investors' tendency to be willing to go to greater lengths and take greater risks to avoid losses than to make gains. A loss averse consumer has two utility functions: A concave utility function for gains, and a convex utility function for losses. This allows the consumers to act simultaneously risk averse and risk loving depending on whether losses or gains are involved.

It has been observed (Haigh and List, 2005) that traders who are 'in the red' are more prone to take risks than those who are 'in the black'. In many cases this desperation to get 'back in black' only ends up causing bigger losses.

However, a strong aversion towards losses may also spur an individual to take action and make changes – even drastic, uncomfortable changes – when losses (financial or otherwise) do happen. It is important to keep in mind that in addition to loss aversion, behavioral economists have also identified a status quo bias. Perhaps loss aversion in some way acts as a 'counter-weight' to the status quo bias, spurring individuals to abandon the convenience of what is known to avoid losses.

Sunk cost fallacy

A neoclassical consumer, when deciding whether to spend money to pursue a certain end, would only consider the marginal benefit vs the marginal cost, ignoring any irretrievable costs in the past. The sunk cost fallacy (Staw, 1976) however results in consumers taking these irretrievable (sunk) costs into account when making decisions. This commonly results in 'throwing good money after bad', when an investor who has lost half the value of his portfolio continues to hold simply because he's already lost so much and does not want the agony he has gone through to have been for nothing instead of objectively estimating the likelihood of the portfolio having a positive return in the future and based on that deciding whether or not to sell. Succumbing to this flawed reasoning can lead to lower future income. Outside of economics, many people stay in relationships because they feel that since they have already invested so many years, they do not want to throw those years away.

Thus, this fallacy may also hurt mental health, as in the aforementioned example of people who stay in loveless, unhealthy relationships simply because they have already been in the relationship for a long time.

There is, however, a case to be made in favor of the sunk cost fallacy: Consider an obese person who, on January 1, buys a 1-year gym membership. Over the course of the year, the obese person may be able to 'fool' himself into going to the gym even though he does not feel like it, by telling himself that "I spent so much money on it, if I do not use my gym membership it will all have been a waste". The sunk cost fallacy, in this case, becomes a motivator for a positive behavior (exercising).

Likewise, for a severely depressed person, the sunk cost fallacy may be able to prevent suicide attempts – "I have fought this depression for so many years, I am not going to just throw it all away now". This is an irrational motivation for living, as any past efforts you have sunk into keeping yourself alive are irretrievable and should not matter for your decision on whether or not to carry on. A 'rational' consumer would decide on whether or not to commit suicide by objectively estimating whether the net utility of the rest of his/her life was positive or negative. In reality this "lifetime utility estimate" is almost impossible to calculate, and especially so for someone whose mind is clouded by depression.

One may counter that individuals should not need to rely on fallacies like the sunk cost fallacy in order to partake in healthy behaviors like exercising, or more basic things like merely staying alive. However, the fact that there may be a thousand rational, fact-based arguments in favor of exercising does not mean that consumers do not need or do not in practice benefit from the additional 'motivation' that the 'flawed' thinking provides.

Doody (2013) argues that the sunk cost fallacy is mislabeled and that, from a social perspective, caring about sunk costs may make sense. An individual who often quits endeavors half-way through may lose reputation and be considered less trustworthy, even if quitting was the right choice. An individual who realizes that a certain investment, financial or otherwise, was a mistake and quits is essentially admitting that he or she committed an error of judgement when he or she started, which will impact negatively on how other people view him/her. Depending on how great this loss of reputation is, it may make sense to hold and hope that the investment will turn out well against the odds.

Impact bias

Impact bias (Kahneman and Snell, 1992; Gilbert et al., 1998) refers to a tendency by consumers to overestimate the impact of a given event. In economics, the event in question is usually a change in income, or the purchase or loss of a good or service. Several studies have shown that consumers tend to overestimate in particular how long, for example, a salary raise will make them happier, or how long a salary reduction will make them less happy. The impact bias can cause sub-optimal decision-making as consumers may decide to spend more on something than what it is really worth or work harder to receive a raise than what the raise is really worth in terms of utility (Hsee and Zhang, 2004). Simply put, the belief that eternal happiness or at least eternally increased happiness is only one purchase or salary increase away can cause consumers and workers to make poor choices.

This fallacy is also likely to be exacerbated by cultural factors, at least in the developed world. Plenty of advertising is aimed at convincing consumers that the product being advertised will be revolutionary and that life will never be the same again after purchasing it. Companies also benefit from employees believing that a raise or promotion would make them permanently happier, so that they work harder than they would if they realized that any increase in utility would be quite small and temporary.

However, similar to how the sunk cost fallacy may keep someone from suicide, the impact bias may prevent a person from falling into despair. If one sincerely believes that eternal happiness is merely one purchase or promotion away it may be easier to feel hopeful about the future. If it is the case that the impact bias helps people avoid despair, then the economic impact may very well be positive as the negative economic consequences of despair are severe (Pecchenino, 2014).

Present bias

For consumers with present bias, the per-period discount rate is not constant as assumed under the neoclassical Discounted Utility model, but rather falls over time. This means that a consumer, who rejects €110 in one year in favor of €100 today, may still choose €110 in two years in favor of €100 in one year. There are several behavioral economic models that incorporate present bias, most notably the hyperbolic discounting model (Loewenstein and Prelec, 1992) and quasi-hyperbolic discounting model (Laibson, 1997)

High discount rates have been associated with smoking (Fersterer and Winter-Ebmer, 2003), obesity (Zhang and Rashad, 2008) and drug use (Kirby, Petry and Bickel, 1999). It should be noted, however, that these studies have failed to show a causal link. Furthermore, the mindset of a ‘present biased’ individual may be one that reduces the risk of depression and despair by focusing on today and not worrying about any possible dark clouds that a neoclassical consumer may see on the horizon. Hence, while there is a strong case to be made that present bias is socially undesirable, we cannot tell from existing literature whether it reduces individual utility.

Gambler’s fallacy

The gambler’s fallacy, first described by Tversky and Kahneman (1974), and its counterpart, the hot hand fallacy (throughout this paper these terms are used interchangeably), refers to a tendency among consumers, which is especially common among gamblers, from which the fallacy gets its name, to believe that chance has a memory. A person suffering from the gambler's fallacy would believe that a coin is more likely to come up heads if it came up tails the last time, since he/she knows that there is a 50/50 chance of either outcome, while someone suffering from the hot hand fallacy would think the likelihood is greater that it comes up heads again. In both cases the person is unable to comprehend the difference between cumulative probability and individual probability. Though it is unlikely to get heads when tossing a coin three times in a row (the cumulative probability is 1/8), *if* it were to happen it is not any less likely for the coin to come up heads on the fourth toss.

Chen, Moskowitz and Shue (2016) found evidence for negative autocorrelation in decision-making, i.e. gamblers fallacy, among everyone from judges in asylum courts to baseball umpires and loan application officers.

It is easy to see how the gambler's fallacy could hurt a consumer at a casino – such a consumer may convince him/herself to continue gambling because "every time I lose, I get closer to a win", or convince him/herself to bet everything on Red at the roulette table just because Black has come up on the last five spins.

Outside of the casino, the gambler's fallacy has the potential to cause problems by fooling consumers into thinking that their luck must "even out" – if they have suffered one unfortunate random event, such as having had their car stolen, they do not need to worry about it happening again because the likelihood of one person suffering car theft twice in their life is very small. This creates a false sense of security that can lead to poor decision-making. Likewise, the hot-hand fallacy may cause a person to think that a certain random negative event will not ever happen to him/her just because it has not happened so far even though it "should" have.

At the same time, this false sense of security can allow consumers to ignore risks that, if they were ‘hanging over their heads’, would reduce their utility. This is particularly the case for risks that are difficult or impossible to mitigate. In those cases, if consumers did not believe themselves to be immune to whatever the negative event was, the anxiety would merely reduce their utility without the knowledge of the risk giving them a chance to reduce the risk.

If it is in fact the case that biases do not harm consumers individually or society as a whole, this puts into question the usefulness of debiasing. It would also weaken the case for paternalism, i.e. nudging, that is based on the idea that consumers, when left to their own devices, end up making choices that reduce both their own utility and that of society.

Given the above analysis I test three hypotheses: First, that no biases other than the present bias will be associated with harmful behaviors. Secondly, that only the sunk cost fallacy will be associated with reduced earnings. Finally, that both these biases – and only these biases – will be associated with a higher rate of depression.

3. Methodology

I conducted a quasi-experimental survey online using the platform QuestionPro from 11 December, 2017 to 11 January, 2018. In total, 1154 participants took part.

Conducting this experiment online rather than in-person provided a number of benefits. First and most importantly, it allowed the experiment to include people of all age groups, from dozens of countries, of all employment statuses. This is in contrast to traditional lab experiments which tend to have a heavy overrepresentation of college students and the demographic groups that they typically belong to, that is overwhelmingly young and middle class. Greater diversity improves the external validity of the survey.

Second, there is significantly less effort and time commitment involved in participating in an online experiment. Lab experiments, in contrast, take more time as participants need to get to and from the location of the lab, material needs to be handed out to all participants, and all participants would typically be given time to ask questions. This may create a selection bias as the participants who are willing to go through this time-consuming ordeal may not be representative of the overall population, and possibly not even of their own demographic groups. This experiment, on the other hand, took just 20 minutes on average to complete.

Third, it is likely that the relative anonymity of the internet reduces the observer effect, where participants act differently because they feel, rightly or wrongly, that they are being watched.

Finally, the internet allows for a sample size that is practically impossible to achieve in a physical setting. The sample size of a lab experiment is limited to the size of whatever room acts as the “lab”. Very few rooms hold more than few hundred people and most labs used for experimental studies are much smaller.

There are of course also a number of drawbacks associated with online experiments. As discussed by Grimelikhuijsen and Meijer (2014), since participants cannot be observed while completing the experiment, the experimenter has less control and the internal validity suffers. Since I was unable to observe the participants, I cannot know for certain whether some of them may have looked up the “correct” answers to the questions on the internet. Online experiments are also vulnerable to multiple submission, though this appears to be rare (Reips, 2000), a risk that was reduced in this survey since participants could not take the survey twice using the same IP address.

Finally, as has been discussed by among others Duda and Nobile (2010), online surveys and experiments cannot be considered unbiased, since the condition for unbiasedness is that every member in the population under study must have a known chance of participating. Since there are no representative samples of email addresses for the general population, and since not all in the general population are online, this is difficult or impossible to achieve. However, I maintain that online experiments still compare favorably to lab experiments on this point since these mainly rely on students.

Usually in economic experiments participants are divided into treatment and control groups and the results compared. Here, because the purpose of the study was to find out if individuals who acted in a manner consistent with neoclassical predictions had better outcomes in life, from both individual and social viewpoints, such a design was unsuitable since it does not allow identification of which participants are “irrational”. An experiment on loss aversion may reveal that the treatment group which was solving tasks involving losses was less risk-averse than the control group which solved tasks involving gains, but such an experiment cannot tell us *which* participants suffer from loss aversion, only that loss aversion to some extent exists.

To identify which individuals suffer from loss aversion and other biases/fallacies, this experiment asked the same set of questions to all participants. This allowed for patterns to be found in the responses that give identification of which participants suffer from which biases.

The survey was divided into two parts. The first part of the survey consisted of economic decision-making tasks and tested participants’ tendency to exhibit present bias, loss aversion, impact bias, gambler’s fallacy and sunk cost fallacy. To make it harder for participants to figure out the purpose of the study, no two questions in a row dealt with the same bias/fallacy, and demographic questions were interspersed between the tasks. To help participants consider each question separately, there was only one question per page. While this study did not rely on any outright deception, it was necessary to keep the purpose and hypotheses hidden from participants as this might otherwise have affected their choices.

The second part of the survey asked questions on mental health and whether participants partook in a series of socially desirable/undesirable behaviors.

Interspersed throughout the survey were three ‘trick’ questions, the purpose of which was to determine whether or not participants were paying attention and taking the survey seriously. These questions were necessary as unserious or inattentive participants may seriously reduce the external validity of the findings.

This experiment did not utilize real incentives as doing so would have been unfeasible for many of the scenarios. Whereas real incentives may trigger more realistic responses, in the context of an online survey it would also have limited the type of questions and scenarios one could pose to participants.

Instead, the survey relied on the above-mentioned trick questions to weed out unserious participants. It also, to a great extent, relied on questions with realistic scenarios that many participants would have found themselves in (or could see themselves being in), allowing participants to draw on their experiences when answering the questions. Questions were designed to elicit specific biases.

Loss aversion

Participants' loss aversion was examined through three pairs of questions. In the first pair, participants were asked to choose between two options: With the first option they had a 100 percent chance of winning (losing) €5, with the second option they had a 50 percent chance of winning (losing) €10 but a 50 percent chance of not winning (losing) anything. The second and third pairs were identical except the gains/losses were increased to 20/40 and €100/€200 respectively. A loss averse participant would tend to choose the safe option when gains were involved but reverse their preference and choose the riskier option in the "loss" tasks.

While I am unaware of any experiment that has utilized these exact tasks, I chose them because they are unequivocally related to loss aversion and they are easy to understand. Other experiments (Thaler, Kahneman and Schwartz, 1997) have utilized tasks with scenarios from the stock market, but I felt that this would have been suboptimal since most people have little or no experience trading stocks and thus their choices may have resulted from confusion rather than preference.

Sunk cost fallacy

Four different tasks were meant to discern which participants were prone to “throw good money after bad” as the sunk cost fallacy is informally described. In the first task participants are posed with the scenario of having accidentally booked a ticket to another movie than the one they wanted to see, and to make it worse, the ticket was expensive, non-refundable, and the movie is not even one the participant would enjoy watching. Do you still go to the movie? Rationally the answer is No since no entertainment would come from watching a bad movie and it is reasonable to assume that the evening could be spent on a more enjoyable activity. Participants who suffer from the sunk cost fallacy however would choose to go to the movie as they have already paid for the ticket and want to get something out of the money rather than acknowledge it as an unrecoverable loss not worth dwelling on.

This task was inspired by Thaler (1980), who cited as an example of a sunk cost fallacy a family who decided to attend a game in the middle of a blizzard because they had already paid for the (presumably non-refundable) tickets, even though they conceded that, had the tickets been free, they would not have gone. Similarly, in the task above, having already paid for a ticket induces (some) consumers into a non-pleasurable activity.

In the second task, participants are asked to imagine that they are visiting an expensive restaurant that they have looked forward to going to for a long time. The meal is just as delicious as expected, but the portion is huge and they soon feel full, even though the plate is only half-empty. The restaurant does not offer any to-go bags so if they do not finish the meal, it goes to waste. Participants are asked whether they would finish the meal or leave it? The Neoclassical consumer would no doubt leave the meal as the marginal utility of eating once full is negative. A consumer suffering from the sunk cost fallacy on the other hand will choose to finish the meal since it is expensive and they will be paying for the full portion regardless.

While to the best of my knowledge no previous experiment has used this specific task, the tendency for some consumers to consume in order to "get their money's worth", including in the context of restaurants and buffets, is well-established (Clark and Goldsmith, 2005; Just and Wansink, 2011) and was the inspiration for both this and the final sunk cost fallacy task.

In the third task participants are asked to imagine they go to a club that has a €10 cover fee. They really like the club so they think it is worth it, but shortly after arriving they decide to go outside, and when they try to go back in they realize they have lost their ticket and the bouncer refuses to believe them when they say they have already paid. They are not in a bad spot economically so they could afford to pay the cover fee again, or they could go somewhere else. What should they do? From a neoclassical viewpoint, the answer is obvious: If the club was worth a €10 cover fee the first time you entered, it is worth it the second time as well. The fact that you already spent €10 makes no difference as there is no way to recover that money. The sunk cost fallacy on the other can convince a consumer to refuse to pay the additional €10 because the club is not worth a €20 cover fee.

This question was inspired by Tversky and Kahneman (1981), who asked in the context of going to the cinema (buying a second ticket after losing the first one).

In the final task participants are asked whether they usually eat more to "get their money's worth" at all-you-can-eat buffets. There is no rational reason to do so as the cost paid for the buffet is a sunk cost and so the neoclassical restaurant guest will simply eat until the next bite no longer generates positive marginal utility. The 'irrational' guest sitting at the table next to his on the other hand may very well continue stuffing himself with food until he is sick just so he can get his money's worth.

Impact bias

In this experiment, two multiple-choice tasks test for impact bias. In the first task, consumers are asked for how long they would be happier if they were to win €100,000; in the second task, how long they would be happier if they received a 25 percent salary increase. The only answer option that counted as indicating impact bias was "I would be permanently happier". While it is possible that some respondents would in fact be permanently happier, it is unlikely given the well documented human ability to adapt to new circumstances. Eventually, the new money would simply become part of a "new normal" and utility levels would fall back to pre-impact levels.[1]

While these tasks have not been used in previous literature, the specific scenario of a lottery win has been used in the past to estimate impact bias, including by Buechel, Zhang and Morewedge (2017).

Present bias

To test for present bias, this experiment included four choice tasks meant to discern patterns of discount rates that fall over time. If a participant for example chose €100

[1] In the case of individuals with very low levels of income, it may be hypothetically possible that a rise in income would make them permanently happier. However, these cases are rare, and likely even rarer in this sample as the median income was high.

today over €109 in one month, but then chose €281 in one year over €100 today, this would indicate present bias as the per-month interest rate is the same ($1.09^{12}=2.81$).

The use of choice tasks to determine intertemporal choice preferences is widespread and is in fact the most common way to elicit discount rates (Frederick, Loewenstein and O'Donoghue, 2002). One important benefit of choice tasks is the ease through which participants can understand the tasks, and how similar the tasks are to real world situations (such as when participants are asked between spending their money or saving it in a bank account in exchange for a certain fixed interest rate for a given time period).

Gambler's fallacy

This experiment includes three tasks that test for gambler's fallacy. The first task asked participants to predict the likelihood of rolling a six, provided they had previously rolled two sixes in a row. The second task asked participants to predict the outcome of the next coin toss, provided the last six tosses all came up heads.

While these two tasks were similar, the third task was a scenario task where participants were asked to imagine they had arrived at a roulette table, and the dealer offers to tell them the outcome of the last 10 spins in exchange for 1 percent of their winnings. Accepting this deal indicates that a participant suffers from the gambler's fallacy, as roulette is a game of chance and, since chance does not have a memory, any information about past spins is absolutely worthless. Many past experiments (Ayton and Fischer, 2004; Barron and Leider, 2010) on gambler's fallacy and hot hand's fallacy have used roulette as a setting, though none as far as I know have used this particular task. The main reasoning behind this task was to have one task in a more applied setting so as to test not just participants' understanding of probability theory but also whether they could apply this in a real-world setting.

Measuring depression

While happiness (utility) cannot be directly measured, several tests have been developed by psychiatrists to measure the presence and severity of depression. This experiment relied on the Patient Health Questionnaire, or PHQ-9, which is a well-recognized screening tool for depression backed up by multiple large studies (Kronke, Spitzer and Williams, 2001; Arroll et al., 2010; Martin et al., 2006). PHQ-9 relies on the patient answering nine multiple-choice questions regarding their behavior and state of mind over the past few weeks. There are 4 possible answers to each question ranging from "Not at all" to "Nearly every day", with points allocated for each question from 0 ("Not at all") to 3 ("Nearly every day"). A total score is then calculated, ranging from 0 to 27, where a higher score indicates a higher risk of depression. If biases are making consumers less happy, one would expect to find that biased consumers score higher on this test.

Economic and lifestyle decision-making

Even if it were to be the case that biases did not increase the risk of depression, there may still be a case for debiasing if biases could be found to be positively correlated with socially undesirable behaviors or negatively correlated with socially desirable behaviors. This experiment tested for the presence of a range of economic and non-economic behaviors, as well as income and employment status.

Finally, participants were given the opportunity to provide feedback. This served two purposes. First, the feedback opportunity gave participants an opportunity to point out things that I, the experimenter, may have overlooked. Second, the feedback may reveal that a participant did not know what he or she was doing or did not take the experiment seriously. In this way, the feedback question compliments the previously mentioned trick questions.

Limitations

As with any methodology there are certain limitations and potential weaknesses that need to be discussed.

The first is the lack of real incentives. It is generally advised to use real incentives in economic experiments, in order to induce participants to act in the way they would in real life. This was not feasible in this case as many of the tasks did not have a monetary payout at all (such as the sunk cost fallacy tasks). Even those that did, such as the intertemporal choice tasks, would have required immediate payout to participants (if they chose the smaller-sooner option) which would not have been possible. Furthermore, in order to use real incentives, there would have been a need for me to have a way to contact participants, such as by having them give me their email address, which would have compromised their anonymity and may have affected how they responded to some of the more sensitive questions.

Secondly, many questions touched upon sensitive topics, and as such there is a risk that some participants did not answer honestly. This risk contributed to the choice of using an anonymous online survey that did not ask participants for any identifying details.

Thirdly, some participants may genuinely not know the answer to all the questions. This may explain why only around 11 per cent of participants state that they suffer from obesity and/or Type II diabetes. It is likely that not all obese people know what their body mass index (BMI) is, or that their BMI qualifies them as obese. In retrospect asking participants about their height and weight would have been wiser.

Likewise, some people might not know exactly how much alcohol they consume in a typical week. Other surveys have employed other techniques to accurately measure alcohol consumption, such as having participants look at pictures and choose which picture their alcohol consumption most looks like. The platform used for this survey, however, did not support the use of graphics. While it is possible that some may have underestimated their alcohol consumption, this would not necessarily change which biases affect alcohol consumption, which after all is why this question was included in the first place.

The question regarding annual income also suffered from this issue. In many countries, including all of Scandinavia where a majority of participants reside, salaries are typically stated in monthly terms. While it may seem easy to multiply one's monthly income by twelve in order to get the annual income, it cannot be ruled out that some participants unintentionally put down the wrong answer. It also cannot be ruled out that some participants, particularly working-class participants who are paid on an hourly basis and who may not work the same hours every month, may not have known exactly how much money they make in a year. In some cases, it could be deduced that participants had written down their monthly, rather than their annual income. Participants were asked to provide their income in the currency they were paid in, but many who lived in Scandinavia did not and instead wrote down their income in euros. There is no way of knowing what exchange rate these participants used or how close this exchange rate was to the correct exchange rate. This issue was however mitigated by turning the responses to the income question into three separate binary variables, which are less sensitive to outliers. Even if some individuals (intentionally or unintentionally) misstated their income. Overall it seems reasonable to assume that participants in the upper quartile have higher incomes than those in the bottom quartile.

There were relatively few tasks associated with measuring each bias. This was done for several reasons: Had there been more tasks, the drop-out rate among participants would no doubt have increased as few people are willing to spend significant amount of time on internet surveys. This would have increased the risk of a self-selection bias, as well as reducing the total sample making regression estimates less reliable. Furthermore, it was essential for the external validity of the experiment that participants remained unaware of the purpose of the experiment, since if participants realized that a certain task is testing for the presence of a certain bias it could affect how they act. It appears intuitively likely that the likelihood of participants realizing the purpose behind the survey would increase with the number of questions as participants are more likely to spot patterns.

It should be noted that the survey dropout rate is unknown since participants who did not complete the survey had their responses deleted immediately as promised in the survey instructions. This was done for ethical reasons, as a participant who did not complete the survey might not consent to having their data stored (i.e. they may have changed their mind since they started the survey). None the less, this lack of data is a shortcoming as it may have revealed how successful the survey design (with the relatively few tasks etc.) was at reducing the dropout rate.

4. Results

These results were obtained by estimating logistic regressions using the socially desirable/undesirable behaviors, mental/physical health states and income as dependent variables and the biases and demographic variables as independent variables.

For the biases, scores were calculated for each participant based on their answers to the survey. A sunk cost fallacy score of 3 means that the participant chose the answer indicating sunk cost fallacy on 3 out of a maximum of 4 tasks. Present bias is a binary variable, while the gambler's fallacy and loss aversion have a maximum value of 3 and impact bias a maximum value of 2.

For the purpose of regression, the biases were turned into binary variables, with cut-off scores: The cut-off score is set at 3 for sunk cost fallacy, 2 for impact bias, gambler's fallacy and loss aversion and 1 for present bias. That is; any participant who answered in a sunk cost fallacy manner in 3 or more tasks is classified as suffering from the sunk cost fallacy, etc.

Each task has been given the same weight for the purpose of calculating scores. While it may be argued that some tasks should be more heavily weighted than others, it is not clear which tasks these would be or that there is an objective standard by which one could determine how much more heavily some tasks ought to be weighted compared to others.

An additional set of regressions was also estimated with the cut-off score being the maximum score for each bias; that is, with participants only being classified as suffering from a bias if their answers indicated that they did for every task related to that bias.

Regressions were estimated with and without control variables. The regressions with the income-related dependent variables were estimated with a reduced sample that excluded retirees, students, unemployed/disabled and part-time workers as these would otherwise have muddled the results by having lower-than-average income for reasons unrelated to behavior. The “Unemployed” regression was estimated without retirees, students and also those over the age of 64 as none of them were unemployed.

Two variables relate to depression. As discussed in the previous section, this experiment utilized the PHQ-9 questionnaire, which provides all participants with a score from 0 to 27. The cutoff score for the “Depression” variable is 10, which is a commonly used score among psychiatrists at which point further treatment would be necessary (Kroenke, Spitzer and Williams, 2001; US Preventive Services Task Force, 2005). The cutoff score for “NoDepression” is 4; that is, any participant scoring *higher* than a 4 receives a zero in this variable. This, too, is based on psychiatric guidelines.

Furthermore, two variables relate to alcohol consumption – “AlcoholDanger” and “AlcoholOveruse” where the former refers to consuming more than 14 units of alcohol per week, which is the maximum consumption advised by the NHS (National Health Service, 2018). While there are other indicators of problematic alcohol consumption, the total consumption in a given week was chosen as other measures, such as the frequency with which a participant engages in binge drinking, may have increased the number of dishonest responses as participants may be ashamed to admit to engaging in binge drinking sessions. “AlcoholOveruse” refers to consuming more than 4 units per week, which can be considered socially sub-optimal since anything beyond minimal alcohol consumption is negative from a health perspective and therefore increases health care costs.

One of the final questions asked participants how much attention they had paid to the survey. Those who admitted to not paying any or very little attention were removed from the sample. Only those who stated they paid full attention to the survey were included in the regressions below as results may otherwise be muddled by participants who were not taking the questions and scenarios seriously. Participants who failed any of the “trick questions” were also removed, for the same reason.

Model specification and assumptions

LOGISTIC REGRESSION MODEL 1: No control variables

$$\log \text{Prob}(Y=1)/(1 - \text{Prob}(Y=1)) = B_0 + B_{PresentBias}d_{PresentBias} + B_{lossaverse}d_{lossaverse} + B_{impactbias}d_{impactbias} + B_{gamblersfallacy}d_{gamblersfallacy} + B_{sunkcost}d_{sunkcost} + B_{patiencescore}d_{patiencescore}$$

LOGISTIC REGRESSION MODEL 2: Control variables included

$$\log \text{Prob}(Y=1)/(1 - \text{Prob}(Y=1)) = B_0 + B_{PresentBias}d_{PresentBias} + B_{lossaverse}d_{lossaverse} + B_{impactbias}d_{impactbias} + B_{gamblersfallacy}d_{gamblersfallacy} + B_{sunkcost}d_{sunkcost} + B_{undergrad}d_{undergrad} + B_{postgrad}d_{postgrad} + B_{age2435}d_{age2435} + B_{age3664}d_{age3664} + B_{over64}d_{over64} + B_{Anglosphere}d_{Anglosphere} + B_{developingcountry}d_{developingcountry} + B_{otherlocation}d_{otherlocation} + B_{patiencescore}d_{patiencescore}$$

where Y is a dependent variable (see Tables 6-10).

As with any type of regression, there are certain assumptions that must hold true in order for a logistic regression – the type that was used for this research – to yield valid, reliable results.

First, given that these are binary logistic regressions, the dependent variables must be binary variables. This is clearly the case.

Second, as is the case with ordinary least squares regression, logistic regression too requires that observations be independent of one another. There is no reason to believe that the answers of one respondent would not be independent of the other respondents. The sample is diverse with participants of all ages spread out over most of the western world (with a few coming from other parts as well). The only way this assumption could reasonably be violated would be if one participant filled out the survey multiple times, but this is unlikely to be the case as IP address tracing allowed for duplicates to be deleted. While IP address tracing is not foolproof, a participant would have to go to great lengths to circumvent it, and it seems implausible that any participant would do so as there was no reward for finishing the survey.

Third, there must be little or no multicollinearity among the independent variables in a logistic regressions. By calculating the variance inflation factor it was confirmed that this is indeed the case.

Fourth, logistic regression requires that continuous independent variables be linearly related to the log odds. Using the Box-Tidwell test, this assumption was tested and the null hypothesis of linearity was never rejected at a 5 per cent significance level regardless of which dependent variable was used.

Finally, logistic regression requires a relatively large sample size. Peduzzi et al. (1996) argued that as a guideline the sample size, N, should be at least equal to 10k/p, where k is the number of independent variables and p the smallest number of negative or positive cases in the population. The regressions that do not include control variables fulfill this condition, and it is on these that the conclusions of this study are mainly based.

Credibility of results

With this as with any experiment, there is a very legitimate question on whether or not the results are reliable. Were participants honest, even though they had no incentive, beyond their own goodwill, to be? Did they take the survey seriously or did they view the questions more as a ‘game’ than a serious decision-making exercise? One way to get an idea about this is to look at the correlations between the demographic, economic and lifestyle variables and compare them with what one would expect to find. From the data of this experiment, the following should be noted:

- Private saving increases with age, until retirement when it drops again. This is consistent with the life-cycle theory of consumption.
- Drug use is linked with higher depression scores, a link backed up by literature (Grant, 1995).
- Young people in the survey are more likely to use drugs, which is also known to be the case in the general population (National Institute on Drug Abuse, 2017).
- Those who are unemployed have higher depression scores in this experiment. The link between unemployment and depression is well established in the literature (Paul and Moser, 2009).
- Having a postgraduate degree is associated with a higher income, as is age.

- Obese individuals in this survey have higher discount rates, which both intuitively makes sense and has been backed up by previous studies (Zhang and Rashad, 2008).
- Obese individuals also score higher depression scores. Obesity has been linked to depression (Dong, Sanchez and Price, 2004).
- Drug users in this experiment are more likely to smoke, which previous studies have also found (Degenhart, Hall and Lynskey, 2001). They are also more likely to have a dangerous level of alcohol consumption, which is also backed up by literature (Burns and Teesson, 2002).

Based on this, it seems very likely that participants in this survey did in fact answer honestly, as it is otherwise hard to explain how all these significant correlations that correspond with what we know to be true from previous research occurred.

TABLE 1: DESCRIPTION OF VARIABLES	
PresentBias	1 if participant exhibited present bias, 0 otherwise
sunkcost	1 if participant exhibited sunk cost fallacy on at least 3/4 tasks, 0 otherwise
gamblersfallacy	1 if participant exhibited gambler's fallacy on at least 2/3 tasks, 0 otherwise
lossaverse	1 if participant exhibited loss aversion on at least 2/3 tasks, 0 otherwise
impactfallacy	1 if participant exhibited impact fallacy on 2/2 tasks, 0 otherwise
unbiased	1 if participant exhibits one or fewer biases as defined by the thresholds, 0 otherwise.
patiencescore	Takes values between 0 and 4 depending on how many times the participant chose the larger-later option over the smaller-sooner option on the intertemporal choice tasks
depression	1 if participant had a PHQ-9 score of 10 or higher, 0 otherwise
nodepression	1 if participant had a PHQ-9 score of 4 or lower, 0 otherwise
druguse	1 if participant has used illegal drugs in the past year, 0 otherwise

alcoholdanger	1 if participant consumes more than 14 units of alcohol in a typical week, 0 otherwise
alcoholoveruse	1 if participant consumes more than 4 units of alcohol in a typical week, 0 otherwise
smoking	1 if participant smokes or uses other forms of tobacco, 0 otherwise
obesity	1 if participant suffers from obesity or type II diabetes, 0 otherwise
yesbudget	1 if participant budgets his or her spending, 0 otherwise
saveprivate	1 if participant regularly saves privately (not just through a pension plan), 0 otherwise
Scandinavia	1 if participant resides in Scandinavia, 0 otherwise
Anglosphere	1 if participant resides in the English-speaking world, 0 otherwise
developing-country	1 if participant resides in a developing country, 0 otherwise
otherlocation	1 if participant resides in a country not included above, 0 otherwise
highschool	1 if the highest level of education achieved by the participant is a high school diploma or less, 0 otherwise
undergrad	1 if the highest level of education achieved by the participant is an undergraduate degree or equivalent, 0 otherwise
postgrad	1 if the highest level of education achieved by the participant is a postgraduate degree or equivalent, 0 otherwise
age1823	1 if participant is 18-23 years old, 0 otherwise
age2435	1 if participant is 24-35 years old, 0 otherwise
age3664	1 if participant is 36-64 years old, 0 otherwise
over64	1 if participant is over 64 years old, 0 otherwise
fulltime	1 if participant has held full-time employment, 0 otherwise
parttime	1 if participant has been employed part-time but not full-time, 0 otherwise
neveremployed	1 if participant has never held employment, 0 otherwise
student	1 if participant is a student, 0 otherwise
retired	1 if participant is retired, 0 otherwise
unemployed	1 if participant is unemployed, 0 otherwise
highincome	1 if participant's annual income is in the upper quartile of the sample, 0 otherwise
abovemedian-income	1 if participant's annual income is above the median of the sample, 0 otherwise
lowincome	1 if participant's annual income is in the lower quartile of the sample, 0 otherwise

Examining the raw data I find the following:

TABLE 2: SUMMARY OF BIAS AND PATIENCESCORE VARIABLE(S)

Variable	Mean
PresentBias	.5586854
sunkcost	.2230047
gamblersfallacy	.1866197
lossaverse	.2969484
impactbias	.2546948
patiencescore	2.255869

As the table shows, the share of individuals suffering from any given bias or fallacy ranges from around 18 % for the gambler's fallacy to just below 56 % for the present bias.

TABLE 3: SUMMARY OF DEPENDENT NON-INCOME VARIABLES

Variable	Mean
Depression	.1760563
NoDepression	.6056338
druguse	.1678404
alcoholdanger	.1795775
alcoholoveruse	.4401408
smoking	.2511737
obesity	.1126761
yesbudget	.5539906
saveprivate	.6314554

Just over 17 % of participants qualify as depressed according to the PHQ-9 scores. Only 11 % suffer from obesity of Type II diabetes, though, as discussed, this variable may suffer from a higher degree of measurement error.

TABLE 4: DISTRIBUTION OF ANNUAL INCOME AMONG PARTICIPANTS

Percentiles	
1%	€ 4 000
5%	€ 14 400
10%	€ 19 000
25%	€ 31 500
50%	€ 45 800
75%	€ 65 450
90%	€ 93 500
95%	€ 130 000
99%	€ 500 900

At €45,800 the median income in the sample is higher than in most countries, something that may be due to the higher proportion of postgraduate degree holders (see Table 5).

TABLE 5: SUMMARY OF DEMOGRAPHIC VARIABLES

Variable	Mean
Scandinavia	.5352113
Anglosphere	.3532864
developingcountry	.0446009
otherlocation	.0669014
highschool	.1666667
undergrad	.4225352
postgrad	.4107981
age1823	.149061
age2435	.2359155
age3664	.4776995
over64	.1373239
fulltime	.8603286
parttime	.1044601
student	.1619718
retired	.1748826
neveremployed	.0352113
unemployed	.0481221

Just over half the sample reside in Scandinavia, with around one third living in the Anglosphere. Very few participants live in the developing world, which makes sense as the survey was marketed at platforms dominated by European and American users, and the survey was available exclusively in English. Those aged 18-23 are overrepresented relative to their share of the adult population in western countries, though it should be noted that they are a much smaller share of the sample than in traditional lab experiments which frequently rely on all-student samples. The unemployment rate in the sample is just below 5 %, which is in line with most western countries.

Before moving on to the regressions, I would like to reiterate my hypotheses: First, that no biases other than the present bias will be associated with harmful behaviors. Secondly, that only the sunk cost fallacy will be associated with reduced earnings. Finally, that both these biases – and only these biases – will be associated with a higher rate of depression.

TABLE 6: DEPRESSION[2]

VARIABLES	(1) depression	(2) nodepression
PresentBias	0.824	1.332*
	(0.320)	(0.0649)
lossaverse	1.239	0.944
	(0.287)	(0.716)
impactbias	0.836	1.318
	(0.415)	(0.104)
gamblersfallacy	1.416	0.950
	(0.117)	(0.784)
sunkcost	2.961***	0.295***
	(3.61e-08)	(0)
patiencescore	0.737***	1.094
	(5.80e-05)	(0.135)
Constant	0.302***	1.367
	(2.91e-06)	(0.144)
Observations	852	852
Pseudo R2	0.0652	0.0513

[2] Notes on Tables 6-10: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

My third hypothesis stated that only the sunk cost fallacy and the present bias would be associated with higher likelihood of depression. The sunk cost fallacy is indeed associated with higher likelihood of scoring above the depression threshold of the PHQ-9 test, and lower likelihood of having a score of 4 or lower that would indicate no sign of depression, but the present bias is insignificant. Including the control variables made no difference as to which variables were or were not significant.

TABLE 7: INCOME[3]

	(1)	(2)	(3)
VARIABLES	highincome	abovemedianincome	lowincome
PresentBias	1.070	1.336	0.739
	(0.773)	(0.159)	(0.194)
lossaverse	0.695	0.796	1.695**
	(0.132)	(0.267)	(0.0220)
impactbias	0.956	0.776	0.897
	(0.853)	(0.233)	(0.660)
gamblersfallacy	0.725	0.689	1.786**
	(0.321)	(0.160)	(0.0369)
sunkcost	0.511**	0.343***	2.637***
	(0.0250)	(1.38e-05)	(9.62e-05)
patiencescore	1.207**	1.172**	0.845*
	(0.0394)	(0.0437)	(0.0540)
Constant	0.278***	0.925	0.362***
	(0.000170)	(0.786)	(0.00126)
Observations	477	477	477
Pseudo R2	0.0259	0.0465	0.0519

My second hypothesis stated that only the sunk cost fallacy would be linked to lower earnings. Instead, loss aversion and gambler’s fallacy also turned out to be associated with having a low income, although only the sunk cost fallacy can be linked to a reduced likelihood of having a high, or above median, income.

[3] The regressions for the income-related variables excluded 2 participants who had not disclosed their income, bringing the final sample size for the regression down to 475.

TABLE 8: DRUG USE AND ALCOHOL CONSUMPTION

VARIABLES	(1) druguse	(2) alcoholdanger	(3) alcoholoveruse
PresentBias	1.217	1.183	1.704***
	(0.322)	(0.391)	(0.000428)
lossaverse	1.007	0.929	1.049
	(0.973)	(0.709)	(0.755)
impactbias	1.078	1.108	1.002
	(0.724)	(0.617)	(0.990)
gamblersfallacy	0.894	0.734	0.835
	(0.642)	(0.219)	(0.323)
sunkcost	2.255***	0.639*	0.707**
	(5.38e-05)	(0.0592)	(0.0420)
patiencescore	0.760***	1.132	1.103*
	(0.000452)	(0.110)	(0.0963)
Constant	0.256***	0.171***	0.512***
	(2.45e-07)	(5.29e-10)	(0.00176)
Observations	852	852	852
Pseudo R2	0.0425	0.0115	0.0160

Only the sunk cost fallacy appears to increase the likelihood of drug use, and the inclusion of control variables renders this fallacy insignificant as well. Somewhat surprisingly, the present bias is not associated with use of illegal drugs; other studies have shown a link between high discount rates and drug use. However, it should be noted that the "patiencescore" variable captures high discount rates, meaning the present bias variable only represents the effect of having a higher discount rate for the present period than for future periods.

No biases appear to significantly increase the risk of problem drinking. When control variables are included, one bias – the gambler's fallacy – does in fact turn out to be significant, but participants suffering from this fallacy are on average **less** likely to have a dangerous alcohol consumption.

It should also be noted that the sunk cost fallacy variable is significant at a 10 % level and thus may be associated with a lower likelihood of overdrinking, but once control variables are included the variable is totally insignificant.

Only the present bias appears to be correlated with a higher risk of consuming more alcohol than what is socially optimal, that is consuming more than very little. This makes intuitive sense as the present bias refers to overvaluing the present relative to the future, in this context overvaluing the pleasure of consuming alcohol today relative to the future health consequences of doing so. The sunk cost fallacy is in fact associated with a lower risk of socially suboptimal alcohol consumption, though once again this variable turns out to be insignificant once control variables are included.

TABLE 9: SMOKING AND OBESITY/TYPE II DIABETES

VARIABLES	(1) smoking	(2) obesity
PresentBias	0.820	0.879
	(0.240)	(0.569)
lossaverse	0.967	1.113
	(0.850)	(0.651)
impactbias	1.173	0.970
	(0.376)	(0.905)
gamblersfallacy	1.631**	1.078
	(0.0119)	(0.781)
sunkcost	0.616**	1.096
	(0.0190)	(0.721)
patiencescore	0.981	0.818**
	(0.767)	(0.0220)
Constant	0.378***	0.197***
	(3.29e-05)	(5.89e-08)
Observations	852	852
Pseudo R2	0.0143	0.00969

In a surprising twist, the gambler's fallacy appears to increase the likelihood of smoking. The sunk cost fallacy is associated with a decreased likelihood of smoking, however the inclusion of control variables renders this variable insignificant.

No bias appears to be linked to increased risk of obesity or Type II diabetes, including, somewhat surprisingly, the present bias. Previous studies have found a correlation between high discount rates and obesity (Komlos, Smith and Bogin, 2004), but it appears that it is only the discount rate itself and not the structure of it (whether it is hyperbolic or not) that affects the likelihood of obesity. The inclusion of control variables did not substantially change anything; all biases remained insignificant.

TABLE 10: BUDGETING, SAVING AND UNEMPLOYMENT

	(1)	(2)	(3)
VARIABLES	yesbudget	saveprivate	unemployed
PresentBias	0.957	1.575***	0.769
	(0.767)	(0.00316)	(0.442)
lossaverse	1.162	1.034	0.746
	(0.327)	(0.836)	(0.441)
impactbias	1.217	1.106	0.622
	(0.222)	(0.546)	(0.248)
gamblersfallacy	1.547**	1.023	2.137**
	(0.0173)	(0.904)	(0.0471)
sunkcost	0.955	0.932	0.974
	(0.782)	(0.685)	(0.948)
patiencescore	1.058	1.320***	0.936
	(0.326)	(3.41e-06)	(0.614)
Constant	0.952	0.703*	0.116***
	(0.813)	(0.0966)	(2.16e-06)
Observations	852	852	538
Pseudo R2	0.00820	0.0225	0.0226

Budgeting is a good way for consumers to gain an oversight over their income and expenditure and avoid spending more than they can afford. The data from this experiment suggests that biases do not reduce consumers' tendency to budget: Biased consumers budget just as much as 'rational' consumers. In the case of consumers suffering from the gambler's fallacy, they actually seem to budget to a greater extent than their 'rational' counterparts. This remains the case once control variables are included.

Surprisingly, the present bias is positively correlated with regular private saving. No other biases are significant; adding control variables does not change this.

Finally, only the gambler's fallacy appears to be linked to a higher risk of unemployment. All the other biases are insignificant. This remains the case when control variables are taken into account.

My first hypothesis stated that no biases other than the present bias would be correlated with harmful behaviors. This is clearly not the case; present bias can only be linked to socially sub-optimal alcohol consumption. However, caution is very much advised. As previously stated, these regressions included a "patience score" variable which measured how many times participants chose the larger-later over the smaller-sooner option in the survey. This variable captured any change due to high discount rates, and as such the present bias variable only captures any change caused by having a hyperbolic discount rate. It appears that what matters is not the shape of the discount function, but rather the rate itself. The gambler's fallacy is associated with higher rates of smoking, but also lower alcohol consumption and a greater tendency to keep a budget.

It is important to note that even when biases are significant, their explanatory power is consistently very low: The explanatory power, as measured by the pseudo-R^2, is never higher than 7 percent and often below 1 percent. It appears that even if biases are harmful, the harm is relatively small.

Discussion of results

Of all the biases, the sunk cost fallacy turns out to be the overall most significant, being associated with a higher risk of depression, a lower income and a higher likelihood of using drugs. It should be noted however that we cannot be certain whether there is a causal relationship and, if so, how that relationship works. It is possible that the sunk cost fallacy may make an employee stay at a workplace even though he/she would be better off elsewhere because the employee feels like he/she has invested a lot of time and effort into the current workplace which would be wasted if they quit. Likewise, as discussed in the introduction, the sunk cost fallacy may make an individual stay in a relationship that is not fulfilling his/her needs, simply because they have invested a lot in it. This combined may increase the likelihood of depression, which in turn increases the likelihood of drug use.

However, it is also plausible that a low income may increase the likelihood of an individual falling victim to the sunk cost fallacy: People with lower incomes may be more likely to feel that they have to “get their money’s worth” and thus, as in the experiment, keep eating even when they are full or attend a movie that they have already bought a ticket for even though they do not like the movie.

Likewise, a person suffering from depression may “use” the sunk cost fallacy as a way of coping: Telling oneself that suicide is not an option because it would mean throwing away all the efforts one has to put into staying alive is irrational from a strictly economic viewpoint; the Neoclassical economist’s calculus suggests that all that matters is whether total utility of staying alive is expected to be positive or negative. However, this “irrational” way of thinking, that is non-neoclassical, may end up keeping some depressed people alive.

Next, the gambler's fallacy is associated with an increased risk of having a low income, of being unemployed and of smoking – but also of a higher tendency to budget. It should be noted that the gambler's fallacy is strongly correlated with having a low (high school diploma or less) level of education, and that this may be causing the gambler's fallacy: Since the gambler's fallacy is essentially a misunderstanding of how probability theory works, it would seem intuitively reasonable to think that less educated individuals may be more prone to this error. It is well-known that having a low education is associated with having a low income and a higher likelihood of unemployment. This however cannot fully explain the link as the fallacy remains significant even when education is included as a control variable.

Another possible explanation is that those suffering from the gambler's fallacy may be more prone to believe that they could become rich by playing the lottery (believing that "I am bound to win soon, I have played it so many times") or other forms of gambling. The inability to understand that they have no more chance of winning the lottery the 100th time they play than you have the 1st, and that the likelihood is always astronomically small (and that most if not all casino games are rigged against them) may make them put less effort into earning a high income through work.

It is also possible that the gambler's fallacy could act as a sort of coping mechanism, being caused by having a low income rather than causing it in the first place: In this scenario, a low-income earner may be more prone to think that since they have been unlucky in the past (or feels that bad luck has led to them having a low-paying job) they are bound to be lucky in the future. In other words, like with the sunk cost fallacy, there is a plausible way that low income may be the cause of, rather than being caused by, the bias or fallacy in question. One may think that, if this were the case, those suffering from gambler's fallacy ought to also show a higher tendency to suffer from depression, but this is not necessarily the case as coping mechanisms like this one might in some cases prevent individuals from becoming depressed in the first place.

In the case of smoking it seems plausible that those who suffer from gambler's fallacy, or rather, the hot hand fallacy, may not understand that just because they have not developed cancer or other smoking-related diseases yet, that does not mean it will not happen to them eventually.

Loss aversion is also associated with a higher likelihood of having a low income. One possible explanation for this would be that loss averse individuals are less likely to take chances, perhaps preferring jobs with lower-but-safe salaries and a low risk of being laid off, to more volatile industries (such as finance) where pay on average may be higher but where there is also a higher turnover and a higher risk of earning less in one year than one did in the year before if market conditions change (which would mean "losing" income relative to the status quo). Again however, it is plausible that having a low income is the cause rather than the effect. An individual with a low income may have a more visceral reaction towards the possibility of losing money, causing them to do almost anything to avoid it since even a small loss is disastrous.

The present bias appears to *increase* an individual's likelihood of regular private saving (that is, saving outside of a pension plan). This may seem counterintuitive, but it is important to remember that a lot of those who from the present bias know about this weakness in themselves: Someone who knows that he/she is likely to overspend due to present bias may be more likely to instruct his/her bank to transfer part of his or her salary to a savings account from which it cannot be easily withdrawn so that he or she doesn't get the chance to spend the money. It is also possible that those who are already saving may be more prone to feel that they "deserve" to choose the smaller-sooner reward since they are acting "patiently" in their everyday life.

Furthermore, the present bias is associated with a higher tendency towards socially suboptimal alcohol consumption, which makes sense given that the present bias implies favoring short-term enjoyment (such as those that stem from consuming alcohol) over long-term benefits (such as better health).

The least harmful bias appears to be the impact bias, which does not significantly affect any variable.

Correlation coefficients confirm these results with few exceptions: The present bias is associated with socially suboptimal alcohol consumption and drug use, whereas gambler's fallacy is associated with depression and higher likelihood of unemployment. Further, the sunk cost fallacy is negatively correlated with socially suboptimal and dangerous alcohol consumption, as well as drug use.

In addition to the set of regressions detailed above I ran another set in which the bias variables were replaced with a variable named "unbiased" which took the value 1 if a participant exhibited one or fewer biases, and 0 otherwise (see Tables 11-15 in Appendix B). The purpose of this was to get insight into whether biases as a whole appeared to be harmful, as it is hypothetically possible that two biases might be individually harmless but harmful when an individual suffers from both of them. Being unbiased was associated with a higher likelihood of having a high or above median income and a lower likelihood of having a low income. For all other dependent variables, the unbiased variable turned out to be insignificant, suggesting that being unbiased does not affect an individual's tendency to participate in socially undesirable, or desirable, activities. Neither does it appear to affect an individual's likelihood of depression. The unbiased variable is balanced, with only slightly more participants unbiased than biased, making these predictions more reliable than those from the main set.

Another set of regressions used only participants who stated that they reside in Scandinavia. Most notably among these participants, the present bias is associated with a lower risk of suffering from depression and increases the likelihood of not exhibiting any symptoms of depression as defined by the PHQ-9 scale. The sunk cost fallacy is once again associated with a higher risk of depression, but also with a higher likelihood to save. Caution is advised as the sample size is small (N=456)- Further research is necessary to confirm these results and, if confirmed, investigate what elements of the Scandinavian culture and/or economy may be responsible for these differences between Scandinavians and people from other countries.

Finally, a set of regressions was estimated with the cut-off score for each bias variable changed to the maximum – for a participant to be classified as suffering from the sunk cost fallacy, he/she would have to have answered in a way that indicates sunk cost fallacy on all four out of four tasks, and likewise with the other biases. In this set, only the sunk cost fallacy is associated with a higher risk of depression, whereas the present bias is associated with not having any signs of depression (of scoring between 0-4 on the PHQ-9 test). Only the sunk cost fallacy is associated with a lower likelihood of having an above median income, and a higher likelihood of having a low income. Sunk cost fallacy is also the only bias associated with drug use, and weakly (at a 10 per cent significance level) associated with having a dangerous alcohol consumption. Present bias in turn is associated with a socially suboptimal alcohol consumption. Those in the sample who are suffering from gambler's fallacy are more prone to smoke. Sunk cost fallacy is weakly associated with smoking and obesity/type II diabetes. No bias appears to have any effect on the tendency of a person to budget, or the risk of a person being unemployed. Finally, the present bias increases the tendency to save. It should be noted that these estimates are less reliable. This is because few participants are classified as biased, meaning the bias variables are heavily skewed while the sample size is unchanged.

Sensitivity and specificity are generally poor (see Appendix B, Tables 35-36), with the area under the ROC curve never being higher than 0.68 for the regressions without control variables. This however is to be expected given that many additional variables not included in the model(s) affect the dependent variable, and the purpose of the study was not to fully explain and account for all variables that affect the dependent variables, but rather to determine the effect, or lack thereof, that biases have.

My first hypothesis stated that only the present bias would be linked to harmful behaviors. This turned out not to be the case as sunk cost fallacy is associated with drug use, and gambler's fallacy with smoking.

My second hypothesis stated that only the sunk cost fallacy would be associated with reduced earnings. While the sunk cost fallacy is indeed associated with reduced earnings, the same is true for loss aversion and the gambler's fallacy.

My third and final hypothesis stated that only the sunk cost fallacy and the present bias would be associated with higher levels of depression. In reality, *only* the sunk cost fallacy is associated with a higher risk of depression

5. Conclusions

Are biases harmful? The results of this study suggest that the answer is “Not much, if at all”. Biases appear to be only at most a relatively minor factor in determining a person’s health, income, tendency towards socially destructive/constructive behaviors and likelihood of depression.

As discussed in the previous section, there are areas and situations where being biased may be beneficial. Proponents of debiasing may concede this point, but still argue that debiasing in certain specific areas where biases are harmful will not stop individuals from being biased when they benefit from being biased. In other words, teaching someone not to be biased in one context will not carry over into another context. While this may be true, the burden is on those who support debiasing to prove that debiasing actually does increase utility and that it does not have any serious adverse effects. In pharmaceutical research it is standard practice to monitor the overall health of the test subjects and not just the area that the medicine is supposed to affect. This is to discover any possible side-effects that a new drug may have. Behavioral economists, when developing debiasing techniques, need to act more like pharmaceutical researchers and dare to look into side-effects and also track participants after an intervention has ended as side-effects may not be immediately apparent. This becomes all the more critical as debiasing techniques become more sophisticated and permanent. Medical drugs are often rejected even though they work, simply because the side-effects are too severe to justify the benefit the drug provides. As far as I am aware, no behavioral economist has ever rejected a debiasing technique on these grounds – or even tested for possible side effects. Future research will focus on exploring this.

Proponents may further argue that even if biases do sometimes make people happy, that this happiness is "fake" and that having people embrace reality is better even if it reduces their utility. Whether it is better to be happy living in a lie or sad living with the truth is ultimately a philosophical question, however it should be noted that virtually all humans lie to themselves to make themselves happy. Ignoring our own mortality, signs of aging, or the fact that nearly all of us will be forgotten within 100 years of our deaths are all examples of these mood-enhancing "white lies". Would debiasing proponents have the government expend resources on reminding citizens about their gray hairs or upcoming death? If not, they will have to explain and preferably back up with evidence how the "biased" lies that can keep us going through depression and hard times are any different from these.

In any case, given the low explanatory power of biases, it may be wise for policymakers to focus their attention on other ways of reducing social ills and improving quality of life. Debiasing is not and will never be a magic bullet towards these ends, and behavioral economists who claim so may be suffering from an academic form of tunnel vision where they overvalue the significance of biases simply because their own field focuses so heavily on them: "When all you have is a hammer, everything looks like a nail" as the saying goes.

Future research will focus on determining whether there is a causal link between the biases identified in this work, and if so which way it runs. This may best be accomplished through a longitudinal study that would track participants' biases, depression scores, incomes, etc. over several years, perhaps decades. Future research will also attempt to reach demographics that were not included in this experiment, mainly those who do not speak English. This will be accomplished by translating future surveys into several languages.

To conclude, there appears to be little connection between being happy, healthy and wealthy on the one hand and "rational" on the other, and the recent policy focus on debiasing appears to be severely disproportionate when compared to the possible harm that biases cause.

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Appendix A: Survey design

Welcome!

My name is John Gustavsson and I'm an Economics PhD student at Maynooth University in the Department of Economics, Finance and Accounting.

This survey is part of the experimental research on the topic of economic behavior and happiness that I am conducting for my PhD dissertation. In this survey you'll be presented with a series of consumer scenarios and asked to choose between different options and courses of action. Please be advised that no question has just one right answer; what the correct answer is depends entirely on taste and preferences. Please consider each question separately!

This survey also contains demographic questions and questions regarding personal finances as well as mental and physical health. Please note that some of these questions are of a highly sensitive nature! All data will be stored in a password-protected folder stored in the university system, and there will be no further use of the data beyond this study. The data will be saved for 10 years after which it will be deleted.

This survey is entirely anonymous; you will not have to share your name, email or any other identifying information to submit your response. This survey is has been approved by and is bound by the rules of the Maynooth University Ethics Committee.

You may quit the survey at any time for any reason; if you quit before finishing the survey your data will be deleted.

If you have any questions or you wish to contact me for any reason, you can reach me at john.gustavsson.2010@mumail.ie.

You must be 18 or older to participate in this survey. This survey will take approximately 10-15 minutes to complete, obviously depending on how much time you spend thinking about your decisions.

It must be recognized that, in some circumstances, confidentiality of research data and records may be overridden by courts in the event of litigation or in the course of investigation by lawful authority. In such circumstances the University will take all reasonable steps within law to ensure that confidentiality is maintained to the greatest possible extent. Although as you don’t have to share any identifying information about yourself in this survey, it won’t be possible even with the full dataset to find out whom and where you are.

By proceeding, you agree to take part in this survey, and have your data stored under the conditions outlined above. Thank you for your participation!

Q1: I have read and understand the details of this project;

I understand

Q2: I understand that I can leave the survey at any time and my data will not be retained.

I understand

Q3: I am aware of the sensitive nature of the questions being asked regarding physical and mental health as well personal finances

I understand

Q4: I confirm that I am over 18.

Click here to confirm

Q5: NOTE: The scenarios in this survey use euros. 1 euro equals roughly 9.5 Swedish crowns, 1.2 US Dollars and 0.9 Pound Sterling. Again, please consider each scenario separately.

This survey contains questions regarding mental and physical health as well as personal finances. If you're uncomfortable answering these kinds of questions, please exit the survey now. Remember that you are anonymous.

I understand

Q6: You win a lottery and as your prize, you get to choose between 100 euro today or 127 euro a year from now. What do you choose?

100 euro today

127 euro a year from now

Q7: You are given a choice between two options: With Option A you have a 100 % chance of losing 20 euro, with Option B a 50 % chance of losing 40 euro but also 50 % chance of not losing anything. What do you choose?

Option A

Option B

Doesn’t matter

Q8: You win a lottery and as your prize, you get to choose between 100 euro today or 281 euro a year from now. What do you choose?

100 euro today

281 euro a year from now

Q9: You roll a dice twice and get 6 both times. What is the likelihood of getting 6 the third time you roll it?

[comment field]

Q10: You are given a choice between two options: With Option A you have a 100 % chance of losing 5 euro, with option B a 50 % chance of losing 10 euro but also a 50 % chance of not losing anything. What do you choose?

Option A

Option B

Doesn’t matter

Q11: What is 7 x 7 x 2?

98

55

78

28

Q12: You win a lottery and as your prize, you get to choose between 100 euro today or 109 euro a month from now. What do you choose?

100 euro today

109 euro a month from now

Q13: What is 4 x 5 x 2

40

20

11

58

Q14: You are given a choice between two options: With Option A you have a 100 % chance of winning 20 euro, with option B a 50 % chance of winning 40 euro but also 50 % chance of winning nothing. What do you choose?

Option A

Option B

Doesn’t matter

Q15: You want to go see a movie in the cinema, but while booking your ticket online you absentmindedly book a ticket for another movie that you don’t even like, and to make matters worse, the ticket was both expensive and non-refundable. Do you still go to the movie?

Yes

No

Q16: What age range are you in?

18-23

24-35

36-64

Over 64

Q17: You are a given a choice between two options: With Option A you have a 100 % chance of winning 100 euro, with option B a 50 % chance of winning 200 euro but also 50 % chance of winning nothing. What do you choose?

Option A

Option B

Doesn’t matter

Q18: You toss a coin six times, and it comes up heads all six times. What is the most likely outcome of the next coin toss?

Heads

Tails

No difference

Q19: You’re visiting a very expensive restaurant that you’ve been looking forward to go to for a very long time, and the meal is as great as you expected – however, your portion is gigantic and after finishing just over half, you already feel really full. The restaurant unfortunately does not have any bags that you could bring the food home in, so unless you eat the rest of this expensive meal, it goes to waste. What do you do?

Finish the meal

Leave it

Q20: You are a given a choice between two options: With Option A you have a 100 % chance of winning 10 euro, with option B a 50 % chance of winning 20 euro but also 50 % chance of winning nothing. What do you choose?

Option A

Option B

Doesn’t matter

Q21: Imagine that you won 100 000 in a lottery. How long do you think this would make you happier than you are now?

Less than 3 months

3-6 months

6-9 months

9-12 months

I would be permanently happier

Q22: Have you ever been employed?

Yes, I’ve had full-time employment or both full- and part-time employment

Yes, but only part-time employment.

No

Q23: You’ve just arrived at a roulette table and you’re getting ready to play when the dealer makes you an offer: He’ll tell you the results of the last 10 spins so you can determine which numbers are hot and cold, in exchange for you giving up 1 % of your winnings if you do win. There is no other way to find out this information, and the dealer’s offer is perfectly legal. Do you accept the deal?

Yes

No

Q24: What country do you live in?

[Comment field]

Q25: You’re going to a club that has a 10 euro cover fee. You like the club so you think the fee is worth it. But shortly after you get in you head out for just a few minutes, and when you try to get back in your ticket is gone and the bouncer refuses to believe you when you insist you already paid. You’re not in a bad spot economically so you could afford to simply pay the fee again, annoying as it is, or you could go somewhere else. What do you do?

Pay the cover fee again

Go somewhere else

Q26: What is the highest level of education you have achieved? If you're a student, pick the option you are currently studying for.

High school or less

Undergraduate degree or equivalent

Postgraduate degree or equivalent

Q27: You win a lottery and as your prize, you get to choose between 100 euro today or 102 euro a month from now. What do you choose?

100 euro today

102 euro a month from now

Q28: You are a given a choice between two options: With Option A you have a 100 % chance of losing 100 euro, with option B a 50 % chance of losing 200 euro but also 50 % chance of losing nothing. What do you choose?

Option A

Option B

Doesn’t matter

Q29: Do you usually eat more just to “get your money’s worth” at all-you-can-eat buffets?

Yes

No

Q30: Suppose you were given a 25 % pay increase (or grant/dole increase if you’re a student/unemployed) – for how long do you think this may make you happier than you currently are? Note: The pay increase is permanent, not temporarily.

Less than 1 month

1 to 3 months

4 to 12 months

I would be permanently happier

PART 2

Below you will find a number of questions regarding your physical and mental health as well as some questions on your personal economy. The following nine questions are known as the PHQ-9 test.

PLEASE KEEP IN MIND THAT THESE ANSWERS ARE 100 % ANONYMOUS AND CANNOT BE TRACED BACK TO YOU.

Over the last two weeks, how often have you been bothered by any of the following problems?

Q31: Little interest or pleasure in doing things?

Not at all

Several days

More than half the days

Nearly every day

Q29: Feeling down, depressed or helpless?

Not at all

Several days

More than half the days

Nearly every day

Q30: Trouble falling or staying asleep, or sleeping too much?

Not at all

Several days

More than half the days

Nearly every day

Q31: Feeling tired or having little energy?

Not at all

Several days

More than half the days

Nearly every day

Q32: Poor appetite or overeating?

Not at all

Several days

More than half the days

Nearly every day

Q33: Feeling bad about yourself – or that you are a failure or have let yourself or your family down?

Not at all

Several days

More than half the days

Nearly every day

Q34: Trouble concentrating on things, such as reading the newspaper or watching television?

Not at all

Several days

More than half the days

Nearly every day

Q35: Moving or speaking so slowly that other people could have noticed? Or the opposite – being so fidgety or restless that you have been moving around a lot more than usual?

Not at all

Several days

More than half the days

Nearly every day

Q36: Thoughts that you would be better off dead, or of hurting yourself in some way?

Not at all

Several days

More than half the days

Nearly every day

Q37: Do you smoke or use other forms of tobacco?

Yes

No

Q38: How much alcohol do you consume in an average week? NOTE: One unit equals approximately half a pint of beer (4 %), half a glass of wine, one third of a glass of cider (4.5 %) or one 25 ml glass of spirits. Hence, if you drink 5 beers in a week, you're consuming just over 10 units of alcohol that week.

0-4 units

5-9 units

9-14 units

15-20 units

21-25 units

Over 25 units

Q39: What is your annual income (before taxes)? Please include which currency you’re paid in.

[Comment field]

Q40: Do you suffer from obesity and/or Type II diabetes?

Yes

No

Q41: Are you currently working?

Yes

No, I’m unemployed

No, I’m a student

No, I’m retired

Q42: Do you save regularly?

Yes, privately or both privately and through a pension plan

Yes, through a pension plan

No

Q43: Approximately how often have you used illegal drugs in the past year?

Never

1-4 times

5-12 times

13-26 times

Over 26 times

Q44: Do you plan your spending ahead of time and then keep to the plan (that is, do you stick to a budget)?

Yes

No

Q45: How much attention did you pay to this survey?

None or very little of my attention

Some of my attention

My full attention

Q46: Do you have any feedback? If so, please use this field.

[Comment field]

Q46: By ticking this box and submitting this survey, you are also confirming that

o You agree to have you responses stored, and processed in a manner compatible with the purposes of this research.

- You understand that this anonymous data will be held for ten years by Maynooth University

ACKNOWLEDGEMENT

Thank you for taking part in this survey. It is recognized that some of these questions may have caused distress. If this is the case for you, please do not hesitate to seek help.

Appendix B: ADDITIONAL STATISTICAL ANALYSIS

TABLE 11: DEPRESSION (INCLUDING UNBIASED VARIABLE)[4]

VARIABLES	(1) depression	(2) nodepression
unbiased	0.715*	1.110
	(0.0841)	(0.505)
undergrad	0.600**	1.697**
	(0.0401)	(0.0161)
postgrad	0.277***	2.707***
	(9.03e-06)	(9.00e-06)
age2435	0.577**	2.365***
	(0.0372)	(0.000773)
age3664	0.385***	4.613***
	(0.00129)	(1.48e-08)
over64	0.109***	8.935***
	(0.000191)	(2.67e-09)
Anglosphere	1.361	0.706*
	(0.251)	(0.0914)
developingcountry	1.316	0.683
	(0.563)	(0.320)
otherlocation	1.947*	0.491**
	(0.0982)	(0.0297)
patiencescore	0.822**	0.941
	(0.0123)	(0.323)
Constant	1.260	0.330***
	(0.558)	(0.00133)
Observations	852	852
Pseudo R2	0.125	0.122

[4] Notes on Tables 11-15: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

TABLE 12: INCOME (INCLUDING UNBIASED VARIABLE)

VARIABLES	(1) highincome	(2) abovemedianincome	(3) lowincome
unbiased	2.056***	2.062***	0.568**
	(0.00264)	(0.000570)	(0.0195)
undergrad	3.062**	2.348***	0.610
	(0.0283)	(0.00761)	(0.129)
postgrad	7.689***	4.704***	0.276***
	(3.52e-05)	(1.08e-06)	(0.000197)
age2435	1.816e+06	5.327**	0.250**
	(0.981)	(0.0338)	(0.0116)
age3664	3.125e+06	12.50***	0.132***
	(0.981)	(0.00147)	(0.000366)
over64	7.902e+06	36.37***	0.0961***
	(0.979)	(0.000215)	(0.00750)
Anglosphere	2.015**	1.248	1.274
	(0.0135)	(0.391)	(0.420)
developingcountry	0.187	0.105***	15.41***
	(0.127)	(0.00655)	(1.74e-05)
otherlocation	0.389	0.234**	5.796***
	(0.234)	(0.0186)	(0.00142)
patiencescore	1.130	1.044	0.944
	(0.192)	(0.609)	(0.551)
Constant	1.19e-08	0.0250***	4.289**
	(0.976)	(1.92e-05)	(0.0282)
Observations	477	477	477
Pseudo R2	0.137	0.145	0.160

TABLE 13: DRUG USE AND ALCOHOL CONSUMPTION (INCLUDING UNBIASED VARIABLE)

VARIABLES	(1) druguse	(2) alcoholdanger	(3) alcoholoveruse
unbiased	0.735	0.901	0.785*
	(0.118)	(0.575)	(0.0948)
undergrad	0.790	0.896	1.203
	(0.386)	(0.679)	(0.381)
postgrad	0.502**	0.899	1.019
	(0.0218)	(0.683)	(0.930)
age2435	1.246	1.604	1.524*
	(0.405)	(0.222)	(0.0998)
age3664	0.656	2.601**	2.243***
	(0.163)	(0.0131)	(0.00218)
over64	0.198**	5.610***	2.797***
	(0.0149)	(8.54e-05)	(0.00160)
Anglosphere	2.573***	1.151	0.617**
	(0.000550)	(0.585)	(0.0143)
developingcountry	1.576	0.722	0.677
	(0.373)	(0.524)	(0.281)
otherlocation	3.382***	0.934	0.933
	(0.00172)	(0.882)	(0.826)
patiencescore	0.836**	1.063	0.982
	(0.0239)	(0.412)	(0.745)
Constant	0.348***	0.0885***	0.546*
	(0.00977)	(1.41e-07)	(0.0664)
Observations	852	852	852
Pseudo R2	0.126	0.0382	0.0372

TABLE 14: SMOKING AND OBESITY/TYPE II DIABETES (INCLUDING UNBIASED VARIABLE)

VARIABLES	(1) smoking	(2) obesity
unbiased	0.792	0.700
	(0.159)	(0.114)
undergrad	0.839	1.057
	(0.445)	(0.864)
postgrad	0.691	0.799
	(0.106)	(0.487)
age2435	1.751*	5.530***
	(0.0807)	(0.00228)
age3664	2.014**	9.582***
	(0.0326)	(6.15e-05)
over64	1.225	14.26***
	(0.610)	(3.19e-05)
Anglosphere	0.499***	2.565***
	(0.00251)	(0.00107)
developingcountry	0.364**	1.130
	(0.0444)	(0.847)
otherlocation	0.508*	0.920
	(0.0860)	(0.897)
patiencescore	0.968	0.840*
	(0.619)	(0.0557)
Constant	0.410**	0.0225***
	(0.0235)	(3.46e-09)
Observations	852	852
Pseudo R2	0.0390	0.0627

TABLE 15: BUDGETING, SAVING AND UNEMPLOYMENT (INCLUDING UNBIASED VARIABLE)

VARIABLES	(1) yesbudget	(2) saveprivate	(3) unemployed
unbiased	0.759*	0.820	1.361
	(0.0530)	(0.188)	(0.367)
undergrad	0.994	0.985	0.599
	(0.976)	(0.945)	(0.273)
postgrad	0.944	1.333	0.648
	(0.779)	(0.192)	(0.351)
age2435	0.935	0.887	1.352
	(0.776)	(0.622)	(0.661)
age3664	0.657*	0.839	1.271
	(0.0905)	(0.494)	(0.735)
over64	0.770	0.219***	
	(0.408)	(3.86e-06)	
Anglosphere	0.594***	0.515***	1.239
	(0.00784)	(0.00140)	(0.616)
developingcountry	0.747	0.635	1.741
	(0.407)	(0.218)	(0.496)
otherlocation	0.791	0.305***	5.625***
	(0.458)	(0.000239)	(0.00224)
patiencescore	1.050	1.303***	0.897
	(0.383)	(1.10e-05)	(0.407)
Constant	2.111**	1.885*	0.0801***
	(0.0182)	(0.0540)	(0.00255)
Observations	852	852	538
Pseudo R2	0.0112	0.0577	0.0358

TABLE 16: DEPRESSION (CONTROL VARIABLES INCLUDED)[5]

	(1)	(2)
VARIABLES	depression	nodepression
PresentBias	0.883	1.270
	(0.540)	(0.151)
lossaverse	1.338	0.916
	(0.168)	(0.606)
impactbias	0.806	1.386*
	(0.345)	(0.0721)
gamblersfallacy	1.587*	0.833
	(0.0548)	(0.378)
sunkcost	1.802***	0.501***
	(0.00834)	(0.000390)
undergrad	0.579**	1.836***
	(0.0324)	(0.00701)
postgrad	0.287***	2.742***
	(2.16e-05)	(1.00e-05)
age2435	0.622*	2.166***
	(0.0782)	(0.00301)
age3664	0.433***	3.882***
	(0.00685)	(9.58e-07)
over64	0.117***	7.613***
	(0.000419)	(8.30e-08)
Anglosphere	1.329	0.726
	(0.296)	(0.126)
developingcountry	1.065	0.805
	(0.898)	(0.582)
otherlocation	1.702	0.543*
	(0.196)	(0.0677)
patiencescore	0.808***	0.972
	(0.00826)	(0.668)
Constant	0.848	0.345***
	(0.704)	(0.00578)
Observations	852	852
Pseudo R2	0.139	0.138

[5] Notes on Tables 16-20: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

TABLE 17: INCOME (CONTROL VARIABLES INCLUDED)

VARIABLES	(1) highincome	(2) abovemedianincome	(3) lowincome
PresentBias	0.986	1.191	0.955
	(0.955)	(0.439)	(0.859)
lossaverse	0.559**	0.687*	2.001***
	(0.0242)	(0.0946)	(0.00705)
impactbias	1.004	0.777	0.850
	(0.986)	(0.271)	(0.548)
gamblersfallacy	0.642	0.571*	2.236**
	(0.217)	(0.0599)	(0.0109)
sunkcost	0.544*	0.420***	1.820**
	(0.0650)	(0.00169)	(0.0402)
undergrad	3.371**	2.525***	0.577*
	(0.0180)	(0.00439)	(0.0993)
postgrad	8.184***	4.878***	0.265***
	(2.32e-05)	(8.57e-07)	(0.000186)
age2435	2.320e+06	4.991**	0.227***
	(0.983)	(0.0442)	(0.00874)
age3664	3.945e+06	11.04***	0.118***
	(0.982)	(0.00289)	(0.000303)
over64	9.373e+06	29.20***	0.0767***
	(0.981)	(0.000615)	(0.00498)
Anglosphere	2.140***	1.304	1.207
	(0.00837)	(0.312)	(0.539)
developingcountry	0.222	0.125**	13.77***
	(0.166)	(0.0117)	(4.31e-05)
otherlocation	0.374	0.237**	5.601***
	(0.221)	(0.0218)	(0.00276)
patiencescore	1.199*	1.116	0.929
	(0.0813)	(0.223)	(0.464)
Constant	1.58e-08	0.0454***	2.577
	(0.979)	(0.000507)	(0.170)
Observations	477	477	477
Pseudo R2	0.139	0.156	0.184

TABLE 18: DRUG USE AND ALCOHOL CONSUMPTION (CONTROL VARIABLES INCLUDED)

VARIABLES	(1) druguse	(2) alcoholdanger	(3) alcoholoveruse
PresentBias	1.305	1.178	1.685***
	(0.202)	(0.417)	(0.000796)
lossaverse	0.944	0.912	1.092
	(0.789)	(0.651)	(0.575)
impactbias	1.050	1.108	1.019
	(0.828)	(0.623)	(0.910)
gamblersfallacy	1.105	0.594**	0.710*
	(0.701)	(0.0470)	(0.0740)
sunkcost	1.245	0.955	1.050
	(0.329)	(0.860)	(0.797)
undergrad	0.782	0.863	1.173
	(0.372)	(0.584)	(0.457)
postgrad	0.506**	0.854	0.967
	(0.0250)	(0.546)	(0.873)
age2435	1.234	1.512	1.403
	(0.431)	(0.288)	(0.192)
age3664	0.677	2.581**	2.210***
	(0.212)	(0.0166)	(0.00366)
over64	0.214**	6.116***	3.041***
	(0.0219)	(6.71e-05)	(0.00106)
Anglosphere	2.655***	1.189	0.636**
	(0.000393)	(0.507)	(0.0238)
developingcountry	1.567	0.791	0.763
	(0.384)	(0.649)	(0.463)
otherlocation	3.465***	1.021	1.018
	(0.00152)	(0.965)	(0.956)
patiencescore	0.850*	1.062	1.020
	(0.0509)	(0.456)	(0.751)
Constant	0.222***	0.0859***	0.343***
	(0.000870)	(1.79e-06)	(0.00418)
Observations	852	852	852
Pseudo R2	0.126	0.0449	0.0478

TABLE 19: SMOKING AND OBESITY/TYPE II DIABETES (CONTROL VARIABLES INCLUDED)

VARIABLES	(1) smoking	(2) obesity
PresentBias	0.753	0.842
	(0.102)	(0.465)
lossaverse	1.034	1.028
	(0.854)	(0.908)
impactbias	1.144	0.898
	(0.467)	(0.678)
gamblersfallacy	1.631**	1.022
	(0.0175)	(0.939)
sunkcost	0.834	1.553
	(0.423)	(0.124)
undergrad	0.887	0.985
	(0.606)	(0.962)
postgrad	0.741	0.782
	(0.195)	(0.451)
age2435	1.791*	6.030***
	(0.0720)	(0.00151)
age3664	1.863*	11.01***
	(0.0634)	(3.32e-05)
over64	1.037	16.78***
	(0.929)	(1.84e-05)
Anglosphere	0.502***	2.537***
	(0.00288)	(0.00127)
developingcountry	0.337**	1.044
	(0.0320)	(0.946)
otherlocation	0.488*	0.873
	(0.0719)	(0.835)
patiencescore	0.944	0.811**
	(0.401)	(0.0231)
Constant	0.407**	0.0193***
	(0.0367)	(8.00e-09)
Observations	852	852
Pseudo R2	0.0470	0.0632

TABLE 20: BUDGETING, SAVING AND UNEMPLOYMENT (CONTROL VARIABLES INCLUDED)

VARIABLES	(1) yesbudget	(2) saveprivate	(3) unemployed
PresentBias	0.950	1.503**	0.764
	(0.735)	(0.0104)	(0.445)
lossaverse	1.208	1.119	0.671
	(0.224)	(0.494)	(0.308)
impactbias	1.248	1.074	0.618
	(0.172)	(0.680)	(0.249)
gamblersfallacy	1.555**	1.159	2.208**
	(0.0197)	(0.450)	(0.0493)
sunkcost	0.926	0.964	0.872
	(0.675)	(0.850)	(0.753)
undergrad	1.050	1.011	0.640
	(0.813)	(0.959)	(0.349)
postgrad	0.998	1.345	0.732
	(0.994)	(0.183)	(0.508)
age2435	0.922	0.837	1.304
	(0.735)	(0.472)	(0.703)
age3664	0.603**	0.788	1.079
	(0.0489)	(0.370)	(0.917)
over64	0.671	0.208***	
	(0.222)	(3.90e-06)	
Anglosphere	0.590***	0.526***	1.290
	(0.00752)	(0.00215)	(0.551)
developingcountry	0.701	0.660	1.361
	(0.319)	(0.265)	(0.715)
otherlocation	0.765	0.312***	5.850***
	(0.399)	(0.000336)	(0.00209)
patiencescore	1.040	1.360***	0.933
	(0.516)	(1.27e-06)	(0.609)
Constant	1.650	1.174	0.115**
	(0.155)	(0.660)	(0.0122)
Observations	852	852	538
Pseudo R2	0.0161	0.0629	0.0555

TABLE 21: CORRELATION COEFFICIENTS[6]

	patiencescore	**saveprivate**	**druguse**
age1823	-0.1169***	-0.0287	0.1471***
	(0.0006)	(0.4035)	(0.0000)
age2435	0.0199	0.1364***	-0.1466***
	(0.5620)	(0.0001)	(0.0000)
age3664	0.0199	0.1364***	-0.1466***
	(0.5620)	(0.0001)	(0.0000)
over64	0.1517***	-0.1405***	-0.1518***
	(0.0000)	(0.0000)	(0.0000)
patiencescore	1***	0.1372***	-0.1361***
	(0.0000)	(0.0001)	(0.0001)
saveprivate	0.1372***	1***	-0.0410
	(0.0001)	(0.0000)	(0.2319)
druguse	-0.1361***	-0.0410	1***
	(0.0001)	(0.2319)	(0.0000)
depression	-0.1426***	-0.1515***	0.0975***
	(0.0000)	(0.0000)	(0.0044)
unemployed	-0.0277	-0.1011***	0.0458
	(0.4186)	(0.0031)	(0.1821)
smoking	-0.0037	0.0161	0.1454***
	(0.9140)	(0.6390)	(0.0000)
obesity	-0.0769**	-0.0663*	-0.0408
	(0.0249)	(0.0530)	(0.2336)
alcoholdanger	0.0545	-0.0736**	0.1008***
	(0.1121)	(0.0317)	(0.0032)
	depression	**unemployed**	**smoking**
age1823	0.2392***	-0.0479	-0.1132***
	(0.0000)	(0.1623)	(0.0009)
age2435	-0.1151***	0.0594*	0.1342***
	(0.0008)	(0.0829)	(0.0001)
age3664	-0.1151***	0.0594*	0.1342***
	(0.0008)	(0.0829)	(0.0001)
over64	-0.1486***	-0.0897***	-0.0188
	(0.0000)	(0.0088)	(0.5843)
patiencescore	-0.1426***	-0.0277	-0.0037
	(0.0000)	(0.4186)	(0.9140)

[6] Notes on Table 21: Numbers below correlation coefficients are p-values. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level.

saveprivate	-0.1515***	-0.1011***	0.0161
	(0.0000)	(0.0031)	(0.6390)
druguse	0.0975**	0.0458	0.1454***
	(0.0044)	(0.1821)	(0.0000)
depression	1***	0.1552***	0.0094
	(0.0000)	(0.0000)	(0.7839)
unemployed	0.1552***	1***	-0.0164
	(0.0000)	(0.0000)	(0.6323)
smoking	0.0094	-0.0164	1***
	(0.7839)	(0.6323)	(0.0000)
obesity	0.0692**	-0.0107	0.0076
	(0.0435)	(0.7541)	(0.8248)
alcoholdanger	-0.0396	-0.0052	0.2296***
	(0.2478)	(0.8800)	(0.0000)
	obesity	**alcoholdanger**	
age1823	-0.1075***	-0.1014***	
	(0.0017)	(0.0031)	
age2435	0.0456	0.0239	
	(0.1833)	(0.4851)	
age3664	0.0456	0.0239	
	(0.1833)	(0.4851)	
over64	0.0304	0.1598***	
	(0.3758)	(0.0000)	
patiencescore	-0.0769**	0.0545	
	(0.0249)	(0.1121)	
saveprivate	-0.0663*	-0.0736**	
	(0.0530)	(0.0317)	
druguse	-0.0408	0.1008***	
	(0.2336)	(0.0032)	
depression	0.0692**	-0.0396	
	(0.0435)	(0.2478)	
unemployed	-0.0107	-0.0052	
	(0.7541)	(0.8800)	
smoking	0.0076	0.2296***	
	(0.8248)	(0.0000)	
obesity	1***	0.0364	
	(0.0000)	(0.2890)	
alcoholdanger	0.0364	1***	
	(0.2890)	(0.0000)	

TABLE 22: CORRELATION COEFFICIENT (INCOME VARIABLES)

	highincome	**abovemedianincome**	**lowincome**
age1823	-0.1282***	-0.1850***	0.2411***
	(0.0050)	(0.0000)	(0.0000)
age2435	-0.0625	-0.1616***	0.1379***
	(0.1726)	(0.0004)	(0.0025)
age3664	0.0616	0.1748***	-0.2041***
	(0.1792)	(0.0001)	(0.0000)
over64	0.1162**	0.1221***	-0.0628
	(0.0111)	(0.0076)	(0.1707)
Postgrad	0.2471***	0.2286***	-0.1856***
	(0.0000)	(0.0000)	(0.0000)

TABLE 23: BOX-TIDWELL P-VALUES[7]

Variable	**P-value**
Depression	0.284
NoDepression	0.613
abovemedianincome	0.302
lowincome	0.361
highincome	0.131
druguse	0.851
alcoholdanger	0.165
alcoholoveruse	0.701
smoking	0.820
obesity	0.400
yesbudget	0.105
saveprivate	0.580
unemployed	0.732

[7] Table 23 displays the p-values for the Box-tidwell test of the hypothesis that the non-binary variable, patiencescore, is linearly related to the dependent variable, listed in the variable column.

TABLE 24: CORRELATION COEFFICIENTS, BIASES/WELFARE MEASURES[8]

	PresentBias	Lossaverse	impactbias	gamblersfallacy	sunkcost
Depression	0.0074	0.0233	-0.0227	0.0792**	0.1965***
	(0.8286)	(0.4966)	(0.5088)	(0.0209)	(0.0000)
Nodepression	0.0422	-0.0012	0.0418	-0.0265	-0.2485***
	(0.2189)	(0.9725)	(0.2232)	(0.4402)	(0.0000)
Highincome	-0.0251	-0.0606	-0.0160	-0.0635	-0.1030**
	(0.5850)	(0.1867)	(0.7277)	(0.1662)	(0.0244)
Abovemedianincome	0.0317	-0.0380	-0.0705	-0.0794*	-0.2044***
	(0.4899)	(0.4072)	(0.1241)	(0.0833)	(0.0000)
Lowincome	-0.0192	0.0895*	-0.0022	0.1119**	0.1752***
	(0.6754)	(0.0507)	(0.9611)	(0.0145)	(0.0001)
Druguse	0.0703**	-0.0101	0.0114	0.0025	0.1442***
	(0.0403)	(0.7694)	(0.7401)	(0.9413)	(0.0000)
AlcoholDanger	0.0094	-0.0096	0.0143	-0.0514	-0.0670*
	(0.7848)	(0.7798)	(0.6777)	(0.1336)	(0.0506)
Alcoholoveruse	0.1071***	0.0085	-0.0082	-0.0424	-0.0717**
	(0.0017)	(0.8041)	(0.8112)	(0.2166)	(0.0363)
Smoking	-0.0412	0.0027	0.0279	0.0838**	-0.0762**
	(0.2296)	(0.9377)	(0.4156)	(0.0144)	(0.0261)
Obesity	0.0027	0.0121	-0.0038	0.0199	0.0142
	(0.9364)	(0.7237)	(0.9109)	(0.5627)	(0.6791)
yesbudget	-0.0271	0.0405	0.0422	0.0783**	-0.0071
	(0.4295)	(0.2374)	(0.2187)	(0.0223)	(0.8352)
saveprivate	0.0511	0.0119	0.0166	-0.0150	-0.0174
	(0.1362)	(0.7279)	(0.6283)	(0.6620)	(0.6121)
Unemployed	-0.0209	-0.0347	-0.0493	0.0907**	-0.0012
	(0.6288)	(0.4216)	(0.2541)	(0.0355)	(0.9786)

[8] Numbers below correlation coefficients are p-values. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level.

TABLE 25: DEPRESSION (EXTREME BIAS VARIABLES)[9]

VARIABLES	(1) depression	(2) nodepression
PresentBias	0.805	1.351**
	(0.259)	(0.0473)
lossaverseextreme	0.834	1.219
	(0.550)	(0.380)
impactbias	0.849	1.267
	(0.449)	(0.152)
gamblerfallacyextreme	1.179	1.292
	(0.638)	(0.392)
sunkcostextreme	2.776***	0.384***
	(0.00219)	(0.00276)
patiencescore	0.740***	1.099
	(4.79e-05)	(0.106)
Constant	0.446***	1.003
	(0.000610)	(0.988)
Observations	852	852
Pseudo R2	0.0352	0.0148

TABLE 26: INCOME (EXTREME BIAS VARIABLES)

VARIABLES	(1) highincome	(2) abovemedianincome	(3) lowincome
PresentBias	1.049	1.380	0.720
	(0.838)	(0.115)	(0.155)
lossaverseextreme	0.718	1.546	0.873
	(0.343)	(0.143)	(0.698)
impactbias	0.920	0.793	0.871
	(0.730)	(0.272)	(0.576)
gamblerfallacyextreme	0.656	0.881	1.740
	(0.454)	(0.769)	(0.215)
sunkcostextreme	0.740	0.227***	5.440***
	(0.601)	(0.00943)	(0.000353)
patiencescore	1.208**	1.157*	0.863*
	(0.0384)	(0.0600)	(0.0904)
Constant	0.232***	0.670	0.526**
	(1.09e-05)	(0.147)	(0.0309)
Observations	477	477	477
Pseudo R2	0.0131	0.0283	0.0361

[9] Notes on Tables 25-29: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

TABLE 27: DRUG USE AND ALCOHOL CONSUMPTION (EXTREME BIAS VARIABLES)

VARIABLES	(1) druguse	(2) alcoholdanger	(3) alcoholoveruse
PresentBias	1.189	1.193	1.701***
	(0.383)	(0.370)	(0.000461)
lossaverseextreme	0.792	0.774	0.831
	(0.465)	(0.391)	(0.402)
impactbias	1.079	1.097	0.993
	(0.721)	(0.649)	(0.966)
gamblerfallacyextreme	1.013	1.124	1.286
	(0.972)	(0.749)	(0.380)
sunkcostextreme	3.198***	0.326*	0.778
	(0.000422)	(0.0641)	(0.434)
patiencescore	0.769***	1.147*	1.121*
	(0.000720)	(0.0773)	(0.0529)
Constant	0.292***	0.150***	0.459***
	(8.59e-07)	(0)	(0.000161)
Observations	852	852	852
Pseudo R2	0.0376	0.0113	0.0132

TABLE 28: SMOKING AND OBESITY/TYPE II DIABETES (EXTREME BIAS VARIABLES)

VARIABLES	(1) smoking	(2) obesity
PresentBias	0.837	0.865
	(0.293)	(0.525)
lossaverseextreme	1.091	0.788
	(0.723)	(0.521)
impactbias	1.162	0.982
	(0.407)	(0.941)
gamblerfallacyextreme	2.350***	0.713
	(0.00332)	(0.489)
sunkcostextreme	0.450*	0.165*
	(0.0757)	(0.0769)
patiencescore	0.980	0.806**
	(0.762)	(0.0143)
Constant	0.357***	0.241***
	(4.87e-06)	(4.58e-07)
Observations	852	852
Pseudo R2	0.0149	0.0202

TABLE 29: BUDGETING, SAVING AND UNEMPLOYMENT (EXTREME BIAS VARIABLES)

VARIABLES	(1) yesbudget	(2) saveprivate	(3) unemployed
PresentBias	0.948	1.589***	0.801
	(0.721)	(0.00276)	(0.517)
lossaverseextreme	1.167	1.150	1.024
	(0.479)	(0.542)	(0.963)
impactbias	1.213	1.108	0.633
	(0.227)	(0.540)	(0.266)
gamblerfallacyextreme	1.027	1.155	2.027
	(0.925)	(0.630)	(0.223)
sunkcostextreme	1.072	1.188	
	(0.824)	(0.604)	
patiencescore	1.044	1.324***	0.904
	(0.456)	(2.66e-06)	(0.441)
o.sunkcostextreme			-
Constant	1.081	0.670**	0.130***
	(0.694)	(0.0492)	(2.60e-06)
Observations	852	852	512
Pseudo R2	0.00271	0.0231	0.0124

TABLE 30: DEPRESSION (SCANDINAVIA-ONLY SAMPLE)[10]

VARIABLES	(1) depression	(2) nodepression
PresentBias	0.502**	1.911***
	(0.0354)	(0.00539)
lossaverse	1.142	1.116
	(0.701)	(0.656)
impactbias	0.678	1.417
	(0.309)	(0.181)
gamblersfallacy	0.775	1.258
	(0.531)	(0.419)
sunkcost	2.335**	0.337***
	(0.0408)	(0.000534)
patiencescore	0.764**	1.088
	(0.0288)	(0.353)
Constant	0.312***	1.498
	(0.00791)	(0.232)
Observations	456	456
Pseudo R2	0.0348	0.0391

TABLE 31: INCOME (SCANDINAVIA-ONLY SAMPLE)

VARIABLES	(1) highincome	(2) abovemedianincome	(3) lowincome
PresentBias	0.730	1.315	0.700
	(0.304)	(0.322)	(0.334)
lossaverse	0.590	1.098	1.307
	(0.118)	(0.746)	(0.471)
impactbias	0.664	0.676	0.758
	(0.227)	(0.176)	(0.494)
gamblersfallacy	0.793	0.608	3.014***
	(0.565)	(0.146)	(0.00654)
sunkcost	0.355**	0.280***	2.451**
	(0.0418)	(0.000838)	(0.0364)
patiencescore	1.097	1.019	1.181
	(0.460)	(0.868)	(0.278)
Constant	0.508	1.609	0.103***
	(0.152)	(0.261)	(0.000117)
Observations	276	276	276
Pseudo R2	0.0352	0.0466	0.0570

[10] Notes on Tables 30-34: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level.

TABLE 32: DRUG USE AND ALCOHOL CONSUMPTION (SCANDINAVIA-ONLY SAMPLE)

VARIABLES	(1) druguse	(2) alcoholdanger	(3) alcoholoveruse
PresentBias	0.986	0.877	1.308
	(0.971)	(0.598)	(0.189)
lossaverse	0.868	0.848	1.130
	(0.739)	(0.536)	(0.569)
impactbias	0.756	1.128	0.902
	(0.528)	(0.647)	(0.641)
gamblersfallacy	0.824	0.630	0.790
	(0.686)	(0.152)	(0.330)
sunkcost	1.944	1.007	0.971
	(0.168)	(0.986)	(0.923)
patiencescore	0.873	1.035	0.943
	(0.371)	(0.733)	(0.472)
Constant	0.118***	0.297***	1.133
	(0.000122)	(0.00129)	(0.685)
Observations	456	456	456
Pseudo R2	0.0127	0.00801	0.00706

TABLE 33: SMOKING AND OBESITY/TYPE II DIABETES (SCANDINAVIA-ONLY SAMPLE)

VARIABLES	(1) smoking	(2) obesity
PresentBias	0.770	0.766
	(0.230)	(0.424)
lossaverse	1.265	1.219
	(0.297)	(0.559)
impactbias	0.886	0.908
	(0.611)	(0.793)
gamblersfallacy	1.217	1.429
	(0.438)	(0.333)
sunkcost	0.978	1.288
	(0.945)	(0.591)
patiencescore	0.966	1.031
	(0.687)	(0.819)
Constant	0.544*	0.106***
	(0.0608)	(7.95e-06)
Observations	456	456
Pseudo R2	0.00692	0.00885

TABLE 34: BUDGETING, SAVING AND EMPLOYMENT (SCANDINAVIA-ONLY SAMPLE)

VARIABLES	(1) yesbudget	(2) saveprivate	(3) unemployed
PresentBias	1.096	1.928***	0.487
	(0.659)	(0.00297)	(0.158)
lossaverse	1.331	1.239	0.317
	(0.196)	(0.364)	(0.138)
impactbias	1.117	1.012	0.548
	(0.624)	(0.962)	(0.366)
gamblersfallacy	1.577*	1.288	1.935
	(0.0719)	(0.343)	(0.275)
sunkcost	0.907	2.307**	0.781
	(0.751)	(0.0313)	(0.754)
patiencescore	1.021	1.175*	0.831
	(0.799)	(0.0623)	(0.364)
Constant	1.046	0.835	0.190**
	(0.887)	(0.578)	(0.0164)
Observations	456	456	289
Pseudo R2	0.00964	0.0278	0.0564

TABLE 35: SENSITIVITY AND SPECIFICITY (NO CONTROL VARIABLES)[11]

	Sensitivity (%)	Specificity (%)	Area under ROC curve
depression	2.00	99.29	0.6711
NoDepression	86.05	35.12	0.6418
highincome	0	100	0.6112
abovemedianincome	72.95	48.50	0.6423
lowincome	5.83	96.64	0.6589
druguse	0	100	0.6435
alcoholdanger	0	100	0.5727
alcoholoveruse	28.80	80.50	0.5851
smoking	0	100	0.5808
obesity	0	100	0.5740
yesbudget	86.86	19.74	0.5631
saveprivate	94.61	11.78	0.5970
unemployed	0	100	0.5916

TABLE 36: SENSITIVITY AND SPECIFICITY (CONTROL VARIABLES INCLUDED)

	Sensitivity (%)	Specificity (%)	Area under ROC curve
depression	11.33	97.86	0.7596
NoDepression	84.88	47.62	0.7466
highincome	17.36	94.10	0.7519
abovemedianincome	71.72	63.95	0.7531
lowincome	35.00	94.68	0.7713
druguse	0.70	100	0.7495
alcoholdanger	0	100	0.6466
alcoholoveruse	44.80	71.91	0.6425
smoking	2.34	98.75	0.6558
obesity	0	100	0.6749
yesbudget	83.05	26.58	0.5830
saveprivate	88.48	25.80	0.6669
unemployed	0	100	0.6407

[11] Notes on Tables 35-36: The cutoff score for sensitivity and specificity is 0.5. The first column shows a list of dependent variables, with the columns next to it displaying the sensitivity, specificity and area under ROC curve for the regressions on these variables.

Appendix C: Maynooth University Research Ethics Committee letter of approval

MAYNOOTH UNIVERSITY RESEARCH ETHICS COMMITTEE
MAYNOOTH UNIVERSITY,
MAYNOOTH, CO. KILDARE, IRELAND

Maynooth University
National University of Ireland Maynooth

Dr Carol Barrett
Secretary to Maynooth University Research Ethics Committee

12 December 2017

John Gustavsson
Department of Economics, Finance and Accounting
Maynooth University

RE: Application for Ethical Approval for a project entitled: 'Happy, healthy, wealthy and rational: Are biases harmful?'

Dear John,

The Ethics Committee evaluated the above project and we would like to inform you that ethical approval has been granted.

Any deviations from the project details submitted to the ethics committee will require further evaluation. This ethical approval will expire on 31 December 2018.

Kind Regards,

Dr Carol Barrett
Secretary,
Maynooth University Research Ethics Committee

c.c. Professor Rowena Pecchenino,
Department of Economics, Finance and Accounting

Reference Number
SRESC-2017-087

The Marginal Benefit of Manipulation: Investigating paternalistic interventions in the context of intertemporal choice

Abstract

Libertarian paternalism (LP) has gained popularity in recent years as an alternative way for governments to induce consumers into making "good" decisions. Many, however, question the ethics of such interventions, calling them a form of psychological manipulation, and instead argue interventions should focus on expanding the information set available to consumers and encouraging consumers to reason their way to the right decision. Such interventions are known as Autonomy-Enhancing Paternalism. The question remains how effective such interventions are relative to LP interventions. In this paper I introduce the term Marginal Benefit of Manipulation (MBoM), the difference between the treatment effect of an LP and an AEP intervention. I find that the AEP intervention does not succeed in altering behavior, but while the LP intervention initially fares better it backfires towards the end of the survey and the treatment effect reverses. Neither intervention appears to have any greater effect on behavior beyond the immediate present, though the LP intervention achieved a greater degree of permanency than the AEP intervention.

1. Introduction

Libertarian paternalism (LP) is a divisive topic in behavioral economics. [12] First introduced by Thaler and Sunstein (2003), libertarian paternalism is a set of interventions intended to "nudge" consumers towards a certain action (or inaction) without limiting freedom of choice. This is done through "choice architecture", a process through which an architect (typically a policymaker) designs the choice process in such a way as to push consumers towards an action that the architect deems would be beneficial to the consumers. Examples include setting the desirable action as the default option, giving consumers a "cooling off" period during which they can reverse their decision free of charge, and public service announcements/informational campaigns intended to convince consumers to take (or avoid) a certain course of action.

While the idea of libertarian paternalistic interventions is appealing to some and anathema to others (see Mitchell 2005), two critical questions remain: Do such interventions work, and are they ethically justified?

In their book "*Nudge*", Thaler and Sunstein cite evidence for the efficacy of libertarian paternalism from areas as diverse as cafeterias to retirement saving plans. By designing a menu in such a manner that the healthy options are easily seen while the unhealthy options are less visible, consumers can be induced to choose a healthy option, while still having the choice of not doing so. And by allowing workers to opt

[12] The term is itself controversial, with some arguing that it is an oxymoron, and that no true libertarian could possibly support libertarian paternalism. Mitchell (2005) argues that since the cost of LP interventions are paid for by all consumers, but LP interventions only help those consumers who otherwise would have chosen "poorly", LP effectively redistributes wealth from the rational to the irrational, something that runs contrary to libertarian ideology. The source of conflict stems from a different interpretation of "libertarian", where critics take it to mean "in line with the libertarian political/economic ideology" and supporters take it to mean "relatively non-intrusive compared to other types of paternalism". Mitchell is correct that no paternalism (libertarian or otherwise) can ever be acceptable to a hardline libertarian, but he ignores that supporters of LP make no such claim.

out of a retirement saving plan instead of having to opt *in*, under-saving can be reduced. Through such measures policymakers may nudge consumers into what they consider to be the right direction, without having to resort to outright limiting choices.

While at first look this approach appears less invasive than traditional paternalistic measures such as sin taxes, it has not escaped criticism. Klick and Mitchell (2006) argue that libertarian paternalism may remove opportunities as well as incentives for consumers to learn how to make rational decisions, effectively making consumers less discerning and in need of more paternalism, potentially creating a vicious cycle. In a similar vein, Binder (2014) argues that nudges may put a consumer on a learning trajectory that he or she did not choose and that LP interventions may have dynamic effects that supporters have failed to investigate. Binder also argues that it is nearly impossible to determine what an "acceptable" level of rationality is and how big a deviation from the neoclassical model must be to justify a libertarian paternalistic intervention, creating a risk of a "slippery slope" situation. Additionally, one may of course question whether or not the neoclassical model is even the best model to begin with (Berg and Gigerenzer, 2010). Finally, Binder argues that any libertarian paternalistic intervention will tend to be conservative in nature, aimed at promoting behaviors considered correct by the culture and society at the time.

Defenders of libertarian paternalism argue that since framing is inevitable – the items on a menu have to be ordered in some way, after all – one may as well frame in such a manner as to help the individual make a (from the perspective of the choice architect) good decision (Thaler and Sunstein, 2003). This implicitly assumes that intentions do not matter. It can be argued that to accidentally place the salad at the top of the menu (or to do it for any non-LP reason, such as standard profit maximization) is not the same as to do it on purpose to make people eat salad. The latter creates a precedent for using psychological manipulation to help individuals do what is “right”, a precedent that can then be used to justify further interventions.

Proponents of libertarian paternalism often make the claim that what they are nudging consumers to do are the same things the consumers wished they had the willpower to do on their own; the nudges are, so to speak, in line with consumer metapreferences. Thaler and Sunstein in their aforementioned book note that the vast majority of smokers would like to quit, and so by imposing nudges that make it harder for them to smoke or to access cigarettes, policymakers would really be doing them a favor.

The problem with this argument is that metapreferences are not observable. While it is true that most smokers who have been surveyed claim to want to quit, we have no way of knowing whether they actually want to quit or whether they are merely stating what they believe to be the most socially acceptable position. They may claim to want to quit because they do not want to have to explain themselves and/or because they think it is what the person asking them wants to hear. Basing nudges on metapreferences means that we may unintentionally manipulate people into choices that are socially acceptable but not in line with their utility functions. Since metapreferences are so strongly influenced by cultural norms and beliefs one would, in order to accept this argument from LP proponents, essentially have to accept that all widely held beliefs are by definition correct – a rather extreme form of moral relativism and dictatorship of the majority.

This goes back to Binder's criticism of libertarian paternalism that it is inherently conservative and promotes whatever is considered correct behavior by the culture in the time and place where it is being applied. If nudges are based on metapreferences, which is almost certain given that they play such a prominent role in justifying their existence in the first place, then there is a high risk that these nudges will serve to reinforce cultural beliefs and stigmas.

Another key criticism against libertarian paternalism, advanced by Binder and Lades (2015), is that it is unethical to use psychological biases in policy interventions, *even* when this is done to benefit the consumer. They argue that consumers are not actually taught to act in a more rational manner by LP interventions – they are merely tricked into doing so. They suggest an alternative, restricted version of libertarian paternalism, which they call "autonomy-enhancing paternalism" (AEP). In order for an intervention to qualify under the criteria of AEP, the intervention must not rely on psychological biases and must instead work to strengthen the individual's autonomy (the ability to make an actual conscious decision) by, for example, providing more information (through public service announcements, etc.) or by preventing an individual from making a hasty decision, by for example introducing a mandatory waiting time between the purchase decision and the delivery of a good/service during which the individual can cancel the purchase. Traditional LP interventions such as the use of default options and framing are then off limits as their efficacy stems from psychological biases (status quo bias and the framing effect, respectively).

While it is clear that an ethical case can be made in favor of AEP over LP, the question remains whether or not LP treatments are more effective than AEP treatments, and if so to what extent. This defines "*the marginal benefit of* *manipulation*" (MBoM), the difference between the treatment effect of an LP treatment and an AEP treatment. This term is appropriate since LP interventions rely on psychological manipulation of consumers, while AEP interventions do not. The additional benefit offered by using an LP intervention is therefore the marginal benefit of using manipulation. In this paper I conduct an experiment the ostensible goal of which is to reduce the individual time discount rate, to measure the MBoM by randomly assigning participants into three groups: An AEP treatment group, an LP treatment group, and a control group. This random assignment allows the experiment to run under both the AEP and LP umbrella, allowing them to be compared directly. In the AEP treatment group, participants were presented with a list of arguments in favor of the larger-later option *and* a list of arguments in favor of the smaller-sooner option (see Appendix A), while in the LP treatment participants were instead given the larger-later option as the default option and had to check a box if they wanted to choose the smaller-sooner option. In the control treatment participants neither received arguments in favor of an option, nor were there any default options.

While the efficacy of a treatment in the immediate term is interesting, it is equally interesting from a policy viewpoint to determine to what extent the effect of a treatment outlives the treatment itself. A treatment that causes a small but permanent effect may be considered preferable to a treatment that causes a bigger effect which disappears as soon as the treatment is discontinued. For this reason, in my study all participants were invited to take part in a follow-up survey which they could complete (at the earliest) seven days after completing the first survey. In the follow-up survey everyone received the same tasks and information as the control group received in the first survey. Allcott and Rogers (2012) find that the treatment effect can outlast the treatment itself in a study on reducing energy consumption, however this treatment was a combination of AEP (they provided information on monthly energy usage) and LP (they used social pressure by pointing out to those who consumed more than their neighbors that they were doing so) and hence there is no way to know whether the permanency was caused by the AEP or the LP component, or both. There is also the problem of the transaction cost; a consumer who has switched to an energy-saving device after receiving a monthly report is unlikely to switch back (at least immediately) after the monthly reports end, but this does not apply in all intertemporal choice situations. In my experiment, there was no cost associated with choosing different options in the second survey than in the first survey (i.e., choosing the larger-later options in the first survey and the smaller-sooner options in the follow-up survey), which leads to a more accurate estimate of the permanency of the effect of the different treatments.

From the neoclassical model of time discounting (commonly known as the Discounted Utility [DU] model) introduced by Samuelson (1937) we would expect there to be no difference between the control group and the treatment groups as consumers have stable preferences (thus framing does not affect them) and full information (thus the AEP intervention adds nothing of value). Further we would not expect anyone who chooses the smaller-sooner option for the shortest delay (one week vs one month) to choose the larger-later reward for the longer delays as the implied annual interest rate on the shortest delay is higher than for any of the later delays, so a consumer with a constant discount rate (as per the DU model) who rejects the larger-later option in the tasks with the shortest delay would also reject it for the longer delays. Finally, the DU model implicitly assumes there to be no domain-specific discounting, which in the context of this experiment means that participants should choose the same course of action regardless of whether the reward consists of money or vouchers. Hence, no participant should, for example, choose to the larger-later option when asked to choose between €30 in one week and €50 in one month and then choose the smaller-sooner option when asked to choose between a €30 Amazon voucher in one week and a €50 Amazon voucher in one month.

The paper is organized as follows: In Section 2 I develop my methodology and discuss its limitations, in Section 3 I present the results from both surveys and discuss what they mean, and finally Section 4 contains my conclusions.

Hypotheses

I test two hypotheses: First, that LP and AEP both increase the likelihood that a participant opts for the larger-later options, and that these treatments will prove equally effective; that is, that there will not be a positive marginal benefit of manipulation.

Second, that the AEP treatment effect will still be present in the follow-up survey while the LP treatment effect will not. This hypothesis is based on Rogers and Frey (2014) who found that adding information could permanently change individual decision making, while to the best of my knowledge no evidence exists that default options are capable of this.

2. Methodology

To test the hypotheses concerning the relative efficacy of LP and AEP I conducted an online experiment using the platform Surveymonkey between the 27th of April and the 3rd of June 2015 with the original survey conducted between the 27th of April and 27th of May, and the follow-up survey conducted between the 4th of May and 3rd of June. A total of 535 participants completed the experiment, with 263 of those completing the follow-up survey. Participants were recruited mainly through social media websites including Facebook, Reddit, Twitter and Craigslist, and through an email invitation sent out to all economics, finance and accounting students at Maynooth University.

Following the incentive structure used by Coller and Williams (1999), this study used real incentives, with three randomly chosen participants being paid based on one pre-selected task.[13] Limiting the number of paid participants to three was done purely due to budget limitations. The randomly selected participants were contacted via email and paid through PayPal. Participants were informed about the incentive structure before agreeing to take part in the experiment but were neither informed of the hypotheses nor which task would determine the payment if they were one of the randomly chosen participants, as this may have biased the results. The "real" task was task number 4.

There is little evidence indicating that incentives matter in the context of intertemporal choice experiments as documented by an extensive review of the intemporal choice literature by Frederick, Loewenstein and O'Donoghue (2002). Coller and Williams (1999) found no difference between participants who were

[13] This experiment was self-funded.

offered real incentives and those were not, while Abdellaoui, Bleichrodt and l'Haridon (2013) found only small differences. Bickel et al. (2009) found, through a neuroimaging study, that responses to intertemporal choice tasks were the same regardless of whether incentives were offered or not. Even if participants were to display different discount rates depending on the type of incentives offered, this would not be of great concern seeing as how the purpose of the study was not to determine discount rates per se, but to determine the effect of various interventions. That is, as long as the type of incentive offered did not affect one treatment group differently from another, comparisons can still be made between groups to determine whether or not one treatment performed better than another. There is no intuitive reason to believe that this is not the case.

Participants were randomly assigned into one of three groups: the LP treatment group, the AEP treatment group, and the control group. Due to platform limitations no true randomization was possible. Instead, in the first part of the survey participants were asked in what part of the month they were born and based on that answer were assigned to one of the groups.

The second part of the experiment differed depending on into which group participants fell. All participants were asked to choose between receiving €30 in one week or €50 in one month, €30 in one month or €50 in 6 months, and €30 in 6 months or €50 in 12 months, and the choices were repeated with €30 and €50 Amazon and Apple vouchers being used instead, giving a total of nine tasks (exchange rates for US Dollars and Pound Sterling were provided). Amazon and Apple vouchers were used to complement the money tasks to mitigate the issue of participant choices being affected by operative liquidity constraints. That is, liquidity constrained individuals could choose the smaller-sooner option not because they are inherently impatient, but because they suffer from a shortage of liquid funds. While some individuals may choose the smaller-sooner option for the money tasks for that reason, it is highly unlikely that any individual desperately needs an Apple product and cannot delay receiving a voucher for that reason. The tasks in this experiment were inspired by Green, Myerson and McFadden (1997), Hesketh (2000) and Madden et al. (1997).

While choice tasks such as those used by this experiment provide less precision than other tasks, such as matching, they are preferable since they are the closest equivalent to the type of intertemporal choices faced by most consumers on a daily basis, that is, a choice between one fixed amount now and another fixed amount at a specific later point. There are very few, if any, real life situations where consumers are asked to “match” how much money at a later point is the equivalent of a certain amount in the present. Thus, as discussed by Frederick, Loewenstein and O’Donoghue (2002), participants in experiments tend to rely on heuristics to solve matching tasks, and this overuse of heuristics appears to be an experimental artifact. Matching tasks also require more time and effort which may reduce the number of participants who complete the experiment. Rating tasks were also considered but ultimately rejected as they too do not resemble any real-life situation and so are inferior in terms of generalizability, and also because they may be sensitive to extremeness aversion (Tversky and Simonson, 1993). The main reason for limiting the number of intertemporal choice tasks to nine was to ensure a high response rate and also because a high number of tasks may increase the risk of participants not paying attention. [14] Needless to say there are also very few real-life situations where

[14] Galesic and Bosnjak (2009) showed that there is a negative relationship between the number of questions and the survey completion rate.

participants are faced with dozens of intertemporal choice tasks at the same time, therefore had participants had to solve a large number of tasks, the generalizability of the experiment would have been reduced.

The AEP treatment group, those born in the last third of the month, was presented with a list of arguments in favor of the larger-later and the smaller-sooner option. The arguments, together with the rest of the survey, can be found in Appendix A. Additionally, participants in this group were asked which arguments they found most convincing for the smaller-sooner and larger-later option respectively to get a better idea about how individuals make intertemporal choice decisions.

The LP treatment group, those born in the second third of the month, was presented with the option to receive €50 (or a €50 voucher) in 1 month/6 months/12 months, or €30 (or a €30 voucher) in 1 week/1 month/6 months. For the latter option to be availed of, a box had to be checked. By requiring participants to make an active choice to receive the smaller-sooner option, this treatment relied on the default option bias (also known as the status quo bias) to nudge participants toward the larger-later option.

The control group, those born in the first third of the month, was neither provided with a default option nor presented with any arguments in favor of either option.

The third part of the survey was identical for all participants and consisted of a set of demographic questions covering age, country of residence, marital status, gender, education, saving and attitude toward saving and email address (participants were not required to share any identifying information). The survey also asked participants to rate Apple and Amazon on a scale from 1 (dislike) to 5 (strongly like).

For participants to be included in the statistical analysis of the Apple/Amazon voucher tasks, they had to have indicated that they at least somewhat liked the company (rated 2 or higher) as time discounting for losses (Thaler, 1981) and less desirable rewards (Tsukayama and Duckworth, 2010) have been shown to differ from that of more desirable rewards. In the regressions that included control variables, those participants who had answered "I'd rather not say" to any of the relevant questions (for example, participants who had refused to state their level of education) were dropped. As a result, the sample size used in the statistical analysis varies from 411 to 501 depending on the task and depending on whether or not control variables were included.

Additionally the final part of the survey included two questions to weed out inattentive and less serious participants. These questions were "Is water wet?" and "What is two plus three?" Participants who answered either of these questions incorrectly had their answers removed from the data analysis to strengthen the credibility of the results and conclusions from the study (this was inspired by de Haan and Linde (2011) who used a similar procedure).

As a final question, participants were asked to provide feedback and/or ask any questions they may have in a comment field. The purpose was two-fold: By allowing participants a chance to give feedback, future experimental designs may be improved, and also the feedback question may reveal that some participants had no idea what they were doing and/or did not take the survey seriously, in which case their answers would be removed, just like with the participants who failed to answer the trick questions correctly. Participants were also asked to indicate if they wanted to find out what the experiment discovered and all participants who indicated that they did and who had provided their email addresses received a summary of the conclusions by email.

All participants who provided their email addresses were invited to take part in the second survey exactly one week after they took part in the first survey. The second survey once again asked participants during what part of the month they were born, but the second part of the survey was identical regardless of what participants answered, as the purpose of the follow-up survey was to measure the permanence of the treatment effect(s) from the first survey.

The rewards, as mentioned above, were set at €30 and €50 or the voucher equivalent. Thaler (1981) showed that discount rates are negatively correlated with the size of the reward (*"the magnitude effect")*; meaning very small rewards would cause an overwhelming number of participants to choose the smaller-sooner option. Large rewards solve this problem but creates an additional two: Participants may not be used to making decisions involving large amounts of money, and this inexperience may affect their decision-making, while €30 and €50 are amounts that most people spend quite frequently. Also, had larger rewards been used, hypothetical incentives would have been necessary due to the limited budget of the study.

The purpose of conducting the experiment online was to allow for a larger, more diverse sample. This was achieved since the experiment had 535 participants from all age groups and several countries. The internet also allowed for a greater degree of anonymity than what can be provided by a regular lab experiment, potentially reducing the observer effect. Finally, since taking part in an online experiment requires less participant effort and time expenditure, online experiments can attract even those who would not volunteer to take part in a lab experiment, reducing the self-selection problem associated with such experiments.

Conducting an experiment online is, however, also associated with certain drawbacks. Wright (2005) identifies that there is a risk that participants could take part of the experiment multiple times, a risk that was mitigated by making it impossible to take the experiment more than once from the same device. While this is not a fool-proof measure, due to the relatively low incentives used in this experiment, it is unlikely that many participants found it worthwhile to take the experiment several times. Second, participants may be suspicious of financial incentives used in an online experiment (they may be afraid of being scammed), a risk which was mitigated by assuring participants before the experiment that they would not have to share any bank account details to receive payment.

Participants taking part in an online experiment may be more easily distracted during the course of the experiment than they would have been during a lab experiment, something which may affect their decision-making. This, however, may not be a disadvantage, as real-life economic decisions are often taken in “noisy” environments (i.e. shopping centers) where participants are distracted, and so this actually strengthens the generalizability of the experiment. Also having participants take part in an experiment from the comfort of their own home with their own computers (or other internet-connected devices) should increase the likelihood that they act naturally, again strengthening the generalizability of the experiment, though at the same time as discussed by Grimelikhuijsen and Meijer (2014), this hurts the internal validity as the experimenter cannot observe participants during the experiment and ensure that they are not, for example, getting input from their friends or from the internet.

There is no way to know whether any, and, if so, how many, participants were under the age of 18. Although the experiment was never advertised to children and the instructions on the first page made it clear that children ought not to take part, due to the lack of ability to verify the age of participants there is no way to know for certain whether children did take part. Further as discussed by Duda and Nobile (2010), online surveys struggle with unbiasedness as there is no representative samples of email addresses for the general population from which to draw, and not the entire population is online. This problem however is shared by lab experiments which generally use student-only samples.

Finally, unlike in a lab experiment, there is no way to pay participants in an online experiment immediately upon completion of the experiment. Hence, measuring very short-term discount rates is not possible in studies which use real incentives, such as this one. For that reason the shortest delay in this experiment was one week. While this means that some of the "present bias" (Laibson, 1997) is lost, this is not a major concern as discount rates appear to be falling for at least one year from the present (Frederick, Loewenstein and O'Donoghue, 2002), and therefore most of the present bias is preserved even though participants cannot choose to receive the reward immediately.

The dropout rate for this survey is not known. The responses of those participants who did not complete the survey was deleted, as was promised in the survey instructions. This was done for ethical reasons: Participants who did not complete the survey might not consent to having their data stored (i.e. they may have changed their minds since they started the survey). Never-the-less, this lack of data is a shortcoming as it may have indicated how successful the survey design was at keeping the dropout rate low.

3. Results

These results were obtained by estimating logistic regressions with the responses to the different choice tasks as the dependent variables. As there were nine different choice tasks, there are nine dependent variables. The coefficients are odds ratios representing the likelihood of a participant choosing the larger-later option relative to the control group.

The final sample size used in the statistical analysis varies between 411-501 for the original survey (see Methodology for details). In the follow-up survey, the sample size is 263.

Model specification and assumptions

Logistic regression model 1: No control variables

$\log Prob(Y=1)/(1 - Prob(Y=1)) = B_0 + B_{lp}d_{lp} + B_{aep}d_{aep}$

Logistic regression model 2: Control variables included

$\log Prob(Y=1)/(1 - Prob(Y=1)) = B_0 + B_{lp}d_{lp} + B_{aep}d_{aep} + B_{ageover23}d_{ageover23} + B_{male}d_{male} + B_{nopostgrad}d_{nopostgrad}$

where Y is a dependent variable based on the answers to a certain intertemporal choice task (see Tables 4-6).

As with any type of regression, there are certain assumptions that must hold true in order for a logistic regression – the type that was used for this research – to yield valid, reliable results.

First, because these are binary logistic regressions, the dependent variables must be binary variables. It is not hard to verify that this is indeed the case.

Second, observations must be independent of one another. There is no reason to believe that the answers of one respondent would not be independent of the other respondents as the participants did not know each other and it is unlikely that any of the participants took the survey multiple times given the low incentives involved and given that IP tracking ensured that a participant would have to use a different device or a proxy/VPN to do so.

Third, there must be little or no multicollinearity among the independent variables in a logistic regressions. Calculating the variance inflation factor (see Appendix B) confirms that this is indeed the case.

Fourth, logistic regression requires that all continuous independent variables be linearly related to the log odds. Since all independent variables are binary this condition does not apply.

Finally, logistic regression requires the sample size to be relatively large. Peduzzi et al. (1996) argued that as a guideline the sample size, N, should be at least equal to 10k/p, where k is the number of independent variables and p the smallest number of negative or positive cases in the population. Every regression that does not include control variables fulfills this criteria, which is why the conclusions of this study are mainly based on this set of regressions.

Table 1: Description of variables	
Control	1 if participant is in the control group, 0 otherwise
lp	1 if participant is in the libertarian paternalist group, 0 otherwise
aep	1 if participant is in the autonomy-enhancing paternalist group, 0 otherwise
onemmoney	1 if participant chose €50 in one month over €30 in one week, 0 otherwise
onemamazon	1 if participant chose a €50 Amazon voucher in one month over a €30 Amazon voucher in one week, 0 otherwise
onemapple	1 if participant chose a €50 Apple voucher in one month over a €30 Apple voucher in one week, 0 otherwise

sixmmoney	1 if participant chose €50 in six months over €30 in one month, 0 otherwise
sixmamazon	1 if participant chose a €50 Amazon voucher in six months over a €30 Amazon voucher in one month, 0 otherwise
sixmapple	1 if participant chose a €50 Apple voucher in six months over a €30 Apple voucher in one month, 0 otherwise
twelvemmoney	1 if participant chose €50 in twelve months over €30 in six months, 0 otherwise
twelvemamazon	1 if participant chose a €50 Amazon voucher in twelve months over a €30 Amazon voucher in six months, 0 otherwise
twelvemapple	1 if participant chose a €50 Apple voucher in twelve months over a €30 Apple voucher in six months, 0 otherwise
age1823	1 if participant is aged 18-23, 0 otherwise
age2435	1 if participant is aged 24-35, 0 otherwise
age3664	1 if participant is aged 36-64, 0 otherwise
ageover64	1 if participant is over the age of 64, 0 otherwise
single	1 if participant is single or not cohabitating with partner, 0 otherwise
married	1 if participant is married, in a civil union, or cohabitating with partner, 0 otherwise
male	1 if participant identifies as male, 0 otherwise
female	1 if participant identifies as female, 0 otherwise
highschool	1 if participant's highest achieved level of education is high school or less, 0 otherwise
undergrad	1 if participant's highest achieved level of education is an undergraduate degree, 0 otherwise
postgrad	1 if participant's highest achieved level of education is a postgraduate degree, 0 otherwise
ireland	1 if participant resides in Ireland, 0 otherwise
sweden	1 if participant resides in Sweden, 0 otherwise

usa	1 if participant resides in the USA, 0 otherwise
canada	1 if participant resides in Canada, 0 otherwise
uk	1 if participant resides in the UK, 0 otherwise
ausnz	1 if participant resides in Australia or New Zealand, 0 otherwise
euro	1 if participant resides anywhere else in Europe, 0 otherwise
other	1 if participant resides anywhere not listed above, 0 otherwise
ageover23	1 if participant is over the age of 23, 0 otherwise.
nopostgrad	1 if participant does not hold a postgraduate degree, 0 otherwise.

TABLE 2: Data summary, all participants (N=501)

Variable	Share (%)
Control	37,3
LP	30,9
AEP	31,7
onemmoney	79,4
onemamazon	91,2
onemapple	88,2
sixmmoney	53,1
sixmamazon	65,7
sixmapple	69,9
twelvemmoney	70,1
twelvemamazon	72,1
twelvemapple	70,9
age1823	49,9
age2435	34,1
age3664	13,2
ageover64	1,8
single	68,9
married	29,3
male	57,7
female	41,7
highschool	34,5
undergrad	47,7
postgrad	15,2
ireland	21,4
sweden	6,2
usa	44,9
canada	5
uk	8,6
ausnz	2,4
euro	6
other	3
ageover23	49,1
nopostgrad	82,2

As seen above, participants from vastly different backgrounds – in terms of age, nationality, education level, etc., – took part in the experiment. This is one relatively unique feature of this experiment as most experiments are only open to students (or only open to non-students), resulting in a homogenous sample not very representative of the overall population.

While this sample was more representative than most experiments, it should be noted that very few participants came from outside the western world, which is unfortunate but difficult to avoid as knowledge of English and access of internet tends to be lower outside the west. Caution is therefore advised before extrapolating any conclusions from this study to non-western populations and cultures.

TABLE 3: Data summary by treatment group

	Share (%)	
Variable	AEP (N=159)	LP (N=155)
Control	0	0
LP	0	100
AEP	100	0
onemmoney	75,5	86,5
onemamazon	90,6	92,3
onemapple	86,8	94,8
sixmmoney	50,3	62,6
sixmamazon	65,4	71
sixmapple	67,9	76,8
twelvemmoney	67,3	73,5
twelvemamazon	67,3	78,1
twelvemapple	66	77,4
age1823	48,4	51,6
age2435	35,8	36,8
age3664	12,6	11
ageover64	1,9	0
single	64,2	72,9
married	34	24,5
male	56	58,7
female	43,4	41,3
highschool	32,7	35,5
undergrad	49,1	47,1
postgrad	14,5	15,5
ireland	18,9	23,2
sweden	3,1	8,4
usa	50,9	40
canada	5,7	5,2
uk	7,5	9,7
ausnz	2,5	3,2
euro	5,7	5,2
other	3,1	2,6
ageover23	50,3	47,7
nopostgrad	81,8	82,6

As can be seen in the table above, randomization was overall successful with demographic groups being close to equally represented in both treatment groups. The exception being Americans and married people, who are overrepresented by about 10 % in the AEP group relative to the LP group.

TABLE 4: €30 (or voucher equivalent) in one week vs €50 (or voucher equivalent) in one month[15]

VARIABLES	(1) onemmoney	(2) onemamazon	(3) onemapple
lp	1.905**	1.212	2.831**
	(0.0273)	(0.626)	(0.0143)
aep	0.919	1.220	1.157
	(0.738)	(0.613)	(0.662)
Constant	3.349***	9.765***	5.520***
	(0)	(0)	(0)
Observations	501	492	429
Pseudo R2	0.0143	0.00122	0.0233

The LP treatment is clearly significant in the first task, increasing the odds of a participant choosing the larger-later option by approximately 90 per cent. The AEP treatment on the other hand is insignificant. Inclusion of control variables did not change which variables were and were not significant.

Neither treatment variable turned out to be significant in the first regression involving Amazon vouchers. Again, inclusion of control variables did not change this.

[15] Notes on Tables 4-9: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column.

In the first regression involving Apple vouchers, the LP treatment is strongly significant, while the AEP treatment again fails to have any impact on how participants choose, and this remained the case when control variables were included.

TABLE 5: €30 (or voucher equivalent) in one month vs €50 (or voucher equivalent) in six months

VARIABLES	(1) sixmmoney	(2) sixmamazon	(3) sixmapple
lp	1.842***	1.500*	1.630*
	(0.00580)	(0.0823)	(0.0643)
aep	1.115	1.192	0.969
	(0.614)	(0.440)	(0.898)
Constant	0.908	1.614***	2.019***
	(0.511)	(0.00164)	(2.44e-05)
Observations	501	492	429
Pseudo R2	0.0122	0.00483	0.00877

As with the previous "Money" regression (see Table 4), the LP treatment turns out to be significant with roughly the same positive odds ratio (1.84 vs 1.90). The AEP variable is still insignificant.

Like in the first regression involving Amazon vouchers, in the second one both treatment variables are insignificant. However, it should be noted that the LP variable is significant at a 10 per cent level and inches closer to significance at a 5 per cent level once control variables are included. In the second regression involving Apple vouchers, both treatments are insignificant but notably LP is significant once control variables are included.

TABLE 6: €30 (or voucher equivalent) in six months vs €50 (or voucher equivalent) in twelve months

VARIABLES	(1) twelvemmoney	(2) twelvemamazon	(3) twelvemapple
lp	1.219	1.401	1.433
	(0.295)	(0.356)	(0.386)
aep	0.902	0.885	0.874
	(0.209)	(0.211)	(0.221)
Constant	2.281***	2.519***	2.396***
	(0.362)	(0.413)	(0.412)
Observations	501	492	429
Pseudo R2	0.00248	0.00581	0.00667

Somewhat surprisingly, the LP variable is no longer significant once the choice is between a smaller-sooner reward in six months and a larger-later reward in twelve months. Inclusion of control variables made no difference.

Discussion of first survey results

What these results indicate is that while LP has a positive treatment effect initially, this effect appears to wear off once applied repeatedly. While it is of course technically possible that LP only works on intertemporal choices involving relatively short time periods, there is no intuitive reason to believe this to be the case; in fact, it could be argued that consumers ought to pay greater attention (and thus be more likely to go with the default option) when payoffs are further away as there is less at stake in the short term.

Instead, I believe the change may be to some extent explained by some relatively impatient participants at first “going along” with the default option. Towards the later tasks they may realize that they have picked the same (patient) option for each task and choose the smaller-sooner option on the final tasks as a way to “diversify” their consumption bundles to be somewhat more in line with their fundamentally impatient preferences (though further research is necessary to confirm whether this is the case). This would suggest that while LP can work in modifying a consumer choice in one situation, making drastic changes to the overall consumption bundle is more difficult. Expressed differently, it may be the case that impatient consumers always will be impatient in the end, even if a choice architect temporarily tricks them into making patient choices.

It is also possible that some participants realize towards the later tasks that they are being manipulated, and that this provokes a negative visceral reaction causing them to choose the smaller-sooner option for the later tasks. Expressed differently, people do not like being manipulated, and some act in the opposite way as to what the manipulator intended once they realize they are being manipulated.

Defenders of libertarian paternalism may invoke that participants merely get bored towards the end of the survey as they are being asked almost the same question several times and pick another option just to “mix things up”; however, the presence of real incentives means that participants have good reason to stay focused and not pick randomly, and any participant who got bored could exit the experiment by clicking the upper right-hand corner as it was conducted online.

The AEP treatment turned out to have no effect whatsoever on the choices made by participants. The likeliest explanation for this is that the AEP treatment provided participants with arguments in favor of both options. A naturally impatient participant may focus on the arguments in favor of the smaller-sooner option and use them as an “excuse” to follow his or her natural inclination, and vice versa for a patient participant. As such, the treatment may have merely reinforced the choices the participants were already leaning towards. It is worth noting that providing arguments for both options is not necessary for a treatment to qualify as AEP; this treatment is therefore a very un-intrusive treatment even by AEP standards.

A separate set of regressions was estimated using only those participants who stated that they reside in the United States. In this set, neither treatment was significant at a 5 per cent significance threshold. While this may suggest that Americans are more resilient to both types of interventions, caution is strongly advised as the sample size was very small (N=225 for the money tasks, 222 for the Amazon voucher tasks and 189 for the Apple voucher tasks).

Sensitivity and specificity were estimated and specificity in particular turned out to be generally poor (see Appendix B; Tables 19-20). This was to be expected as there are many variables that influence intertemporal choice that are unaccounted for in this model, the purpose of which is only to determine the relative efficacy of the LP and AEP treatment.

Only participants who provided their email addresses had a chance to be paid for their participation, on the basis of their response to one task. For those unwilling to provide their email addresses (see Appendix B, Table 22), the rewards in the experiment were in other words hypothetical. No difference was found between these groups, indicating that the presence of real incentives does not affect intertemporal choices.

The first hypothesis stated that both treatments would have a positive equal effect. These results indicate that while LP has a positive treatment effect initially, it also has a strongly negative effect if repeated excessively. The AEP treatment did not have a positive – or negative – effect at all. Overall, LP has a positive treatment effect on three of the intertemporal choice tasks, and a negative effect on two. In conclusion, the first hypothesis was incorrect.

On a final note, these results contradict the Discounted Utility model: Participants frequently turned down the larger-later option during the tasks with the shortest delay but choose it when delays were longer. The LP treatment clearly affected the way participants chose, and many participants chose differently when dealing with money compared to vouchers even when the delay was the same.

TABLE 7: €30 (or voucher equivalent) in one week vs €50 (or voucher equivalent) in one month (Follow-up survey)

VARIABLES	(1) onemmoney	(2) onemamazon	(3) onemapple
lp	1.196	1.167	2.761*
	(0.707)	(0.794)	(0.0845)
aep	0.457**	1.083	0.778
	(0.0458)	(0.887)	(0.540)
Constant	7***	12***	6.429***
	(5.28e-11)	(0)	(9.37e-11)
Observations	263	263	263
Pseudo R2	0.0270	0.000512	0.0290

In the first task, the LP treatment is clearly insignificant, while the AEP treatment actually appears to have a negative effect on participants' tendency to choose the larger-later option.

In the second task of the follow-up survey both variables are clearly insignificant.

In the third task, the LP variable is significant at a 10 per cent level, while the AEP variable is again clearly insignificant.

TABLE 8: €30 (or voucher equivalent) in one month vs €50 (or voucher equivalent) in six months (Follow-up survey)

VARIABLES	(1) sixmmoney	(2) sixmamazon	(3) sixmapple
lp	0.966	1.606	1.224
	(0.913)	(0.164)	(0.560)
aep	0.604*	1.071	0.816
	(0.0911)	(0.827)	(0.526)
Constant	1.737***	1.971***	2.586***
	(0.00671)	(0.00107)	(1.39e-05)
Observations	263	263	263
Pseudo R2	0.00946	0.00665	0.00418

In the second of the money tasks in the follow-up survey the LP treatment variable makes no significant impact, while the AEP variable has a *negative* impact at a 10 per cent significance level.

Neither treatment variable reaches the threshold of significance for the two voucher tasks with this delay.

TABLE 9: €30 (or voucher equivalent) in six months vs €50 (or voucher equivalent) in twelve months (Follow-up survey)

VARIABLES	(1) twelvemmoney	(2) twelvemamazon	(3) twelvemapple
lp	1.481	1.806*	1.345
	(0.232)	(0.0859)	(0.391)
aep	0.936	1.324	0.764
	(0.826)	(0.377)	(0.390)
Constant	1.737***	1.889***	2.355***
	(0.00671)	(0.00203)	(6.46e-05)
Observations	263	263	263
Pseudo R2	0.00623	0.00965	0.00811

In the third and final money task neither variable reaches the threshold of significance.

The LP variable is significant at a 10 per cent level in the final Amazon voucher task, which is interesting as this was one of the two tasks in the original survey in which the LP treatment failed to increase the likelihood of participants choosing the larger-later option.

In the final task both treatment variables turn out to be insignificant.

Discussion of follow-up survey results

As we can see in the follow-up survey the LP treatment fares better than the AEP treatment. It is possible that participants found it easier to remember the arguments in favor of the smaller-sooner option as they are relatively straight-forward, and so they stay with them longer than the arguments for the larger-later option. However, given the number of tasks, it cannot be ruled out that this is merely a coincidence. One limitation with the follow-up survey is that participants were not asked any demographic questions, nor were they asked how much they liked Amazon/Apple, and so, unlike in the main survey, there is no way to control for demographic variables or remove data from participants who disliked Amazon and/or Apple.

My second hypothesis stated that the AEP treatment effect would still be present in the follow-up survey while the LP treatment effect would not, but I have to concede that the data firmly rejects this hypothesis and, if anything, the opposite may be true.

4. Conclusions

These results indicate that while LP is clearly superior to AEP, the effect of an LP intervention may taper off when treatment is repeated frequently. While the AEP treatment was inefficient, the LP treatment was outright counterproductive in the later stages of the experiment, largely undoing its positive effect in the early stage. Binder's (2014) prediction that LP interventions may turn out to have adverse dynamic effects appears to be correct judging from this experiment. Neither treatment showed any greater degree of permanency, though, as discussed in the previous section LP did edge out AEP on this measure.

Proponents of libertarian paternalism may argue that the tapering off seen in the later stages of the survey is an experimental artifact. There are several reasons to believe that this is not the case. First of all, the likelihood of experimental artifacts is reduced substantially by the design of the experiment which, as explained in previous sections, allowed participants to take part in the experiment from the comfort of their own homes, thereby likely inducing more natural behavior. The cost of acting "defiantly", picking the smaller-sooner option instead of the larger-later option, was also much higher (an individual that did so lost out on hundreds of percent of interest) in this experiment than in most real-world situations, and so it is reasonable to assume it would actually be even more common in a real world setting. Finally, individuals face a number of intertemporal choices on an everyday basis (both economic and otherwise), and as such there are several situations where "nudges" would be used if libertarian paternalists had their way. Hence, it is not at all unreasonable to imagine an individual being exposed to several LP interventions during a single day. This experiment suggests that even if the first intervention works, this is far from a guarantee that later interventions will; the consumer choice basket appears to be sturdier than behavioral economists have understood it to be. In the real world there is no way to ensure that an individual is only exposed to one or a few nudges, which means there is no way to ensure that the effect of a nudge does not taper off.

Proponents may also assert that they would not advocate that interventions be done in this manner; that the efficacy wears off because the intervention is being carried out without the knowledge and consent of the participants involved. Had participants merely been informed that one option had been set as the default option for their own good, they would have understood and probably been grateful for the favor done to them by the choice architect, they may say. However, in the real world, it is virtually unheard of for libertarian paternalist interventions to follow these standards. While it is often true that consumers are able to find out about nudges that they are subject to, this requires them to actively seek out information on the topic. As most consumers do not even know what nudges are, it follows logically that most of them never think of looking up information about them. Thus, most consumers are being nudged without them being explicitly informed about them, just like in this experiment.

Finally, proponents might claim that these nudges were "obvious" and that such bold-faced manipulation of course would cause participants to realize that they were being tricked, leading some of them to react in a defiant manner by doing the opposite of what the manipulator clearly wanted them to do. However, in the real world while nudges may sometimes be more subtle, there are media outlets that would be more than happy to inform consumers of what they are being tricked into doing, making these nudges no harder to discover.

Beshears et al. (2009) reviewed the literature on the effect of a default option on retirement saving in the United States and conclude that the literature supports the idea that enrollment rates increase when the default option is to enroll. However, they do not discuss any literature on how many of those enrolled through automatic enrollment later go on to drop out of the plans they have enrolled in (assuming that is possible). It should be noted, however, that even if the dropout rate among those who had been automatically enrolled were no higher than among those who had to make an active choice to be enrolled, this does not in and of itself contradict the idea that the effect of nudging tapers off as there is technically nothing that prevents consumers to "compensate" by changing preferences in another domain (e.g. through a decrease in private saving).

If repeated nudging does in fact reduce the efficacy of nudging – regardless of the reason – there are potentially severe policy implications. In the real world, unlike in an experiment, we do not know exactly when a nudge will stop working and which nudges a consumer will be exposed to first and the most, making the results unpredictable. If a consumer is first exposed to a nudge that is intended to change a minor destructive behavior (i.e. overeating), this may make similar future nudges intended to change severely destructive behaviors (i.e. problem drinking) ineffective. This would suggest nudges should be reserved for severe issues, so as to not "waste" the potential of nudges on behaviors that are only somewhat destructive. It is near-impossible to say to what extent a nudge will prove to be welfare-improving without knowing its dynamic effect; a consumer that is nudged to save more now and as a result cannot be nudged to save more tomorrow will only be better off by the nudge today provided that saving more now and not tomorrow makes sense. An example when this may not be the case is when a consumer is low-paid today but high-paid tomorrow; in this case, saving today makes little sense as the marginal utility of consumption is likely to be high.

Furthermore, it is conceivable that nudging, if repeated enough times, could have an outright negative effect. If we assume that the tapering off seen in this experiment is due to some consumers “lashing out” upon realizing the manipulation, it is conceivable that even more consumers would have acted this way had there been more tasks and more nudges, causing the treatment effect of the LP intervention to become negative. Further research is necessary to explore this possibility.

I must, however, also conclude that this experiment does not support the idea that AEP would have a greater permanency than LP (which was my second hypothesis). It is possible that a “stronger” AEP treatment (for example, providing arguments only in favor of the larger-later option) may have produced better results, but that is a topic for future research.

Future research will further investigate the effect of LP and AEP interventions on different demographic groups and will also focus on determining whether or not transparency reduces the efficiency of LP interventions.

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Appendix A: Surveys

Welcome!

My name is John Gustavsson and I'm a research student at Maynooth University at the Department of Economics, Finance and Accounting.

This survey is an experiment that is part of the research I am doing for my thesis. In this survey, you will be asked a number of questions about how you value future income relative to present income – what we economists call "inter-temporal choice".

You will be posed with a number of scenarios and asked how you would act in them (there will be two options in each scenario). These are not purely hypothetical scenarios; three of you who answer this survey will be paid in accordance with how you answer one of the scenarios. The three who are paid will be randomly selected; your answers have no bearing on your likelihood of being one of them. The final part of this survey contains demographic questions (age, gender, country of residence, education and marital status) as well as some questions on consumer behavior and attitudes. If you are uncomfortable with answering a demographic question, simply choose the option "I'd rather not say" (or write N/A in the box) which is provided for every demographic question.

You will be asked to provide me with your email address at the end of the survey – this is so that I can contact you in case you are one of those who have been selected to be paid. You are not required to provide your email address, but if you don't I won't be able to pay you. You will not need to provide your bank account details to receive payment. The email addresses will be stored only until the selected participants have been paid, while the rest of the data will be retained for research purposes. You may quit the survey at any time; if you quit before finishing the survey, your data will be deleted. You can also withdraw your data at any time by emailing me at the email address provided below.

It must be recognized that, in some circumstances, confidentiality of research data and records may be overridden by courts in the event of litigation or in the course of investigation by lawful authority. In such circumstances the University will take all reasonable steps within law to ensure that confidentiality is maintained to the greatest possible extent.

Everyone who takes this survey will be invited back (by email) to take a shorter version of the survey again after 1 week. Retaking the survey is not mandatory. If you're interested in taking part of the findings of this study you're more than welcome to do so; simply indicate your interest when answering the final question.

If you have any questions or you wish to contact me for any reason, you can reach me at john.gustavsson.2010@mumail.ie.

You must be 18 or older to participate in this survey. This survey will take approximately 10-20 minutes to complete, obviously depending on how much time you spend thinking about your decisions.

By proceeding, you agree to take part in this survey, and have your data stored under the conditions outlined above. Thank you for your participation!

What time of the month is your birthday?

First third of the month

Second third of the month

Last third of the month

Intertemporal choice scenarios [Libertarian Paternalist treatment group]

NOTE: 1 euro is the equivalent of about 1.08 USD or 0.73 Pound Sterling as of this writing.

In one month, you are going to receive 50 euro. If you would rather receive 30 euro in one week, please tick this box.

[]

In one month, you are going to receive an Amazon voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one week, please tick this box.

[]

In one month, you are going to receive an Apple voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one week, please tick this box.

[]

In six months, you are going to receive 50 euro. If you would rather receive 30 euro in one month, please tick this box.

[]

In six months, you are going to receive an Amazon voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one month, please tick this box.

[]

In six months, you are going to receive an Apple voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in one month, please tick this box.

[]

In 12 months, you are going to receive 50 euro. If you would rather receive 30 euro in six months, please tick this box.

[]

In 12 months, you are going to receive an Amazon voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in six months, please tick this box.

[]

In 12 months, you are going to receive an Apple voucher worth 50 euro. If you would rather prefer to receive a voucher worth 30 euro in six months, please tick this box.

[]

Intertemporal choice scenarios [Autonomy-enhancing paternalist group]

Below, you will be presented with a number of scenarios – you will be asked to choose between a smaller-sooner option, and a larger-later option. Before you make your choices, here are a few arguments that I would like you to take into account:

1) Choosing the "later" option means you have something to look forward to.

2) Saving money means you'll be safe in the event of a "rainy day"

3) Every decision that we make is influenced by the choices we've made in the past. By choosing the later option now, it'll be easier to do the same in the future – you can establish a positive precedent for yourself.

4) The annual interest rate in the first three scenarios (see below) is 742961 % (based on a four-week month) in the second and last third of the scenarios it is 241 % and 178 % respectively.

However, you should also keep in mind that:

1) If you choose to receive the money or voucher sooner, you'll also be able to enjoy it sooner and have the freedom to choose whether you use them now or later. The "later" option prevents you from using the money/voucher sooner, but if you choose the sooner option you can always choose to use it later. Basically, the "sooner" option gives you more freedom.

2) Choosing the "later" option means taking a risk, as you could end up needing the money (or voucher) sooner than you thought, and choosing the "later" options means you won't have it.

NOTE: 1 euro is the equivalent of about 1.08 USD or 0.73 Pound sterling as of this writing.

You are given a choice between receiving 50 euro in one month, or 30 euro in one week. Which option do you choose?

50 euro in 1 month

30 euro in 1 week

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in one month, or a voucher worth 30 euro that you can use in one week. Which option do you choose?

A 50 euro voucher in 1 month

A 30 euro voucher in 1 week

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in one month, or a voucher worth 30 euro that you can use in one week. Which option do you choose?

A 50 euro voucher in 1 month

A 30 euro voucher in 1 week

You are given a choice between receiving 50 euro in six months, or 30 euro in one month. Which option do you choose?

50 euro in 6 months

30 euro in 1 month

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in six months, or a voucher worth 30 euro that you can use in one month. Which option do you choose?

A 50 euro voucher in 6 months

A 30 euro voucher in 1 month

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in six months, or a voucher worth 30 euro that you can use in one month. Which option do you choose?

A 50 euro voucher in 6 months

A 30 euro voucher in 1 month

You are given a choice between receiving 50 euro in twelve months, or 30 euro in six months. Which option do you choose?

50 euro in 12 months

30 euro in 6 months

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in twelve months, or a voucher worth 30 euro that you can use in six months. Which option do you choose?

A 50 euro voucher in 12 months

A 30 euro voucher in 6 months

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in twelve months, or a voucher worth 30 euro that you can use in six months. Which option do you choose?

A 50 euro voucher in 12 months

A 30 euro voucher in 6 months

Which argument in favour of the “later” option did you find to be the most convincing?

The “something to look forward to”-argument

The “rainy day”-argument

The “positive precedent”-argument

The effective interest rate-argument

No difference

Which argument in favour of the “sooner” option did you find to be the most convincing?

“Freedom” argument

“Risk” argument

No difference

Intertemporal choice scenarios [control group]

NOTE: 1 euro is the equivalent of about 1.08 USD or 0.73 Pound sterling as of this writing.

You are given a choice between receiving 50 euro in one month, or 30 euro in one week. Which option do you choose?

50 euro in 1 month

30 euro in 1 week

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in one month, or a voucher worth 30 euro that you can use in one week. Which option do you choose?

A 50 euro voucher in 1 month

A 30 euro voucher in 1 week

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in one month, or a voucher worth 30 euro that you can use in one week. Which option do you choose?

A 50 euro voucher in 1 month

A 30 euro voucher in 1 week

You are given a choice between receiving 50 euro in six months, or 30 euro in one month. Which option do you choose?

50 euro in 6 months

30 euro in 1 month

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in six months, or a voucher worth 30 euro that you can use in one month. Which option do you choose?

A 50 euro voucher in 6 months

A 30 euro voucher in 1 month

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in six months, or a voucher worth 30 euro that you can use in one month. Which option do you choose?

A 50 euro voucher in 6 months

A 30 euro voucher in 1 month

You are given a choice between receiving 50 euro in twelve months, or 30 euro in six months. Which option do you choose?

50 euro in 12 months

30 euro in 6 months

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in twelve months, or a voucher worth 30 euro that you can use in six months. Which option do you choose?

A 50 euro voucher in 12 months

A 30 euro voucher in 6 months

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in twelve months, or a voucher worth 30 euro that you can use in six months. Which option do you choose?

A 50 euro voucher in 12 months

A 30 euro voucher in 6 months

Demographic questions

Please state your age

18-23

24-35

35-64

65+

I'd rather not say

What country do you live in? If you'd rather not say, just write N/A in the box

[Comment field]

What is your relationship status?

Single/not living with partner

Married/civil union/living with partner

I'd rather not say

Is water wet?

Yes

No

Thinking about your personal finances, do you think you should save more than you currently do?

Yes

No

Don’t know

I’d rather not say

If Yes, why don’t you?

Not enough money

Lack of motivation

Forgetfulness

I don’t want to save more

I’d rather not say

What gender do you identify as?

Male

Female

I’d rather not say

What is the highest level of education you have achieved?

High-school/post-primary school or less

Undergraduate degree

Postgraduate degree

I’d rather not say

Do you currently save?

Yes, through a pension plan

Yes, privately/both privately and through a pension plan

No

I’d rather not say

What is two plus three?

3

5

1000

69

Do you think people in general should try to save more, less or about the same as now?

More

Less

Same

No opinion

Is it currently morning or evening where you live?

Morning

Evening

How do you feel about Apple on a scale from 1 (dislike) to 5 (strongly like)

[Pull-down menu with numbers 1-5]

How do you feel about Amazon on a scale from 1 (dislike) to 5 (strongly like)

[Pull-down menu with numbers 1-5]

Please provide your email address in the field below (this is voluntary but encouraged)

[Comment field]

Do you have any comments, questions or feedback in general? If you would like to take part of the findings of this study, please indicate this here

[Comment field]

Follow-up survey
Welcome!

One week ago, you participated in a survey I did on inter-temporal choice. In case you forgot, my name is John Gustavsson, and I'm a research student at the National University of Ireland, Maynooth at the Department of Economics, Finance & Accounting, and this research will form part of my thesis. The survey you are about to take is similar (but shorter as there are no demographic questions). Once again, your results are anonymous, and your data will be retained for research purposes. You may quit the survey at any time; if you quit before finishing the survey, your data will be deleted. You can also withdraw your data at any time by emailing me at the email address provided below.

It must be recognized that, in some circumstances, confidentiality of research data and records may be overridden by courts in the event of litigation or in the course of investigation by lawful authority. In such circumstances the University will take all reasonable steps within law to ensure that confidentiality is maintained to the greatest possible extent.

If you have any questions or you wish to contact me for any reason, you can reach me at john.gustavsson.2010@mumail.ie. This survey will take approximately 10 minutes to complete, depending on how much time you spend thinking about each decision.

By proceeding, you agree to take part in this survey, and have your data stored under the conditions outlined above. Thank you for your participation!

Once again, could you please tell me if your birthday is…

In the first third of the month

In the second third of the month

In the final third of the month

You are given a choice between receiving 50 euro in one month, or 30 euro in one week. Which option do you choose?

50 euro in 1 month

30 euro in 1 week

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in one month, or a voucher worth 30 euro that you can use in one week. Which option do you choose?

A 50 euro voucher in 1 month

A 30 euro voucher in 1 week

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in one month, or a voucher worth 30 euro that you can use in one week. Which option do you choose?

A 50 euro voucher in 1 month

A 30 euro voucher in 1 week

You are given a choice between receiving 50 euro in six months, or 30 euro in one month. Which option do you choose?

50 euro in 6 months

30 euro in 1 month

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in six months, or a voucher worth 30 euro that you can use in one month.

Which option do you choose?

A 50 euro voucher in 6 months

A 30 euro voucher in 1 month

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in six months, or a voucher worth 30 euro that you can use in one month. Which option do you choose?

A 50 euro voucher in 6 months

A 30 euro voucher in 1 month

You are given a choice between receiving 50 euro in twelve months, or 30 euro in six months. Which option do you choose?

50 euro in 12 months

30 euro in 6 months

You are given a choice between receiving an Amazon voucher worth 50 euro that you can use in twelve months, or a voucher worth 30 euro that you can use in six months. Which option do you choose?

A 50 euro voucher in 12 months

A 30 euro voucher in 6 months

You are given a choice between receiving an Apple voucher worth 50 euro that you can use in twelve months, or a voucher worth 30 euro that you can use in six months. Which option do you choose?

A 50 euro voucher in 12 months

A 30 euro voucher in 6 months

Appendix B: Additional statistical analysis

TABLE 10: Variance inflation factors (VIF), money tasks

Variable		**VIF**
nopostgrad		2.76
male		2.01
aep		1.68
lp		1.67
ageover23		1.61
Mean VIF		1.95

TABLE 11: Variance inflation factors (VIF), Amazon voucher tasks

Variable		**VIF**
nopostgrad		2.79
male		2.02
aep		1.70
lp		1.67
ageover23		1.60
Mean VIF		1.96

TABLE 12: Variance inflation factors (VIF), Apple voucher tasks

Variable		**VIF**
nopostgrad		2.54
male		1.88
aep		1.62
lp		1.62
ageover23		1.61
Mean VIF		1.85

TABLE 13: €30 (or voucher equivalent) in one week vs €50 (or voucher equivalent) in one month (control variables included)[16]

VARIABLES	(1) onemmoney	(2) onemamazon	(3) onemapple
lp	2.163**	1.361	3.225***
	(0.0133)	(0.447)	(0.00935)
aep	0.961	1.176	1.232
	(0.883)	(0.683)	(0.553)
ageover23	2.174***	1.751	1.514
	(0.00276)	(0.126)	(0.212)
male	1.393	1.256	1.810*
	(0.166)	(0.497)	(0.0635)
nopostgrad	1.377	1.594	0.767
	(0.363)	(0.328)	(0.611)
Constant	1.562	4.337**	4.269**
	(0.294)	(0.0117)	(0.0152)
Observations	481	473	411
Pseudo R2	0.0393	0.0125	0.0473

TABLE 14: €30 (or voucher equivalent) in one month vs €50 (or voucher equivalent) in six months (control variables included)

VARIABLES	(1) sixmmoney	(2) sixmamazon	(3) sixmapple
lp	1.953***	1.559*	1.764**
	(0.00387)	(0.0659)	(0.0401)
aep	1.084	1.207	0.958
	(0.723)	(0.428)	(0.870)
ageover23	2.426***	2.033***	2.703***
	(1.08e-05)	(0.000780)	(3.55e-05)
male	1.198	1.088	1.436
	(0.350)	(0.676)	(0.107)
nopostgrad	0.848	0.689	0.813
	(0.558)	(0.246)	(0.561)
Constant	0.605	1.494	1.243
	(0.157)	(0.301)	(0.612)
Observations	481	473	411
Pseudo R2	0.0503	0.0337	0.0576

[16] Notes on Tables 13-18: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column.

TABLE 15: €30 (or voucher equivalent) in six months vs €50 (or voucher equivalent) in twelve months (control variables included)

VARIABLES	(1) twelvemmoney	(2) twelvemamazon	(3) twelvemapple
lp	1.321	1.413	1.559
	(0.330)	(0.365)	(0.438)
aep	0.844	0.849	0.877
	(0.202)	(0.208)	(0.234)
ageover23	1.959***	1.810***	2.072***
	(0.427)	(0.407)	(0.509)
male	1.581**	1.285	2.306***
	(0.323)	(0.272)	(0.529)
nopostgrad	1.287	1.348	1.735*
	(0.385)	(0.418)	(0.570)
Constant	1.004	1.251	0.674
	(0.372)	(0.483)	(0.276)
Observations	481	473	411
Pseudo R2	0.0277	0.0201	0.0507

TABLE 16: €30 (or voucher equivalent) in one week vs €50 (or voucher equivalent) in one month (USA-only sample)

VARIABLES	(1) onemmoney	(2) onemamazon	(3) onemapple
lp	1.765	1.122	2.281
	(0.824)	(0.589)	(1.279)
aep	0.798	1.286	1.232
	(0.299)	(0.647)	(0.568)
Constant	3.824***	7.000***	4.385***
	(1.042)	(2.366)	(1.348)
Observations	225	222	189
Pseudo R2	0.0144	0.00161	0.0151

TABLE 17: €30 (or voucher equivalent) in one month vs €50 (or voucher equivalent) in six months (USA-only sample)

VARIABLES	(1) sixmmoney	(2) sixmamazon	(3) sixmapple
lp	1.780*	1.588	1.385
	(0.611)	(0.582)	(0.569)
aep	1.188	1.178	0.764
	(0.373)	(0.389)	(0.282)
Constant	0.952	1.667**	2.333***
	(0.210)	(0.385)	(0.609)
Observations	225	222	189
Pseudo R2	0.00943	0.00576	0.00928

TABLE 18: €30 (or voucher equivalent) in six months vs €50 (or voucher equivalent) in twelve months (USA-only sample)

VARIABLES	(1) twelvemmoney	(2) twelvemamazon	(3) twelvemapple
lp	2.033*	1.834	2.034
	(0.834)	(0.818)	(0.956)
aep	0.672	0.548*	0.540
	(0.222)	(0.192)	(0.202)
Constant	2.280***	3.211***	2.889***
	(0.547)	(0.843)	(0.790)
Observations	225	222	189
Pseudo R2	0.0298	0.0359	0.0424

TABLE 19: Sensitivity and specificity, no control variables[17]

	Sensitivity (%)	Specificity (%)	Area under ROC curve
onemmoney	100	0	0.5720
onemamazon	100	0	0.5236
onemapple	100	0	0.5971
sixmmoney	66.54	41.70	0.5654
sixmamazon	100	0	0.5449
sixmapple	100	0	0.5533
twelvemmoney	100	0	0.5320
twelvemamazon	100	0	0.5483
twelvemapple	100	0	0.5516

TABLE 20: Sensitivity and specificity, control variables included

	Sensitivity (%)	Specificity (%)	Area under ROC curve
onemmoney	100	0	0.6474
onemamazon	100	0	0.5843
onemapple	100	0	0.6631
sixmmoney	77.34	46.67	0.6495
sixmamazon	100	0	0.6282
sixmapple	96.50	10.40	0.6677
twelvemmoney	99.70	0.68	0.6114
twelvemamazon	100	0	0.5990
twelvemapple	99.32	1.71	0.6501

[17] Notes on Tables 19-20: The cutoff score when estimating sensitivity and specificity is 0.5. The first column shows a list of dependent variables, with the columns next to it displaying the sensitivity, specificity and area under ROC curve for the regressions on these variables.

TABLE 21: Correlation between email and intertemporal choice variables[18]

	Email
onemmoney	0.4191
onemamazon	0.9738
onemapple	0.1685
sixmmoney	0.6332
sixmamazon	0.7239
sixmapple	0.5241
twelvemmoney	0.1514
twelvemamazon	0.9722
twelvemapple	0.7005

[18] Notes on Table 21: The table shows the p-values for the correlation coefficients between the email variable, and the intertemporal choice variables listed in the first column.

Appendix C: Maynooth University Research Ethics Committee letter of approval

MAYNOOTH UNIVERSITY RESEARCH ETHICS COMMITTEE
MAYNOOTH UNIVERSITY,
MAYNOOTH, CO. KILDARE, IRELAND

Maynooth University
National University of Ireland Maynooth

Dr Carol Barrett
Secretary to Maynooth University Research Ethics Committee

27 April 2015

John Gustavsson
Department of Economics, Finance and Accounting
Maynooth University

Re: Application for ethical approval for a Project entitled: An experimental investigation of paternalist interventions in the context of time discounting

Dear John,

The above project has been evaluated under Tier 2 process, expedited review and we would like to inform you that ethical approval has been granted.

Any deviations from the project details submitted to the ethics committee will require further evaluation. This ethical approval will expire on 30 April 2016.

Kind Regards,

Dr Carol Barrett
Secretary,
Maynooth University Research Ethics Committee

C.c. Prof Rowena Pecchenino, Department of Economics and Finance and Accounting

Reference Number SRESC-2015-025

The Marginal Cost of Transparency: Do honest nudges work?

Abstract

Libertarian paternalism, a term which refers to the practice of "nudging" consumers into making "good" decisions, has grown steadily in popularity in recent years as an alternative to sin taxes and other traditional forms of paternalism. Critics however believe that relying on psychological manipulation is inherently unethical as consumers are typically unaware of the nudge and the intention behind it. While proponents of LP insist that they want LP interventions to be conducted in an ethical manner, there is so far little evidence that LP interventions, when conducted in such a manner, still have the desired effect. In this paper I introduce the term Marginal Cost of Transparency (MCoT), the difference in treatment effect of an LP and what I call a Transparent Libertarian Paternalism (TLP) intervention, a type of LP intervention where consumers are made aware of the nudge and why it is there. In this paper I conduct an experiment the results of which indicate that the MCoT is for the most part not statistically significantly different from zero and that the answer to the question "Do honest nudges work?" is Yes. Moreover, the results indicate that Autonomy-enhancing paternalism (AEP), a type of paternalist interventions that work to enhance the autonomy of consumers (mainly by providing information) and unlike LP do not rely on psychological manipulation, fares at least as well as the LP/TLP treatments provided that participants are paying full attention.

1. Introduction

Since the term was coined (Thaler and Sunstein, 2003), libertarian paternalism has been a topic of debate among behavioral economists. The term refers to measures that intend to change consumers' behavior for their own good, paternalism, without using coercive means, that is, using a "libertarian" approach. It can be thought of as an umbrella term, incorporating various types of so called "nudges": Changing the order of items on a menu, changing the default option on a corporate pension plan from opt-in to opt-out and informing people in a neighborhood of their neighbors' consumption patterns such as how much energy the average person in their neighborhood uses are just a few examples.

Although libertarian paternalism has been embraced by policymakers in several countries as an easy way to "fix" consumer behaviors perceived as flawed, libertarian paternalism has been met with far from universal acclaim in the academic community with critics questioning everything from the suitability of the term itself to the efficacy and ethics of the methods used.

One criticism leveled by Binder and Lades (2015), among others, is that most forms of libertarian paternalism use psychological manipulation and the exploitation of biases to achieve the goal of the "choice architect" the policymaker designing the nudge and usually without the consumer being aware of the nudge or why it is there. Thus, while workers being enrolled in an opt-out retirement saving plan will be informed that they are being enrolled and be provided with information regarding the plan, the same cannot be said for consumers visiting a restaurant whose menu has been designed to induce them to choose the salad over the burger.

Binder and Lades (2015) proposed an alternative they named Autonomy-Enhancing Paternalism (AEP). AEP is technically a subset of LP but with stricter criteria. In order for an intervention to qualify under the AEP umbrella, the intervention cannot rely on the exploitation of psychological biases; instead it must enhance the individual's autonomy, the ability to make a conscious decision, by providing more information, such as public service announcements, nutrition labels on menus etc., or by preventing an individual from making a hasty decision by, for example, introducing a mandatory waiting period between the purchase and delivery of a good/service, such as a payday loan, during which the individual can cancel the purchase. Common forms of libertarian paternalism such as changing the default option to the option the choice architect wants the consumer to choose, is off limits under AEP, as is the use of framing as in the menu example.

AEP relies implicitly on the assumption of classic liberalism that consumers will do what is best for them if given all the necessary information and enough time to make a decision. In contrast, LP is based on the more pessimistic view common among behavioral economists where consumers are seen as helpless victims that cannot be reliably counted on to make good decisions for themselves even if provided with full information. The AEP approach has been discussed in the context of improving the health and well-being of adolescents (Patton et al., 2016) and in promoting sustainable practices (Babutsidze and Chai, 2018). Wagner (2019) advocates for AEP as one measure to reduce opportunism on behalf of principals.

While AEP has a clear ethical advantage over LP, it is not without disadvantages. It is conceivable that providing nutrition information on restaurant menus could cause a loss of utility for all consumers who are buying high or even moderate calorie meals *even* if their action is rational, by inducing guilt and/or shame. While these consumers may be aware that they are eating an unhealthy meal, having the nutrition information "pushed down their throats" may put a damper on the mood even if the meal is, for example, part of the celebration of a special occasion. Such an AEP intervention could also serve to worsen the conditions of those who suffer from eating disorders such as anorexia who are prone to obsess about the calories in the foods they consume. It is an open question what information consumers need to make good choices and who is capable of deciding that and why. Moreover, different sets of consumers need different information, i.e. anorexics do not need calorie information. There is no mechanism to assure all groups are accommodated.

In their book *Nudge* (2008) Thaler and Sunstein assure the readers that they want nudging to be carried out in an ethical, transparent manner, although they do not describe in any further detail what they mean by this. Curiously, however, the vast majority of nudging case studies they present to bolster their case lack transparency, and there is no way to know whether the nudges would work as well or at all had the choice architects been transparent about their work and intentions. If it is in fact the case that transparency does not harm the efficacy of a nudge, then this provides a potential "third option", a compromise of sorts between the LP and AEP approach: Honest nudging, or Transparent Libertarian Paternalism (TLP). A TLP intervention would be identical to an LP intervention, with the exception that consumers are explicitly informed, for example through a written disclaimer, that they are being nudged and why. This approach solves one of the major ethical issues with LP which is the lack of transparency and by extension lack of accountability of the choice architects.

Transparency in the context of nudging has been discussed extensively, though few empirical studies exist on the topic. Bovens (2009) argue that nudges “work best in the dark”, a view that was accepted by the House of Lords in a report released in 2011.

Some studies, however, have challenged this assumption: Kroese, Marchiori and de Ridder (2016) conducted a field experiment in which two groups of participants were nudged towards making healthier food choices. The nudge consisted of a redesign of two stores to make the healthy choices more prominent. One store however informed customers about this through a sign near the cash register display, whereas the other did not. The transparent nudge did not perform any worse than the hidden nudge, suggesting that disclosing the existence of a nudge does not change how consumers react to it. However, it should be noted that the sign that informed customers about the nudge merely stated that “we help you make healthier choices”. Thus, customers were not informed explicitly that they had been nudged, that the store had employed a form of psychological manipulation of their choices, only that the store had done something to make them choose healthier options. Clearly, such a sign could just as easily have referred to the store slashing prices on fruits and vegetables or adding more healthy options *without* hiding the unhealthy ones.

While transparency is a common theme throughout literature, and while almost everyone will speak highly of transparency as a concept, the meaning of the term transparency is not agreed upon. Does transparency only require that consumers or employees in the case of a workplace nudge are able to find out about the nudge? Or does it require that they are told that they are being nudged? If so, how clear must communication be – would consumers have to be told in such a manner that it can be assumed that most of them actively considered the information in their decision making? Does transparency mean that consumers must explicitly be told that they are being nudged, rather than just being told that changes have been made to make them act in a certain way, i.e. “we help you make healthier choices” with no further information provided? Do you have to inform consumers what the nudge is, or merely that there is one? Do you have to be honest about who is behind the nudge or would it be acceptable to phrase a disclaimer using neutral terms? Stating that “Fruit and vegetables have now been more prominently displayed to promote healthy lifestyles” may make consumers believe that the nudge has been implemented by order of the government whereas “In this store we have chosen to display fruits and vegetables more prominently to promote healthy lifestyles” makes it clear who is behind the nudge and thus who consumers ought to hold accountable for it.

In my view, in order for a nudge to be considered transparent, the disclosure should be made in such a way that most consumers will read it and it should be clear what is being done (what the nudge consists of), why, and who is behind it. I argue that the purpose of transparency should not merely be to avoid deception in a technical, legalistic sense, but to avoid consumers *feeling* that they have been deceived. Informing consumers about a nudge in a manner that most of them will miss, for example, by putting a disclaimer in a footnote on a website, will likely not stop consumers from feeling that they have been deceived if/when they find out about the nudge at a later point. The fact that they could have found out on their own will not lessen the feeling of deception. If anything, consumers may see the hiding of the disclosure as proof that the company or government knew that what they were doing was wrong or would be unpopular. Likewise, only providing partial information, such as stating that you want to help people eat healthily but not how, or that you have redesigned the store but not why, can reasonably be considered lying by omission; that is, another form of deception.

Furthermore, as transparency is a necessity for accountability, transparency must entail informing consumers who is responsible for the nudge. Should they complain to the shop owner or their local elected representative if they dislike the nudge?

In their report from 2011, the House of Lords made the argument that since private businesses do not have to disclose the exact methods they use to make (nudge) consumers purchase more, neither should government have to do so when nudging individuals towards its own goals. This, however, ignores that the government is far from just another actor on the market. Rather it assumes that the government does not have a greater ethical responsibility towards the citizens than a private business does and ignores the higher expectations citizens place on their government. It also ignores the higher risk associated with governmental nudging. If consumers feel like they have been deceived or lied to by their government this arguably has a greater negative effect on political stability and society as a whole than if consumers feel that they have been deceived or lied to by a private business. This is especially troubling today as public trust in government and its institutions has fallen dramatically throughout the western world since the financial crisis of 2008. Given these circumstances it seems wise to err on the side of caution with regards to transparency so as not to further erode the public's confidence.

Bruns et al. (2018) conducted an experiment that concluded that even full transparency, fulfilling the criteria listed above, does not reduce the efficiency of a default-option nudge intended to increase the amount participants spent on climate compensation. While transparency did reduce the amounts contributed, the difference was not statistically significant. However, their study suffers from a student-only sample and lack of questions to gauge engagement with the experiment. Moreover, students and young people are more interested in environmental issues than the general population, which could affect the outcome. Perhaps such an environmentally concerned group does not mind being nudged towards an environmentally friendly option, but there is no way to tell whether this is the case in the general population. The combination of real incentives with the lack of engagement identifying questions is also troubling. By offering €10 to all participants out of which they could keep whatever they did not donate towards climate compensation without having any way of weeding out unserious or inattentive participants, they ignored the significant risk that students signed up to make some money rather than to seriously engage with the experiment.

In this paper I introduce the term the "*Marginal Cost of Transparency*" (MCoT), the difference between the treatment effect of a standard libertarian paternalist treatment and a transparent libertarian paternalist treatment. I conduct an experiment with the ostensible goal of reducing the time discount rate, and I measure the MCoT by assigning participants to four different groups: An LP group, a TLP group, an AEP group and a control group. Conducting an experiment with random assignment allows for the effects of the LP and TLP treatments to be compared directly. The reason for including an AEP treatment in the experiment is that AEP is another potentially viable ethically superior alternative to LP.

In the LP treatment group the default option was set to the larger-later option, meaning participants had to make an active choice by checking a box in order to receive the smaller-sooner option. The TLP treatment was identical except for a disclaimer in capital letters informing participants of the nudge, what it is meant to do and why. That I am the person behind the nudge should be clear to all participants. In the AEP treatment there was no default option but participants were instead provided with a list of arguments in favor of choosing the larger-later option. The arguments and the disclaimer message can both be found in Appendix A. In the control group there was neither a default option nor were participants provided with any arguments in favor of either option.

This paper is organized as follows: In Section 2 I outline my methodology, how the experiment was conducted and the advantages and limitations of my approach. In Section 3 I present the results of the experiment and discuss what these results mean. Finally, Section 4 contains my conclusions from this study.

Hypotheses

This study tests two main hypotheses:

1. TLP has no effect
2. The difference between the LP and AEP treatment effects will be statistically insignificant.

2. Methodology

In order to test the hypotheses regarding the relative efficacy of first and foremost TLP and LP and secondly LP and AEP I conducted an online experiment between the 18th of October and the 17th of November 2016. In total 1552 participants completed the experiment. Participants were mainly recruited through social media including Facebook, Reddit and Twitter, and an email invitation sent out to all students at the Department of Finance, Economics & Accounting at Maynooth University.

The incentive structure of this experiment was similar to the one developed by Coller and Williams (1999). Three participants were randomly selected to be paid based on their stated preference for one pre-selected task (task #4). These participants were neither aware at the time they took part in the experiment that they would be paid nor were they nor any other participants aware of which task was the "real" task. All participants were informed of the incentive structure before agreeing to take part in the experiment, but they were not informed of the hypotheses as that may have biased the results. The three selected participants were contacted via email and paid through PayPal.

There is little evidence indicating that incentives matter in the context of intertemporal choice experiments as documented by an extensive review of the intemporal choice literature by Frederick, Loewenstein and O'Donoghue (2002). Coller and Williams (1999) found no difference between participants who were offered real incentives and those were not, while Abdellaoui, Bleichrodt and l'Haridon (2013) found only small differences. Bickel et al. (2009) found, through a neuroimaging study, that responses to intertemporal choice tasks were the same regardless of whether incentives were offered or not. Never the less, as a matter of caution this experiment used real incentives, though due to budget constraints, as discussed, only a few participants could be paid. It should be noted that even if participants were to display different discount rates depending on the type of incentives offered, this would not be of great concern seeing as how the purpose of the study was not to determine individual discount rates per se, but rather to determine the impact of various interventions. That is, as long as the type of incentive offered did not affect one treatment group differently than another, comparisons can still be made between groups to determine whether or not one treatment performed better than another, even if the discount rates recorded are higher or lower than they would be in real life. There is no intuitive reason to believe that any group would be differently affected by the real, low incentives offered in this study.

Participants were randomly assigned into one of four groups: The LP treatment group, the TLP treatment group, the AEP treatment group, and the control group. As the platform did not allow for true randomization, the first question asked participants during what part of the month (first week, second week etc.) they were born, and based on their answers they were assigned to different groups.

In the second part of the experiment, all participants were told that they had won the lottery and were asked to choose between a prize of €20/€50/€250[19] in 1 week/1 month/6 months and €40/€100/€500 in 1 month/6 months/12 months, which made for a total of nine intertemporal choice tasks. Different sized rewards were used as it has been shown (Thaler, 1981) that the discount rate tends to fall as the size of the reward goes up, hence it seems within reason to suspect that demographic

[19] Participants were provided with exchange rates for USD and SEK.

and treatment variables may have different impact on different sized rewards, i.e. some may only affect the lowest rewards, some only the highest.

This experiment used choice tasks that were inspired by those used by Green, Myerson and McFadden (1997) and Hesketh (2000). Choice tasks are tasks where participants are asked to choose between a smaller-sooner reward and a larger-later reward. Such tasks provide less precision in measuring discount rates, yet they are preferable since they are the closest equivalent to the type of intertemporal choices faced by most consumers on a daily basis which consist of a choice between one fixed amount now and another fixed amount at a specific latter point. One option would have been to use "matching" tasks where participants are asked to match how much money they would need at a certain point in the future for it to be equivalent to a specific amount of money today. However, such matching tasks, while they do provide precise measurements of discount rates, are very rare outside of experiments; there are very few if any real life situations where consumers are asked to "match" a certain amount in the future with another amount today. Because of this, as discussed by Frederick, Loewenstein and O'Donoghue (2002), consumers tend to rely on heuristics when solving matching tasks that they would not rely on outside of the experiment. Finally, matching tasks are relatively time consuming and may reduce the number of participants who actually complete the experiment and/or stay focused throughout its duration.

In addition to matching tasks rating tasks were also considered, but ultimately deemed inferior to choice tasks as they, just like matching tasks, do not resemble any real life situation and additionally they may be sensitive to extremeness aversion (Tversky and Simonson, 1993). The number of tasks was kept at the relatively low number of nine for two reasons. First, because survey completion rates have been shown to have a negative relationship with the number of questions (Galesic and Bosnjak, 2009) and second, because the generalizability of the experiment would be reduced by too many tasks, as there are very few real life scenarios where a consumer would face dozens of intertemporal choices at once and little is known regarding whether consumers act differently when faced with a large number of choices compared to a small number.

Participants were randomly assigned into one of four groups, based on their answer to the first question which asked which during which part of the month they had been born. This was necessary as the platform did not allow for true randomization. The four groups were the libertarian paternalist treatment group, the transparent libertarian paternalist treatment group, the autonomy-enhancing paternalist treatment group and the control group.

In the libertarian paternalist treatment group, the default option was set to the larger-later option and participants had to make an active choice by checking a box if they wanted the smaller-sooner option suggesting that the treatment relied on the status quo bias.

In the transparent libertarian paternalist treatment group, the default option was, just like in the LP treatment group, set to the larger-later option. However, participants in this treatment group were explicitly told about the default option and the purpose behind it before the choice tasks there was a message written in all-caps conveying this information. As such, while the treatment still had a nudge, it had a greater degree of transparency and did not seek to unknowingly manipulate participants in the way the LP treatment did.

In the AEP treatment group, nudges were foregone entirely in favor of providing participants with a list of reasons why they should choose the larger-later option. The list, together with the rest of the survey, can be found in Appendix A. Participants were also asked which argument they found the most convincing, and those who stated that they had not read the list were not included in the regressions as they could not be considered part of the AEP group – one option would have been to include them in the control group, but this was rejected as there is no way to know whether they may have read and been influenced by a few of the reasons or whether they did not read any at all.

Participants in the control group were neither provided with a list of arguments nor exposed to a default option.

The third and final part of the survey was identical to all participants and consisted of demographic questions covering age, marital status, gender, education, in which part of the world the participant resided and whether the participant was currently enrolled at university. Participants were not required to provide any identifying information. This section also asked questions regarding saving and the participant's attitude towards it. See Appendix A for complete list. Notably, this survey did not ask for the annual income of participants, even though it is conceivable that it may affect the discount rate. This is for a number of reasons. First, a large number of participants – likely mainly those with low incomes – would be reluctant to provide that information. Second, what is considered a high income in one location may not be a high income in another location; a person making a high salary in Mexico may still make less than the average American. This issue exists even within countries. A salary high enough to afford a very comfortable lifestyle in rural US may not be nearly high enough to afford even a decent lifestyle in

Manhattan or San Francisco. Third, income is far from a perfect predictor of lifestyle. Students, for example, generally have low incomes but also do not have the same expenses that adults out of college tend to have – students tend to save money by living in dorms or at home, most of them do not need a car as they live close to college, they receive student discounts in many shops, etc., and may therefore appear poorer than they really are. The same can be said of retirees.

The final part of the survey also contained two questions designed to find out whether the participants had paid attention while reading the instructions of the survey. This was inspired by Haan and Linde (2011). These questions were "How many participants who take this survey will be paid?" and "How many intertemporal choice scenarios (questions where you were asked to choose between a smaller-sooner and larger-later reward) were there on the previous page?" The answer to the first question was provided in the introduction to the survey, and to answer the second question the participant only needed to remember how many tasks he or she had just completed on the previous page.

The second-to-last question asked participants for their email address so that they could be contacted and paid if they were one of the three selected participants. The last question was a comment field where participants could leave feedback and request to receive the findings from the experiment. The "feedback form" was included for two reasons. First, because this feedback may be used to improve the design of future experiments. Second, because the feedback of some participants may indicate that they did not understand the experiment and their role in it, and in that case their data could be removed from the experiment before statistical analysis took place.

As mentioned above the rewards in this experiment varied from €20, €50 and €250 for the smaller-sooner option to €40, €100 and €500 for the larger-later option. The smaller-sooner reward was always half of the larger-later reward. Given the magnitude effect (Thaler, 1981) we cannot expect smaller rewards to be discounted at the same rate as larger rewards, and it is conceivable to think that a treatment that works on a smaller (larger) reward may not work on a larger (smaller) reward, which is why this experiment used rewards of different sizes.

Conducting this experiment online allowed for a larger and more diverse sample than traditional experiments conducted on college campuses. This experiment has 1552 participants. The experiment was far more diverse than most experiments, with hundreds of participants from all age group groups, both genders, married as well as non-married, etc.

The internet also provides a greater degree of anonymity than traditional lab experiments, potentially reducing the observer effect that otherwise may lead to participants acting unnaturally, which would reduce the generalizability of the results. Finally, participation in an online experiment requires less time and effort on behalf of the participant, meaning even those who would not find it worthwhile to participate in a lab experiment may take the time to participate in this experiment, which reduces the self-selection problem associated with economic experiments.

However, as identified by Wright (2005), an online experiment is also associated with drawbacks not present in a lab experiment. The same participant could potentially take the experiment multiple times, although this risk was mitigated by making it impossible to take the experiment multiple times from the same computer; requiring any “cheaters” to use separate computers or internet-connecting devices. Given the relatively low incentives in this experiment, it is highly unlikely that more than at most a few participants found this worthwhile.

Participants may also be suspicious of the financial incentive and may suspect that the experiment is a scam. However, this risk was mitigated by reassuring participants in the introduction to the experiment that they would not have to provide any banking details to receive payment.

There is also the issue of distractions; while in a lab experiment participants tend to be in a quiet room with nothing else to do than completing their tasks, this is not the case with an online experiment where participants may be distracted by other web content such as popup notifications, and where the experimenter is unable to observe the participants to make sure that they are not, for example, asking for help or getting input from their friends or from the internet. While this lack of control does reduce internal validity (Grimelikhuijsen and Meijer, 2014), on the whole it does not necessarily have to be a negative feature as economic decisions are usually taken in "noisy" environments such as shopping centers, as such the "environment" provided by this experiment may be closer to the kind of environment where real life intertemporal choices are made. This experiment also allows participants to take part from the comfort of their own homes, possibly making them more relaxed and prone to act naturally.

Recruiting a representative sample can be a struggle with online experiments which generally suffer from lack of unbiasedness for this reason (Duda and Nobile, 2010). In particular this may be a problem for surveys like this which are open to the entire adult population, given that the entire adult population is not online. While this does pose a problem, lab experiments generally suffer from this to an even higher degree as samples tend to be student-only.

In the specific setting of conducting an intertemporal choice experiment online has the disadvantage of not being able to pay participants immediately upon completion as could be done in a lab experiment. As such, the shortest delay in an online intertemporal choice task cannot be zero if real incentives are to be used, as in this experiment, as many participants would certainly figure out that it would be impossible to pay rewards immediately and that tasks that gave the option of receiving money immediately were hypothetical. Therefore, the shortest delay in this experiment was set to one week. While this may mean that the experiment may fail to capture some of the "present bias", this is not a grave concern as discount rates appear to be falling for at least one year from the present time (Frederick, Loewenstein and O'Donoghue, 2002) and so most of the present bias is likely to still be present even though participants cannot choose to receive the reward immediately.

Finally, this survey did not record dropout rates as the data from participants who did not finish the survey were deleted as promised in the introductory page of the survey (see Appendix A). Due to this, it is impossible to say how many or what kind of participants dropped out (i.e. was there a higher dropout rate in one particular treatment group?) or to analyze the implications that this information may have had.

3. Results

These results were obtained by estimating logistic regressions using the responses to the intertemporal choice tasks as the dependent variables. As there were nine tasks, there are also nine dependent variables, all of them binary making them suitable for logistic regression. For the sake of simplicity, the coefficients are expressed as odds ratios expressing the likelihood of a participant in the relevant group choosing the larger-later option for that particular task relative to the likelihood of a participant in the control group making the same choice.

After estimating the regression parameters, Wald tests were used to determine whether or not the LP treatment significantly outperformed the TLP or AEP treatment, and whether the TLP treatment significantly outperformed the AEP treatment, or vice versa. Note that since Wald tests p-values are calculated on a two-tailed basis, one must cut this statistic in half in order to get the appropriate one-tailed p-value which is relevant to this paper (i.e. does LP outperform TLP?). Thus, in the tables in this section a p-value less than 0.1 indicates significance.

There are two different sets of regressions below. In the **first set** (Tables 4-6) everyone who passed **at least one question discerning attentiveness** is included, giving a sample size of **1000**. In the **second set** (Tables 7-9), **only those who passed both attentiveness discerning questions** are included, giving a sample size of **323**. The reasoning for including the first set is pragmatic; it seems reasonable to assume that some of those who failed only one of these questions may still have taken the experiment seriously but have paid too little attention to remember the instructions where they could find the answer to one of the questions, and to the number of tasks that they solved which was the answer to the other question. In the real world many consumer decisions, especially those involving low amounts of money, are taken by consumers while they are not paying full attention, and estimating and comparing regressions with both samples can provide insights on the whether and how different treatments affect fully attentive and less attentive participants differently. Finally, the regressions that included control variables have a sample size of **944**; this is because those who answered "I'd rather not say" to any of the relevant demographic questions that made up the control variables (age, education, gender) were excluded.

Model specification and assumptions

Logistic regression model 1: No control variables

$$\log \mathrm{Prob}(Y=1)/(1 - \mathrm{Prob}(Y=1)) = B_0 + B_{lp}d_{lp} + B_{tlp}d_{tlp} + B_{aep}d_{aep}$$

Logistic regression model 2: Control variables included

$$\log \mathrm{Prob}(Y=1)/(1 - \mathrm{Prob}(Y=1)) = B_0 + B_{lp}d_{lp} + B_{tlp}d_{tlp} + B_{aep}d_{aep} + B_{agebelow36}d_{agebelow36} + B_{male}d_{male} + B_{nopostgrad}d_{nopostgrad}$$

where Y is a dependent variable based on the answers to a certain intertemporal choice task (see Tables 4-9).

Like with any type of regression, certain assumptions must hold true in order for a logistic regression to yield valid and reliable results.

First, given that these are binary rather than multinomial logistic regressions, the dependent variables must be binary. It is not difficult to verify that this is indeed the case.

Second, observations must be independent of one another. There is no reason to believe that the answers of one respondent would not be independent of the other respondents as the participants did not know each other and it is unlikely that any of the participants took the survey multiple times given that the survey involved rather low incentives and also seeing as how IP tracking ensured that a participant would have to use a different device or a proxy/VPN to do so.

Third, there must be little or no multicollinearity among the independent variables in logistic regressions. Among the regressions that do not include control variables this must clearly be the case as no participant can be part of two treatment groups, and by calculating the variance inflation factor (see appendix 2, Table 13) I was able to confirm that collinearity was not an issue even for those regressions that did include control variables.

Fourth, logistic regression requires that all continuous independent variables be linearly related to the log odds. Given that all independent variables are binary, this does not apply.

Finally, a logistic regression requires a relatively large sample size. Peduzzi et al. (1996) argued that as a guideline the sample size, N, should be at least equal to 10k/p, where k is the number of independent variables and p the smallest number of negative or positive cases in the population. The first set of regressions meet this criterion, while the second set, that includes control variables, does not (see Appendix 2, Tables 10-12 for regression output including control variables). The restricted sample also does not fulfill this criterion for every task which is why I decided to estimate a set of exact logistic regressions with the restricted sample. By comparing the outcomes to those of the ordinary logistic regressions I was able to confirm that they are very similar (see Appendix 2, Tables 14-16).

TABLE 1: Description of variables	
Control	1 if participant is in the control group, 0 otherwise
lp	1 if participant is in the libertarian paternalist group, 0 otherwise

aep	1 if participant is in the autonomy-enhancing paternalist group, 0 otherwise
tlp	1 if participant is in the transparent libertarian paternalist group, 0 otherwise
onemonth40	1 if participant chose €40 in one month over €20 in one week, 0 otherwise
onemonth100	1 if participant chose €100 in on month over €50 in one week, 0 otherwise
onemonth500	1 if participant chose €500 in one month over €250 in one week, 0 otherwise
sixmonths40	1 if participant chose €40 in six months over €20 in one month, 0 otherwise
sixmonths100	1 if participant chose €100 in six months over €50 in one month, 0 otherwise
sixmonths500	1 if participant chose €500 in six months over €250 in one month, 0 otherwise
twelvemonths40	1 if participant chose €40 in twelve months over €20 in six months, 0 otherwise
twelvemonths100	1 if participant chose €100 in twelve months over €50 in six months, 0 otherwise
twelvemonths500	1 if participant chose €500 in twelve months over 250 in six months, 0 otherwise
age1823	1 if participant is aged 18-23, 0 otherwise
age2435	1 if participant is aged 24-35, 0 otherwise
age3664	1 if participant is aged 36-64, 0 otherwise
ageover64	1 if participant is over the age of 64, 0 otherwise
student	1 if participant is a student, 0 otherwise
married	1 if participant is married, in a civil union, or cohabitating with partner, 0 otherwise
male	1 if participant identifies as male, 0 otherwise

highschool	1 if participant's highest achieved level of education is high school or less, 0 otherwise
undergrad	1 if participant's highest achieved level of education is an undergraduate degree, 0 otherwise
postgrad	1 if participant's highest achieved level of education is a postgraduate degree, 0 otherwise
westerneurope	1 if participant resides in western Europe, 0 otherwise
easterneurope	1 if participant resides in eastern Europe, 0 otherwise
southerneurope	1 if participant resides in southern Europe, 0 otherwise
northamerica	1 if participant resides in North America, 0 otherwise
centralamerica	1 if participant resides in Central America, 0 otherwise
ausnz	1 if participant resides in Australia, New Zealand or elsewhere in Oceania, 0 otherwise
southeastasia	1 if participant resides in south east Asia, 0 otherwise
middleeast	1 if participant resides in the Middle East, 0 otherwise.
africa	1 if participant resides in Africa, 0 otherwise.
southamerica	1 if participant resides in South America, 0 otherwise
agebelow36	1 if participant is below the age of 36, 0 otherwise
nopostgrad	1 if participant does not hold a postgraduate degree, 0 otherwise

TABLE 2: Summary of data (N=1000)

Variable:	Share (%)
lp	21.4
tlp	22.8
aep	28.6
onemonth40	94
onemonth100	94.7
onemonth500	93.3
sixmonths40	74.8
sixmonths100	79.3
sixmonths500	85.4
twelvemonths40	86.5
twelvemonths100	87.7
twelvemonths500	87.8
age1823	7.4
age2435	17
age3664	60.7
over64	14.3
student	11.7
married	39
male	83.2
highschool	29.7
undergrad	24.4
postgrad	41
westerneurope	86.4
easterneurope	1.8
southerneurope	1.1
northamerica	6.7
centralamerica	0.2
ausnz	0.7
southeastasia	0.9
middleeast	0.2
africa	0.6
southamerica	0.2
agebelow36	24.4
nopostgrad	54.1

As can be seen in Table 2, the randomization was quite successful with only a small surplus of AEP participants. Unlike in most experiments, students and those aged 18-23 made up only a small proportion of this experiment's sample, and almost a third of participants have not graduated college at all. Just under 40 per cent are married, and over half are older than 36.

This sample is relatively diverse compared to other experiments, though it should be noted that very few participants (less than 10 per cent) reside outside the western world. Caution is therefore advised before applying these results and conclusions to developing world settings. It should also be noted that men are heavily overrepresented.

TABLE 3: SUMMARY OF DATA DIVIDED BY TREATMENT GROUP

Variable	LP treatment group (N=214) Share (%)	TLP treatment group (N=228) Share (%)	AEP treatment group (N=286) Share (%)
lp	100	0	0
tlp	0	100	0
aep	0	0	100
onemonth40	95.3	91.2	95.5
onemonth100	96.7	94.3	93.7
onemonth500	91.1	92.5	95.5
sixmonths40	83.2	79.4	70.6
sixmonths100	89.3	82.5	75.2
sixmonths500	87.4	84.2	87.1
twelvemonths40	91.6	90.4	79.7
twelvemonths100	93.9	90.4	83.2
twelvemonths500	88.8	84.6	88.8
age1823	7.5	8.8	5.2
age2435	16.8	14	18.5
age3664	61.2	64.9	57.3
over64	13.1	11.8	18.5
student	11.2	11.8	8
married	33.6	41.7	39.5
male	84.1	84.2	81.8
highschool	29.4	31.1	26.2
undergrad	21.5	25	22.7
postgrad	42.5	40.8	44.8
westerneurope	89.3	85.1	84.6
easterneurope	1.4	2.2	3.1
southerneurope	1.9	0.4	0.7
northamerica	4.2	7.9	8.4
centralamerica	0	0	0
ausnz	0	1.8	0.3
southeastasia	1.4	0.9	0.7
middleeast	0.9	0	0
africa	0	0.9	0.7
southamerica	0	0	0
agebelow36	24.3	22.8	23.8
nopostgrad	50.9	56.1	49

As can be seen in Table 3, randomization was by and large very successful, with only small differences between the treatment groups.

TABLE 4: €20/€50/250 in one week vs 40/100/500 in one month[20]

VARIABLES	(1) onemonth40	(2) onemonth100	(4) onemonth500
lp	1.360	1.726	0.727
	(0.453)	(0.243)	(0.352)
tlp	0.693	0.965	0.880
	(0.285)	(0.928)	(0.715)
aep	1.400	0.869	1.488
	(0.374)	(0.697)	(0.288)
Constant	15***	17.13***	14.11***
	(0)	(0)	(0)
Observations	1,000	1,000	1,000
Pseudo R2	0.0103	0.00639	0.00827
Wald LP=AEP	0.9463	0.1315	0.0542*
Wald LP=TLP	0.0918*	0.2249	0.5853
Wald TLP=AEP	0.0562*	0.7795	0.166

In the first task, with the shortest delays and smallest rewards, no intervention appears to be effective. It should be noted however that there is a significant difference between the AEP and TLP intervention, and that the MCoT, the difference between the LP and TLP treatment effect, is also significant at a 10 % level though not at the 5 % level.

Once again in the second task no intervention is significant. While the LP intervention has an odds ratio above 1, it is far from significant and additionally not significantly different from either the TLP or AEP interventions.

At the final short-delay task there are, once again, no interventions with a significant treatment effect. Note however that the AEP intervention significantly

[20] Notes on Tables 4-12: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. The bottom three rows show the p-values for the Wald test statistics testing the hypothesis that one coefficient is equal to the other. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

outperforms the LP intervention at a 10 % level and is only slightly above the 5 % level.

TABLE 5: €20/€50/€250 in one month vs €40/€100/€500 in six months

VARIABLES	(1) sixmonths40	(2) sixmonths100	(3) sixmonths500
lp	2.247***	3.046***	1.410
	(0.000314)	(1.79e-05)	(0.190)
tlp	1.750***	1.724**	1.086
	(0.00754)	(0.0139)	(0.736)
aep	1.093	1.111	1.370
	(0.629)	(0.587)	(0.188)
Constant	2.200***	2.726***	4.913***
	(1.67e-09)	(0)	(0)
Observations	1,000	1,000	1,000
Pseudo R2	0.0168	0.0246	0.00328
Wald LP=AEP	0.0013***	0.0001***	0.9155
Wald LP=TLP	0.3084	0.0429**	0.3412
Wald TLP=AEP	0.0242**	0.0472**	0.3581

For the first time not just one but two interventions – LP and TLP – turn out to be significant. Furthermore, both these interventions are statistically significantly different from AEP, although not from one another: The MCoT is insignificant.

In the second medium-delay task the pattern continues, with LP and TLP being significant while AEP is not. However, it is notable that the difference between the LP and TLP variables – the MCoT – again reaches statistical significance. Furthermore, the differences between the LP and TLP effects and the AEP effect are again statistically significant.

No intervention proves statistically significant on the second high-stakes task, and no treatment effect is statistically significant from any other.

TABLE 6: €20/€50/€250 in six months vs €40/€100/€500 in twelve months

VARIABLES	(1) twelvemonths40	(2) twelvemonths100	(3) twelvemonths500
lp	1.714*	2.666***	1.018
	(0.0754)	(0.00328)	(0.950)
tlp	1.474	1.614*	0.709
	(0.174)	(0.0896)	(0.195)
aep	0.619**	0.855	1.021
	(0.0370)	(0.501)	(0.938)
Constant	6.351***	5.800***	7.774***
	(0)	(0)	(0)
Observations	1,000	1,000	1,000
Pseudo R2	0.0234	0.0229	0.00353
Wald LP=AEP	0.0004***	0.0005***	0.9927
Wald LP=TLP	0.6505	0.1678	0.2030
Wald TLP=AEP	0.0012***	0.0205**	0.1653

On the first long-delay task the LP intervention turns out to be significant at a 10 per cent level, whereas the AEP treatment is negatively significant, meaning it reduced the likelihood of participants choosing the larger-later option. Both the LP and TLP interventions outperform the AEP by a statistically significant margin, but the difference between them is not statistically significant.

Only the LP intervention reaches statistical significance at a 5 per cent level on the second long-delay task, although the TLP treatment is significant at a 10 per cent level. The MCoT is once again insignificant.

Finally, no intervention turns out to be effective on the final task of the experiment.

These results suggest that there is in fact a marginal cost of transparency, with the LP intervention outperforming the TLP intervention on 8/9 tasks. It should be noted that the Wald test indicates the difference is insignificant for all but two tasks.

Still, the pattern of LP more or less consistently outperforming TLP cannot not be ignored.

Nonetheless, the TLP intervention is significant at a 5 per cent level for two tasks and significant at a 10 per cent level for another, and thus the answer to the question “Do honest nudges work?” appears to be Yes – though they are not as effective as hidden nudges.

However, as will become clear when looking at the regression output that used the restricted sample, the picture is more complicated.

TABLE 7: €20/€50/€250 in one week vs €40/€100/€500 in one month (Restricted sample)

VARIABLES	(1) onemonth40	(2) onemonth100	(3) onemonth500
lp	2.406	2.281	0.905
	(0.148)	(0.236)	(0.847)
tlp	2.008	2.410	1.602
	(0.216)	(0.206)	(0.417)
aep	5.362**	1.831	8.671**
	(0.0324)	(0.339)	(0.0427)
Constant	7.273***	10.37***	9.111***
	(6.83e-10)	(2.63e-10)	(3.13e-10)
Observations	323	323	323
Pseudo R2	0.0411	0.0176	0.0515
Wald LP=AEP	0.3633	0.7787	0.0354**
Wald LP=TLP	0.7933	0.9475	0.3373
Wald TLP=AEP	0.2490	0.7253	0.1273

Immediately there is a clear difference in treatment effect compared to the regression that used the less restrictive sample inclusion criteria: While LP and TLP are still insignificant, the AEP intervention has a substantial, significant effect. It is however notable that no intervention has a significantly different effect compared to any other intervention as measured by the Wald test scores.

The results for the second short-delay task in the restricted sample are very similar to the unrestricted sample, with no intervention proving successful and no treatment effect substantially different from any other. Notable is that the MCoT is actually negative, with the TLP intervention ever so slightly outperforming the LP intervention.

In the final short delay task what stands out the most is once again the AEP treatment effect, which is statistically significant and also significantly different from the LP treatment effect as per the Wald score, which was also the case in the equivalent regression that used the larger sample. The MCoT is negative, but again insignificant.

TABLE 8: €20/€50/€250 in one month vs €40/€100/€500 in six months (Restricted sample)

VARIABLES	(1) sixmonths40	(2) sixmonths100	(3) sixmonths500
lp	2.032*	2.700**	1.511
	(0.0514)	(0.0155)	(0.321)
tlp	2.842***	3.691***	1.607
	(0.00652)	(0.00287)	(0.253)
aep	1.137	1.354	2.375*
	(0.695)	(0.384)	(0.0563)
Constant	1.935***	2.370***	3.789***
	(0.00283)	(0.000169)	(2.40e-07)
Observations	323	323	323
Pseudo R2	0.0283	0.0405	0.0147
Wald LP=AEP	0.1228	0.1084	0.3618
Wald LP=TLP	0.4321	0.5356	0.8938
Wald TLP=AEP	0.0206**	0.0280**	0.4300

Results here are similar to the same regression with the less restrictive sample. TLP is clearly significant while the p-value for the LP variable is 0.001 away from the significance threshold. AEP is clearly insignificant, as is the MCoT which once again is negative.

The pattern continues with the LP and TLP interventions being significant for the medium-delay tasks, while the AEP remains insignificant. The MCoT is negative and insignificant.

The LP and TLP interventions are insignificant just as was the case with the larger sample, but the AEP intervention is now significant. The MCoT is once again negative and insignificant.

TABLE 9: €20/€50/€250 in six months vs €40/€100/€500 in twelve months (Restricted sample)

VARIABLES	(1) twelvemonths40	(2) twelvemonths100	(3) twelvemonths500
lp	0.905	2.096	1.253
	(0.847)	(0.184)	(0.642)
tlp	1.602	3.797**	1.165
	(0.417)	(0.0449)	(0.746)
aep	0.439*	0.783	1.873
	(0.0666)	(0.572)	(0.232)
Constant	9.111***	6.583***	6.583***
	(3.13e-10)	(1.18e-09)	(1.18e-09)
Observations	323	323	323
Pseudo R2	0.0314	0.0425	0.00700
Wald LP=AEP	0.1213	0.0752*	0.4774
Wald LP=TLP	0.3373	0.4276	0.8869
Wald TLP=AEP	0.0165**	0.0171**	0.3899

No intervention is significant for the first of the long-delay tasks, although notably the AEP intervention may if anything actually reduce the likelihood of participants choosing the larger-later option. Once again, the TLP intervention outperforms the LP intervention meaning the MCoT is negative, though it is also once again insignificant.

In the second long-delay task, only the TLP intervention is significant and the MCoT is again negative, although as usual insignificant.

On the final task no intervention turns out to be significant, and there is a positive but clearly insignificant MCoT.

Discussion of results

What stands out is that the TLP treatment outperforms the LP treatment in the regression sets where all participants who failed either attentiveness discerning question have been dropped, with a negative MCoT on seven of nine tasks. Although the Wald scores suggests these are not significant differences, in my view this is a pattern that should not be ignored, though of course one should also caution against drawing too strong conclusions based on this, especially considering the relatively small sample.

What these results suggest is that TLP works better when participants pay more attention, which at first seems counterintuitive, though it is in line with previous experimental studies as discussed in the introduction, as those who are not paying attention ought to be more likely to miss the disclaimer revealing the existence and purpose of the default option nudge. In other words, it appears the disclaimer has if anything a positive impact on the efficacy of the nudge.

This brings us to the question of how consumers can be nudged even when they know that they are being nudged. The likeliest explanation in my view is that during any LP treatment, some people will figure out that they are being manipulated and “lash out” against the choice architect by actively doing the opposite of what the architect wants. In this case, once a participant realizes that the choice architect is trying to manipulate him/her to choose the larger-later option, and out of resentment over this manipulation, he/she then chooses the smaller-sooner option. Gustavsson (2016) showed that the effect of an LP treatment may taper off if repeated often enough, presumably as more and more participants figure out what the choice architect is doing and lash out against it. Evidence of this type of backlash against nudges has also been documented by Arad and Rubenstein (2018) in a series of experiments. In this experiment we do not see the same strong pattern of a treatment effect tapering off, but it should also be noted that the Gustavsson (2016) study had a significantly younger sample and the Arad and Rubenstein experiments had student-only samples. It may be the case that younger people are more prone both to at first go along with a nudge, and then to ‘act out’ if they discover its presence and feel that they are being manipulated, which in this case they would do by doing the opposite of what the choice architect wants them to.

Why would this kind of backlash not occur with the TLP intervention? Quite simply nudging appears to be a case where honesty pays. By informing the participants that there is a nudge and why, participants no longer feel the need to “lash out” against the choice architect once they found out, as they do not experience the same feeling of having been deceived and manipulated.

Additionally, it cannot be ruled out that participants are still affected by the default option on a psychological level even though they know why it is there, similar to how humans can experience a placebo effect even when they know that they are taking a placebo (Schafer, Colloca and Wager, 2015).

However, the question remains as to why the TLP underperforms the LP intervention in the larger sample group. Suppose that many of those who are not paying full attention are not reading the TLP disclaimer; should they not logically act the same as those in the LP group? Instead, they seem to be less prone to choose the larger-later option than they would have been if, like for the LP group, the disclaimer had not been there at all.

One possible explanation could be that those who were not paying full attention may in fact have hastily read the disclaimer and picked up the point that someone was trying to make them choose something, but not further reflected on why or whether this was done in their best interest. This would then trigger some of them to reject the option they were being pushed towards because they dislike the idea of someone trying to manipulate their choices. Unlike in the LP group, where some participants may not realize that they are being pushed towards a certain option, in the TLP group everyone who had paid any attention to the disclaimer would have known what was going on. Those who read but did not reflect on the disclaimer may therefore have been more prone to backlash than those in the LP group.

There is also another possible interpretation of the difference between the two samples; rather than attention, it may be the case that those who only managed to answer one attentiveness discerning question correctly have a poorer short-term memory compared to those who got both of them right. This would provide an alternative explanation as to why there is a large difference in AEP treatment effect between the full and the restricted sample; participants who cannot remember the list of reasons provided for the larger-later option obviously cannot be affected by them. This would not however explain why the TLP intervention performs worse than the

LP intervention, as it seems intuitively unlikely these participants could forget what the disclaimer said as it was rather short.

It is worth keeping in mind that a large proportion of the participants in this survey are Swedish[21]. While English proficiency in Sweden is very high it cannot be ruled out that some participants in the TLP group did not understand the meaning of the disclaimer informing them about the nudge. It is however unlikely that this had any greater effect on the results as it is unlikely that many participants simultaneously had a such a poor grasp of English that they could not understand the disclaimer while simultaneously a good enough grasp of English to pass at least one attentiveness discerning question.

Turning attention to the AEP treatment, there is a great difference between the two sets where participants needed only to have passed one attentiveness discerning question and those where they needed to have passed both. AEP has a great effect in the latter case, likely because, as discussed, these participants were paying more attention to the experiment, which likely translated to paying more attention to the list of arguments provided in the AEP treatment. It should not come as a surprise to anyone that in order for a list of arguments to be effective in convincing a consumer to pursue a certain course of action, the consumer has to pay attention to the arguments.

The AEP treatment also appears to work better when rewards are large. This may be because participants are more likely to stop and consider their actions carefully when large amounts are at stake. This may have made the list of arguments more persuasive than with the smaller rewards participants may have simply not

[21] This was a side effect of extensive promotion of the experiment on Swedish-language websites.

bothered to think too hard about the decision and instead just used their intuition. The exception is the first task where, in the restricted sample, the AEP intervention is in fact significant. This is likely due to participants having just read the arguments and thus having them fresh in mind.

A separate set of regressions was estimated using only those participants who stated that they reside in western Europe (Tables 17-19). Differences in treatment effects were small, suggesting western Europeans are not reacting substantially differently to the interventions.

Another separate set used only those participants whose highest educational achievement was a high school diploma or less, and who were not currently studying. All treatments proved far less effective, suggesting that changing preferences among less educated individuals may require other measures. This is a notable finding that would not have been possible if not for the use of a mixed sample (as opposed to the more common student-only samples). Most notably the TLP intervention turned out not to be significant in any intertemporal choice task, while with the full sample, the TLP intervention turned out to be significant twice at a 5 % significance level, and once at a 10 % significance level. This suggests that lower educated individuals may take offense at the idea of an authority trying to guide their choices, even if the authority in question is being transparent. It should be noted that the sample size is relatively small (N=244), and further research is necessary to confirm whether or not this is in fact the case.

Estimates of sensitivity and specificity were in general poor, in particular specificity (see Appendix B, Tables 20-22). This, however, is to be expected as there are many other variables that affect a person's intertemporal choice preferences, and the purpose of this study is not to identify all these variables but rather to determine the impact of the treatment variables.

My first hypothesis stated that the TLP intervention would be ineffective. This is clearly not the case.

My second hypothesis stated that the LP intervention would be just as effective as the AEP intervention. This is true in the restricted sample (Tables 7-9), while LP easily outperforms AEP among the larger sample (Tables 4-6).

In summary these results suggest that honest nudges do work reasonably well and may even be preferable to hidden nudges provided that participants can be assumed to be paying attention during the decision-making process. If stakes are high, it may be better to forfeit nudges altogether in favor of an AEP intervention.

4. Conclusions

These results indicate that nudging can work even if conducted in an open, transparent manner, at least in the context of intertemporal choice. While that may be seen as a victory for libertarian paternalism, if we accept these results it also means that the way that nudges are commonly being used today – without transparency – is not just an ethically questionable way of changing consumer behavior, but an ethically questionable way that carries little or no gain as the same results can be achieved through transparent means, though, as discussed, this depends on circumstances.

Furthermore, these findings also suggest that AEP under the right circumstances may be even better than LP/TLP, and it can be argued that if the same or similar results can be obtained using an AEP treatment, then an AEP treatment should be used as it relies on informing consumers. TLP, while more ethical than LP, still will not teach a consumer anything he or she did not already know. When the TLP nudge is gone, the consumer's behavior is likely to revert back to what it was before the consumer was nudged.

Judging from the results of this experiment, the problem with AEP and quite possibly also TLP interventions is that they require consumers to pay attention for them to be effective, while LP interventions seem to work regardless, which makes sense as AEP still requires an active choice. From a policy standpoint, this means that AEP/TLP interventions should mainly be used when one can be reasonably assured that consumers will be paying attention, and they should be designed in such a way as to grab attention. Finally, it is important to note that some AEP interventions may be less reliant on consumers paying attention to them, such as mandatory cooling off periods.

Future research will investigate whether there may be a marginal cost of transparency in contexts other than intertemporal choice and for LP interventions other than default options, and future experiments will also be provided in several languages to ensure that all participants understand the instructions.

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Appendix A: Survey

Welcome!

My name is John Gustavsson and I'm a PhD student at Maynooth University at the Department of Economics, Finance and Accounting.

This survey is an experiment that is part of the research I am doing for my doctoral thesis. In this survey, you will be asked a number of questions about how you value future income relative to present income – what we economists call "inter-temporal choice".

You will be posed with a number of scenarios and asked how you would act in them (there will be two options in each scenario). These are not purely hypothetical scenarios; three (3) of you who answer this survey will be paid in accordance with how you answer one (1) of the scenarios. The three who are paid will be randomly selected; your answers have no bearing whatsoever on your likelihood of being one of them.

The final part of this survey contains demographic questions (age, gender, what part of the world you live in, education level, whether you are currently a student and marital status) as well as some questions on consumer behavior and attitudes. If you are uncomfortable with answering a demographic question, simply choose the option "I'd rather not say" (or write N/A in the box) which is provided for every demographic question. All data will be stored in a password-protected folder stored in the university system, and there will be no further use of the data beyond this study.

You will be asked to provide me with your email address at the end of the survey – this is so that I can contact you in case you are one of those who have been selected to be paid. You are not required to provide your email address, but if you don't I won't be able to pay you. You will not need to provide your bank account details to receive payment. The email addresses will be stored (in a separate password-protected folder) only until the selected participants have been paid, while the rest of the data will be retained for research purposes. You may quit the survey at any time; if you quit before finishing the survey, your data will be deleted. You can also withdraw your data at any time by emailing me at the email address provided below.

It must be recognized that, in some circumstances, confidentiality of research data and records may be overridden by courts in the event of litigation or in the course of investigation by lawful authority. In such circumstances the University will take all reasonable steps within law to ensure that confidentiality is maintained to the greatest possible extent.

If you're interested in learning the findings of this study you're more than welcome to do so; simply indicate your interest when answering the final question.

If you have any questions or you wish to contact me for any reason, you can reach me at john.gustavsson.2010@mumail.ie.

You must be 18 or older to participate in this survey. This survey will take approximately 15 minutes to complete, obviously depending on how much time you spend thinking about your decisions. Please read the descriptions on the next page carefully.

By proceeding, you agree to take part in this survey, and have your data stored under the conditions outlined above. Thank you for your participation!

Q1: What time of the month is your birthday?

Between the 1^{st} and 7^{th} of the month

Between the 8th and 14th of the month

Between the 15th and 21st of the month

After the 22nd of the month

CONTROL GROUP

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 88 USD, 390 SEK or 36 Pound Sterling.

Q2: You win the lottery and your prize is to receive either 40 euro in one month, or 20 euro in one week. What do you choose?

20 euro in one week

40 euro in one month

Q3: You win the lottery and your prize is to receive either 100 euro in one month, or 50 euro in one week. What do you choose?

50 euro in one week

100 euro in one month

Q4: You win the lottery and your prize is to receive either 500 euro in one month, or 250 euro in one week. What do you choose?

250 euro in one month

500 euro in one month

Q5: You win the lottery and your prize is to receive either 40 euro in six months, or 20 euro in one month. What do you choose?

20 euro in one month

40 euro in six months

Q6: You win the lottery and your prize is to receive either 100 euro in six months, or 50 euro in one month. What do you choose?

50 euro in one month

100 euro in six months

Q7: You win the lottery and your prize is to receive either 500 euro in six months, or 250 euro in one month. What do you choose?

250 euro in one month

500 euro in six months

Q8: You win the lottery and your prize is to receive either 40 euro in twelve months, or 20 euro in six months. What do you choose?

20 euro in six months

40 euro in twelve months

Q9: You win the lottery and your prize is to receive either 100 euro in twelve months, or 50 euro in six months. What do you choose?

50 euro in six months

100 euro in twelve months

Q10: You win the lottery and your prize is to receive either 500 euro in twelve months, or 250 euro in six months. What do you choose?

250 euro in six months

500 euro in twelve months

AEP TREATMENT GROUP

Below, you will be presented with a number of scenarios – you will be asked to choose between a smaller reward received soon, and a larger reward received later.

Before you make your choices, here are a few things that I would like you to take into account:

1) Choosing the “later” option means you have something to look forward to.

2) Saving means you’ll be better off in the event of a “rainy day”

3) Every decision that we make is influenced by the choices we’ve made in the past. By choosing the larger-later option now, it’ll be easier to do the same in the future – you can establish (or strengthen an already existing) good habit.

4) The interest rate is 100 %, or to put it another way on an annual basis in the first three scenarios (one week vs one month, see below) the interest rate is 170 681%, while in the second (one month vs six months) and last third (six months vs twelve months) of the scenarios it is 428 % and 300 % respectively.

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 88 USD, 390 SEK or 36 Pound Sterling.

Q11: You win the lottery and your prize is to receive either 40 euro in one month, or 20 euro in one week. What do you choose?

20 euro in one week

40 euro in one month

Q12: You win the lottery and your prize is to receive either 100 euro in one month, or 50 euro in one week. What do you choose?

50 euro in one week

100 euro in one month

Q13: You win the lottery and your prize is to receive either 500 euro in one month, or 250 euro in one week. What do you choose?

250 euro in one month

500 euro in one month

Q14: You win the lottery and your prize is to receive either 40 euro in six months, or 20 euro in one month. What do you choose?

20 euro in one month

40 euro in six months

Q15: You win the lottery and your prize is to receive either 100 euro in six months, or 50 euro in one month. What do you choose?

50 euro in one month

100 euro in six months

Q16: You win the lottery and your prize is to receive either 500 euro in six months, or 250 euro in one month. What do you choose?

250 euro in one month

500 euro in six months

Q17: You win the lottery and your prize is to receive either 40 euro in twelve months, or 20 euro in six months. What do you choose?

20 euro in six months

40 euro in twelve months

Q18: You win the lottery and your prize is to receive either 100 euro in twelve months, or 50 euro in six months. What do you choose?

50 euro in six months

100 euro in twelve months

Q19: You win the lottery and your prize is to receive either 500 euro in twelve months, or 250 euro in six months. What do you choose?

250 euro in six months

500 euro in twelve months

Q20: Which argument in favor of choosing the larger-later option did you find the most convincing?

The “Something to look forward to”-argument

The “Rainy day”-argument

The “good habit”-argument

The interest rate-argument

No difference

I didn’t find any argument convincing

I didn’t read them

LP TREATMENT GROUP

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 88 USD, 390 SEK or 36 Pound Sterling.

Q21: You’ve won the lottery and your prize is to receive 40 euro in one month, or 20 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

[]

Q22: You’ve won the lottery and your prize is to receive 100 euro in one month, or 50 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

[]

Q23: You've won the lottery and your prize is to receive 500 euro in one month, or 250 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

[]

Q24: You've won the lottery and your prize is to receive 40 euro in six months, or 20 euro in one month. If you would prefer to receive 20 euro in one month, please tick this box.

[]

Q25: You've won the lottery and your prize is to receive 100 euro in six months, or 50 euro in one month. If you would prefer to receive 50 euro in one month, please tick this box.

[]

Q26: You've won the lottery and your prize is to receive 500 euro in six months, or 250 euro in one month. If you would prefer to receive 250 euro in one month, please tick this box.

[]

Q27: You've won the lottery and your prize is to receive 40 euro in twelve months, or 20 euro in six months. If you would prefer to receive 20 euro in six months, please tick this box.

[]

Q28: You've won the lottery and your prize is to receive 100 euro in twelve months, or 50 euro in six months. If you would prefer to receive 50 euro in six months, please tick this box.

[]

Q29: You’ve won the lottery and your prize is to receive 500 euro in twelve months, or 250 euro in six months. If you would prefer to receive 250 euro in six months, please tick this box.

[]

TLP TREATMENT GROUP

BEFORE YOU PROCEED, BE AWARE THAT THE DEFAULT OPTION FOR THIS SECTION IS THE LARGER-LATER OPTION (RECEIVING 40/100/500 EURO AFTER A LONGER DELAY RATHER THAN 20/50/250 AFTER A SHORTER). DEFAULT OPTIONS ARE KNOWN TO AFFECT THE DECISIONS MADE BY CONSUMERS AND THE DEFAULT OPTION HAS BEEN SET THIS WAY TO HELP YOU MAKE GOOD, FORWARD-LOOKING CHOICES.

NOTE: 20 euro equals approximately 22 USD, 195 SEK or 18 Pound sterling. 40 euro equals approximately 44 USD, 390 SEK or 36 Pound Sterling.

Q30: You’ve won the lottery and your prize is to receive 40 euro in one month, or 20 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

[]

Q31: You’ve won the lottery and your prize is to receive 100 euro in one month, or 50 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

[]

Q32: You’ve won the lottery and your prize is to receive 500 euro in one month, or 250 euro in one week. If you would prefer to receive 20 euro in one week, please tick this box.

[]

Q33: You’ve won the lottery and your prize is to receive 40 euro in six months, or 20 euro in one month. If you would prefer to receive 20 euro in one month, please tick this box.

[]

Q34: You’ve won the lottery and your prize is to receive 100 euro in six months, or 50 euro in one month. If you would prefer to receive 50 euro in one month, please tick this box.

[]

Q35: You’ve won the lottery and your prize is to receive 500 euro in six months, or 250 euro in one month. If you would prefer to receive 250 euro in one month, please tick this box.

[]

Q36: You’ve won the lottery and your prize is to receive 40 euro in twelve months, or 20 euro in six months. If you would prefer to receive 20 euro in six months, please tick this box.

[]

Q37: You’ve won the lottery and your prize is to receive 100 euro in twelve months, or 50 euro in six months. If you would prefer to receive 50 euro in six months, please tick this box.

[]

Q38: You’ve won the lottery and your prize is to receive 500 euro in twelve months, or 250 euro in six months. If you would prefer to receive 250 euro in six months, please tick this box.

[]

DEMOGRAPHIC QUESTIONS

Q39: Please state your age

18-23

24-35

36-64

65+

I’d rather not say

Q40: What part of the world do you reside in?

Western Europe

Eastern Europe

Southern Europe

North America

Central America

Australia/NZ/Oceania

Southeast Asia

Middle east

Africa

South America

I’d rather not say

Q41: Are you married?

Yes

No

I'd rather not say

Q42: Are you a full-time student (or a graduate of the class of 2016)?

Yes

No

I'd rather not say

Q43: How many participants who take this survey will be paid?

1

3

5

6

7

Q44: Thinking about your personal finances, do you think you should save more than you currently do?

Yes

No

Don't know

I'd rather not say

Q45: If Yes, why don't you?

I don't feel like I can afford it

Lack of motivation

Forgetfulness

Other/I'd rather not say

Q46: What gender do you identify as?

Male

Female

I’d rather not say

Q47: Do you think you are prone to be affected by psychologically manipulative tactics (such as those commonly employed by advertisers) when making consumer decisions

Yes

No

I don’t know

Q48: What is the highest level of education that you have achieved?

High school/Post-primary school or less

Undergraduate degree

Postgraduate degree/Postgraduate diploma

I’d rather not say

Q49: Do you currently save regularly?

Yes, through a pension plan

Yes, privately/both privately and through a pension plan

No

I’d rather not say

Q50: How many intertemporal choice scenarios (questions where you were asked to choose between a smaller-sooner and larger-later reward) were there on the previous page?

7

6

9

12

15

Q51: Do you think that your consumer choices are affected by the order that the options (such as, items in a shop) are presented in?

Yes

No

I don’t know

Q52: Do you think people in general should save more, less or about the same as now?

More

Same

Less

No opinion

Q53: Do you think that saving is a moral obligation for those who are able to save?

Yes

No

Q54: Do you usually plan your consumption ahead of time (budgeting)?

Yes

No

I’d rather not say

Q55: Thinking back, do you think your attitude towards saving and whether it’s important has changed as you’ve grown older?

Yes, I’m more positive to saving today than when I was younger

Yes, I’m more negative to saving today than when I was younger

No

I’d rather not say

Q56: Please provide your email address in the field below (this is voluntary but it’s necessary for you to have a chance to be paid as I need to be able to get in touch with you).

Q57: Do you have any comments, questions or feedback in general? If you would like to take part of the findings from this study, please indicate this here.

Appendix B: Additional statistical analysis

TABLE 10: €20/€50/€250 in one week vs €40/€100/€500 in one month (control variables included)

VARIABLES	(1) onemonth40	(2) onemonth100	(3) onemonth500
lp	1.789	1.861	0.792
	(0.210)	(0.213)	(0.521)
tlp	0.648	0.909	0.838
	(0.214)	(0.810)	(0.621)
aep	1.510	0.903	1.576
	(0.307)	(0.787)	(0.254)
agebelow36	0.766	0.834	1.416
	(0.423)	(0.609)	(0.316)
male	1.148	0.988	0.738
	(0.711)	(0.975)	(0.469)
nopostgrad	1.005	1.243	0.745
	(0.988)	(0.482)	(0.296)
Constant	14.65***	16.34***	20.63***
	(2.79e-09)	(6.80e-09)	(4.94e-10)
Observations	944	944	944
Pseudo R2	0.0203	0.00894	0.0147
Wald LP=AEP	0.7314	0.1388	0.0889*
Wald LP=TLP	0.0244**	0.1553	0.8765
Wald TLP=AEP	0.0291**	0.9858	0.1138

TABLE 11: €20/€50/€250 in one month vs €40/€100/€500 in six months (control variables included)

VARIABLES	(1) sixmonths40	(2) sixmonths100	(3) sixmonths500
lp	2.082***	2.803***	1.297
	(0.00185)	(9.63e-05)	(0.335)
tlp	1.591**	1.618**	0.996
	(0.0331)	(0.0332)	(0.987)
aep	1.074	1.135	1.404
	(0.718)	(0.534)	(0.180)
agebelow36	0.392***	0.551***	0.868
	(8.22e-08)	(0.00141)	(0.520)
male	1.407*	1.165	0.859
	(0.0882)	(0.477)	(0.568)
nopostgrad	0.840	0.896	0.797
	(0.295)	(0.529)	(0.253)
Constant	2.510***	3.056***	6.916***
	(0.000180)	(1.71e-05)	(9.83e-10)
Observations	944	944	944
Pseudo R2	0.0536	0.0355	0.00714
Wald LP=AEP	0.0050***	0.0007***	0.7795
Wald LP=TLP	0.2906	0.0534*	0.3443
Wald TLP=AEP	0.0725*	0.1202	0.1920

TABLE 12: €20/€50/€250 in six months vs €40/€100/€500 in twelve months (control variables included)

VARIABLES	(1) twelvemonths40	(2) twelvemonths100	(3) twelvemonths500
lp	1.607	2.933***	0.963
	(0.132)	(0.00256)	(0.898)
tlp	1.374	1.562	0.678
	(0.276)	(0.118)	(0.148)
aep	0.591**	0.845	1.073
	(0.0289)	(0.484)	(0.801)
agebelow36	0.849	0.923	1.232
	(0.469)	(0.734)	(0.398)
male	2.276***	1.731**	1.434
	(0.000331)	(0.0255)	(0.165)
nopostgrad	0.760	0.765	0.665*
	(0.191)	(0.216)	(0.0582)
Constant	4.195***	4.418***	7.057***
	(9.88e-07)	(9.03e-07)	(1.62e-09)
Observations	944	944	944
Pseudo R2	0.0448	0.0360	0.0125
Wald LP=AEP	0.0008***	0.0004***	0.7172
Wald LP=TLP	0.6458	0.1011	0.2249
Wald TLP=AEP	0.0021***	0.0279**	0.0923*

TABLE 13: Variance inflation factor (VIF)

Variable		**VIF**
Male		3.11
nopostgrad		2.26
aep		1.63
tlp		1.55
lp		1.51
agebelow36		1.41
Mean VIF		**1.91**

TABLE 14: €20/€50/€250 in one week vs €40/€100/€500 in one month (exact logistic regression)[22]

VARIABLES	(1) onemonth40	(2) onemonth100	(3) onemonth500
lp	2.394339	2.270539	.9060348
	(0.2230)	(0.3706)	(1.0000)
tlp	5.318547**	1.825082	8.589063**
	(0.0329)	(0.5078)	(0.0307)
aep	1.999577	2.397974	1.598086
	(0.3209)	(0.3247)	(0.5950)
Observations	323	323	323

TABLE 15: €20/€50/€250 in one month vs €40/€100/€500 in six months (exact logistic regression)

VARIABLES	(1) sixmonths40	(2) sixmonths100	(3) sixmonths500
lp	2.02359*	2.684273**	1.507643
	(0.0717)	(0.0205)	(0.4290)
tlp	1.135816	1.352048	2.363414*
	(0.8205)	(0.4858)	(0.0800)
aep	2.824378***	3.664153***	1.602881
	(0.0084)	(0.0029)	(0.3438)
Observations	323	323	323

[22] Notes on Tables 14-16: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

TABLE 16: €20/€50/€250 in six months vs €40/€100/€500 in twelve months (exact logistic regression)

VARIABLES	(1) twelvemonths40	(2) twelvemonths100	(3) twelvemonths500
lp	.9060348	2.087289	1.251469
	(1.0000)	(0.2732)	(0.8271)
tlp	.4411514*	.7839891	1.86676
	(0.0987)	(0.7256)	(0.3379)
aep	1.598086	3.770943*	1.163513
	(0.5950)	(0.0580)	(0.9322)
Observations	323	323	323

TABLE 17: €20/€50/€250 in one week vs €40/€100/€500 in one month (Western Europe-only sample)[23]

VARIABLES	(1) onemonth40	(2) onemonth100	(3) onemonth500
lp	1.546	1.984	0.739
	(0.332)	(0.206)	(0.418)
tlp	0.661	0.887	0.752
	(0.255)	(0.780)	(0.444)
aep	1.419	0.807	1.190
	(0.391)	(0.591)	(0.656)
Constant	14.80***	18.75***	14.80***
	(0)	(0)	(0)
Observations	864	864	864
Pseudo R2	0.0139	0.0105	0.00504
Wald LP=AEP	0.8571	0.0872*	0.2177
Wald LP=TLP	0.0522*	0.1429	0.9633
Wald TLP=AEP	0.0533*	0.8170	0.2344

[23] Notes on Tables 17-19: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. The bottom three rows show the p-values for the Wald test statistics testing the hypothesis that one coefficient is equal to the other. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

TABLE 18: €20/€50/€250 in one month vs €40/€100/€500 in six months (Western Europe-only sample)

VARIABLES	(1) sixmonths40	(2) sixmonths100	(3) sixmonths500
lp	2.758***	3.626***	1.573
	(5.16e-05)	(1.18e-05)	(0.106)
tlp	1.864***	1.863**	1.133
	(0.00652)	(0.0113)	(0.632)
aep	1.162	1.075	1.466
	(0.454)	(0.731)	(0.136)
Constant	2.203***	2.823***	4.643***
	(1.76e-08)	(0)	(0)
Observations	864	864	864
Pseudo R2	0.0233	0.0330	0.00518
Wald LP=AEP	0.0006***	0.0000***	0.8102
Wald LP=TLP	0.1549	0.0379**	0.2674
Wald TLP=AEP	0.0404**	0.0254**	0.3472

TABLE 19: €20/€50/€250 in six months vs €40/€100/€500 in twelve months (Western Europe-only sample)

VARIABLES	(1) twelvemonths40	(2) twelvemonths100	(3) twelvemonths500
lp	2.462**	4.097***	1.393
	(0.0137)	(0.000482)	(0.293)
tlp	1.386	1.650*	0.734
	(0.291)	(0.0972)	(0.260)
aep	0.593**	0.853	1.064
	(0.0367)	(0.521)	(0.822)
Constant	6.645***	5.583***	6.900***
	(0)	(0)	(0)
Observations	864	864	864
Pseudo R2	0.0343	0.0363	0.00679
Wald LP=AEP	0.0000***	0.0001***	0.3963
Wald LP=TLP	0.1441	0.0363**	0.0414**
Wald TLP=AEP	0.0034***	0.0254**	0.1788

TABLE 20: Sensitivity and specificity (Full set, no control variables)[24]

	Sensitivity (%)	Specificity (%)	Area under ROC curve
Onemonth40	100	0	0.5766
Onemonth100	100	0	0.5543
Onemonth500	100	0	0.5695
Sixmonths40	100	0	0.5841
Sixmonths100	100	0	0.6017
Sixmonths500	100	0	0.5397
Twelvemonths40	100	0	0.6090
Twelvemonths100	100	0	0.6053
Twelvemonths500	100	0	0.5349

TABLE 21: Sensitivity and specificity (Full set, control variables included)

	Sensitivity (%)	Specificity (%)	Area under ROC curve
Onemonth40	100	0	0.6225
Onemonth100	100	0	0.5780
Onemonth500	100	0	0.5994
Sixmonths40	97.73%	6.72%	0.6593
Sixmonths100	100	0	0.6269
Sixmonths500	100	0	0.5669
Twelvemonths40	100	0	0.6578
Twelvemonths100	100	0	0.6348
Twelvemonths500	100	0	0.5808

[24] Notes on Tables 20-22: The cutoff score when estimating sensitivity and specificity is 0.5. The first column shows a list of dependent variables, with the columns next to it displaying the sensitivity, specificity and area under ROC curve for the regressions on these variables.

TABLE 22: Sensitivity and specificity (Restricted set)

	Sensitivity (%)	Specificity (%)	Area under ROC curve
Onemonth40	100	0	0.6529
Onemonth100	100	0	0.5989
Onemonth500	100	0	0.6507
Sixmonths40	100	0	0.6100
Sixmonths100	100	0	0.6367
Sixmonths500	100	0	0.5844
Twelvemonths40	100	0	0.6242
Twelvemonths100	100	0	0.6475
Twelvemonths500	100	0	0.5586

TABLE 23: €20/€50/€250 in one week vs €40/€100/€500 in one month (Including only participants below Age 36)[25]

VARIABLES	(1) onemonth40	(2) onemonth100	(3) onemonth500
lp	1.485	1.219	0.409
	(0.589)	(0.793)	(0.236)
tlp	0.855	1.219	0.710
	(0.804)	(0.793)	(0.683)
aep	3.000	2.463	2.913
	(0.188)	(0.292)	(0.360)
Constant	11.00***	13.40***	23.00***
	(4.690)	(6.212)	(13.56)
Observations	244	244	244
Pseudo R2	0.0246	0.0127	0.0467
Wald LP=AEP	0.4506	0.4506	0.0773*
Wald LP=TLP	0.4662	1.0000	0.4662
Wald TLP=AEP	0.1434	0.4506	0.2276

[25] Notes on Tables 23-28: The tables show logistic regression coefficients as odds ratios for the variables listed in the left column. Numbers below the odds ratios are p-values. Each column contains the result for a different dependent variable, the name of which can be found at the top of the column. The bottom three rows show the p-values for the Wald test statistics testing the hypothesis that one coefficient is equal to the other. Statistically significant results are marked with asterisks with * denoting significant at 90%, ** significant at 95%, *** significant at 99% confidence level

TABLE 24: €20/€50/€250 in one months vs €40/€100/€500 in six months (Including only participants below Age 36)

VARIABLES	(1) sixmonths40	(2) sixmonths100	(3) sixmonths500
lp	2.111**	2.662**	1.328
	(0.046)	(0.018)	(0.560)
tlp	1.788	1.762	1.153
	(0.116)	(0.145)	(0.763)
aep	2.049**	2.524**	1.810
	(0.038)	(0.014)	(0.216)
Constant	0.895	1.400	4.143***
	(0.211)	(0.335)	(1.234)
Observations	244	244	244
Pseudo R2	0.0182	0.0291	0.00776
Wald LP=AEP	0.7213	0.3960	0.3907
Wald LP=TLP	0.6839	0.3665	0.7910
Wald TLP=AEP	0.9384	0.9051	0.3907

TABLE 25: €20/€50/€250 in six months vs €40/€100/€500 in twelve months (Including only participants below Age 36)

VARIABLES	(1) twelvemonths40	(2) twelvemonths100	(3) twelvemonths500
lp	0.484	0.992	0.597
	(0.121)	(0.987)	(0.326)
tlp	1.516	1.383	0.804
	(0.474)	(0.551)	(0.692)
aep	0.622	1.182	2.000
	(0.296)	(0.730)	(0.277)
Constant	6.200***	5.545***	8.000***
	(2.113)	(1.817)	(3.000)
Observations	244	244	244
Pseudo R2	0.0247	0.00246	0.0243
Wald LP=AEP	0.5668	0.7381	0.0560*
Wald LP=TLP	0.0448**	0.5667	0.5876
Wald TLP=AEP	0.1103	0.7808	0.1647

TABLE 26: €20/€50/€250 in one week vs €40/€100/€500 in one month (Including only participants whose highest educational qualification is a high school diploma or less)

VARIABLES	(1) onemonth40	(2) onemonth100	(3) onemonth500
lp	4.052	3.186	1.158
	(0.209)	(0.307)	(0.828)
tlp	1.121	1.831	1.368
	(0.870)	(0.495)	(0.641)
aep	2.586	1.333	2.070
	(0.267)	(0.714)	(0.320)
Constant	11.60***	14.75***	9.500***
	(1.45e-07)	(1.90e-07)	(1.56e-07)
Observations	229	229	229
Pseudo R2	0.0318	0.0168	0.00922
Wald LP=AEP	0.7173	0.4569	0.4617
Wald LP=TLP	0.2579	0.6551	0.8205
Wald TLP=AEP	0.3455	0.7339	0.5989

TABLE 27: €20/€50/€250 in one month vs €40/€100/€500 in six months (Including only participants whose highest educational qualification is a high school diploma or less)

VARIABLES	(1) sixmonths40	(2) sixmonths100	(3) sixmonths500
lp	1.848	3.178**	1.105
	(0.201)	(0.0359)	(0.852)
tlp	1.355	1.930	0.985
	(0.482)	(0.154)	(0.976)
aep	1.063	1.393	1.274
	(0.881)	(0.432)	(0.637)
Constant	2.706***	2.706***	5.300***
	(0.000453)	(0.000453)	(1.32e-06)
Observations	229	229	229
Pseudo R2	0.00831	0.0246	0.00162
Wald LP=AEP	0.2529	0.1451	0.7991
Wald LP=TLP	0.5399	0.4029	0.8340
Wald TLP=AEP	0.5771	0.4963	0.6252

TABLE 28: €20/€50/€250 in one month vs €40/€100/€500 in six months (Including only participants whose highest educational qualification is a high school diploma or less)

VARIABLES	(1) twelvemonths40	(2) twelvemonths100	(3) twelvemonths500
lp	1.600	4.865**	1.239
	(0.466)	(0.0466)	(0.684)
tlp	1.212	2.750	1.269
	(0.738)	(0.101)	(0.637)
aep	0.756	1.428	1.974
	(0.586)	(0.479)	(0.210)
Constant	6.875***	4.727***	4.727***
	(3.49e-07)	(2.86e-06)	(2.86e-06)
Observations	229	229	229
Pseudo R2	0.00999	0.0407	0.00900
Wald LP=AEP	0.2314	0.1328	0.4321
Wald LP=TLP	0.6821	0.5212	0.9656
Wald TLP=AEP	0.3938	0.3077	0.4421

Appendix C: Maynooth University Research Ethics Committee letter of approval

MAYNOOTH UNIVERSITY RESEARCH ETHICS COMMITTEE
MAYNOOTH UNIVERSITY,
MAYNOOTH, CO. KILDARE, IRELAND

Dr Carol Barrett
Secretary to Maynooth University Research Ethics Committee

17 October 2016

John Gustavsson
Department of Economics, Finance and Accounting
Maynooth University

RE: Application for Ethical Approval for a project entitled: The Marginal Cost of Transparency: An experimental investigation

Dear John,

The above project has been evaluated under Tier 2 process, Expedited review and we would like to inform you that ethical approval has been granted.

This ethical approval will expire on 31th October 2017.

Kind Regards,

Dr Carol Barrett
Secretary, Maynooth University Research Ethics Committee

C.c Professor Rowena Pecchenino
Department of Economics, Finance and Accounting

SRESC-2016-072

Conclusion

This thesis studied critical issues in behavioral economics. Its key findings are that so-called "irrational" biases generally do not reduce and may enhance individual welfare and that nudging can both be ethical and effective, at least in the short run. Issues with survey methods and research design that were encountered in this research must be acknowledged and addressed and new research questions that arose can be pursued in future research.

Chapter 1 concluded that most biases that were tested have no impact on mental health, income, or most socially desirable/destructive behaviors. To the extent there is an impact, it appears to be small. Future research should investigate whether other biases and fallacies than those studied may affect the dependent variables, and whether there is in fact a causal link between the biases on the one hand and the dependent variables on the other. This could best be accomplished through a longitudinal study, which would track a number of participants over a number of years to understand how their biases, income, mental health and habits change over time.

Chapter 2 found that while nudges (Libertarian paternalism; LP) can be temporarily effective at altering time preferences, the effect of a nudge tapers off if repeated. An autonomy-enhancing paternalist (AEP) intervention tested in the same experiment was found to be ineffective, and neither intervention proved to have a lasting effect among subjects who took the same experiment again one week later without the intervention present. The possibility that nudges taper off, and that nudging thus may be a less effective policymaking tool than previously believed, is something that is worth further investigation.

Chapter 3 found that transparent nudges can be just as effective as traditional, hidden nudges, provided that participants are paying full attention. Additionally, a different, stronger AEP intervention was also found to be effective, in particular among those participants who were paying full attention where it proved to be more effective than either the LP or TLP intervention. Further research is, however, necessary to determine whether this holds true in contexts other than intertemporal choice, and whether or not nudges other than the "default option" nudge also retain their efficiency when made transparent.

A key decision that all survey-based research must answer is which platform to use to host the surveys. The platform required for this research had to allow the inclusion of question skip logic, meaning participants could face a different set of questions depending on their answer to one question. This was vital to ensure that participants could be divided into treatment and control groups. Without question skip logic, all participants would have faced all the same tasks, making an experiment impossible. In addition, the platform had to be able to collect a large number of responses and to export all individual responses to Microsoft Excel, and ideally the platform would be able to do so at a reasonable cost. Finally, to avoid platform associated "teething" issues, the platform had to be well established.

SurveyMonkey fulfilled all of these criteria, and was used for the first two experiments, which were the foundations for Chapters 2 and 3. The third experiment was instead hosted on the QuestionPro platform. The change was motivated mainly by budgetary reasons, as QuestionPro was able to perform the necessary functions at a fraction of the cost of SurveyMonkey. QuestionPro, unlike SurveyMonkey, does not charge per response no matter the number of respondents[26]. Since one of the big advantages of online surveys is the ability to have a large sample size, it is important that having a large sample size not be cost-prohibitive.

Each experiment in the end did have a large sample size compared to typical lab experiments, in particular the latter two, but while the samples were large, very few participants came from outside the western world. In no experiment did enough participants reside in any country outside the OECD to be able to isolate and estimate regressions using only those participants, in the way that was done with participants from the USA (Chapter 2) and the western world (Chapter 3).

Two factors appear to have contributed to this. First, the surveys were advertised in English and Swedish, on sites and social media groups whose audience were mainly in the Anglosphere and Scandinavia. While anyone could, in theory, have seen or shared the posts advertising the surveys on social media, there is no doubt that this advertisement strategy was a contributing factor.

Secondly, the surveys were only available in English. While using translation software to make the surveys available in other languages, such as Spanish and French, was considered, a good enough translation for surveys in an academic research context could not be guaranteed. Thus, it was not pursued. If one question had been mistranslated and the meaning of the question changed or become unclear to the participant, the data would have become polluted.

Hiring a professional translator was also considered, but unfortunately this option was too expensive given the available research budget.

[26] SurveyMonkey offers a number of membership plans. I used the “Standard Monthly” plan which limits the number of responses to 1000 per month, with additional fees charged for additional responses.

Despite conducting the surveys in English, it was clear that some participants struggled with understanding some of the questions. This was mainly an issue in the Chapter 1 experiment, where participants were asked a number of sensitive, personal questions regarding, among other things, their annual income. A number of participants misunderstood the question and put down their monthly income or put down their income in a different currency than the currency they were being paid in. Data also showed that the frequency of obesity in the sample was far below that recorded in most countries in the western world, which is likely due to a combination of participants not wanting to admit to being obese and/or participants not knowing that they are obese.

In retrospect, conducting a pilot study as part of the first chapter may have at least partially resolved these issues as it may have indicated their presence and allowed the questions to be rephrased before beginning the final survey.

One important difference between the second and third chapters was the inclusion of a follow-up survey in the second chapter designed to test the permanency of the LP and AEP interventions. After having found that neither had any permanent effect on intertemporal choices, a follow-up survey was not included in the research design. In retrospect, this was a mistake, since the third chapter introduced a new intervention (TLP) which the second chapter did not test for and because the AEP intervention in the third chapter experiment was significantly stronger than the version used in the second chapter. It is conceivable that either of these interventions may have a long-term effect on behavior even after they are discontinued. Further research is necessary to establish whether or not this is the case.

Despite these issues, this thesis represents a step towards a greater understanding of nudging and its alternatives, as well as an understanding of how biases and fallacies impact consumers not just in specific situations, but in terms of mental health, income and commitment to socially desirable and destructive behaviors.