Preliminary remarks:

This review was written in May 2009, with slight revisions later in 2009 and 2010. In the meantime I became aware of two new publications that have important bearing on the contents of this review, but which I could not take into account without a radical revision of what I wrote two years ago:

Nicholas Zair, *The Reflexes of the Proto-Indo-European Laryngeals in Celtic*, PhD-thesis, Oxford 2010 is a thorough and exhaustive study of laryngeal reflexes in Celtic with many new and very interesting suggestions that naturally supersede much of what is written below.

Joseph F. Eska, 'In defence of Celtic /ɸ/', to appear in a *Festschrift* (prob. 2012), provides good additional arguments for interpreting <v> in the inscription of Prestino (LexLep: CO·48) as a means to write Proto-Celtic *φ, arguments that make my previous doubts obsolete.

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Graham R. Isaac, *Studies in Celtic Sound Changes and their Chronology*. Innsbrucker Beiträge zur Sprachwissenschaft Band 127, Innsbruck: Institut für Sprachen und Literaturen der Universität Innsbruck 2007. 128 pp. ISBN 978-3-85124-711-4.

This volume brings together five independent contributions by Graham R. Isaac on sound changes in Celtic, on their chronology, and on the methodology of setting up sound rules.[1] Nevertheless, the five articles, if one may say so, are thematically linked and thus warrant being combined as separate chapters in a single volume. The first four are devoted to aspects of the phonological history of Proto- or Common Celtic, the last chapter to that of Primitive Irish. The index (121–128) contains only such words as are actually attested. It would have been useful to include in it also a list of PIE roots and formations cited. A small erratum leaf inserted before the first page informs the reader that 10 must be added to the page numbers given in the index.

As for the main methodological aspect of the book, Isaac remarks that he adheres to a "general position of phonetic realism" (32 fn. 57) by which he means that reconstructions be phonetically real and not just phonemic abstractions. This insistence recurs throughout the book like a leitmotif. I think that a major misunderstanding lurks behind this position. Its subliminal message is that the average historical linguist tends to formulate sound changes without an eye to phonetic factors and realism. But such an opinion would be grossly incorrect. Every conditioned sound change, that is to say, every sound change that is not a soundshift by which all instances of a phoneme undergo an absolutely identical development, is by implication phonetically sensitive. Its effects are governed by the suprasegmental environment and they are therefore phonetic (cp. the more elaborate thoughts in Kümmel 2007: 16–20). At the same time, it is a truism that because of the lack of technical means we will never be in a position to say anything definite about the exact phonetics of historical stages of languages. We may be able to talk about "proximate phonetics" (a term suggested to me by Joseph Eska, pers. comm.), but for practical purposes it usually suffices to talk about phonology. As long as phonetic developments have no repercussions on phonology (e.g. on the relative distributions of phonemes, on the structural makeup of the phonological system), it does not make much sense to invoke a sound change. The sound changes that we talk about in historical linguistics could all be stated also in phonetic terms. I am not sure, however, if much would be gained by such a procedure.

In any case, against the background of this insistence on phonetic realism, a word must be said about the representation of PIE reconstructions by the author and by the reviewer. It is odd to see that in his PIE reconstructions Isaac does not distinguish phonologically between syllabic and non-syllabic resonants and glides and writes unsyllabified forms instead, in accordance with the practice in the so-called 'Leiden school' of Indo-European studies. This is even more odd in view of the fact that Isaac draws on Schindler's syllabification rules (Schindler 1977) to criticise other people's theories (26). Schindler had a decidedly dismissive attitude towards Leiden-style reconstructions. For him, syllabic and non-syllabic resonants and glides were not merely allophones of each other in PIE, but they were – at least marginally – phonemic, e.g. the acc. sg. of acrostatic and proterokinetic *i*- and *u*-stems end in *-*im* and *-*um*, not †-*im̥* and †-*um̥* as predicted by Schindler's rule of regressive syllabification (see Kümmel 2007: 17–19 for a more explicit discussion of this problem).

The inadequacy of Isaac's notation becomes glaring when he talks about the different results of (in his notation) PIE *dʰgʰomios > *gʰdʰonyos 'belonging to the earth' and Celtic *omyo- 'bronze' (78 fn. 116). Notwithstanding the explanation of the *n* in Greek χθόνιος and Celtic *γdonịos, setting up Celtic *omyo- reveals that Isaac believes in a single adjectival morpheme

*-i̯o-. In fact, Proto-Indo-European had different morphemes *-i̯o- and *-ii̯o-, the distribution of which was not predicated by Sievers' Law (Balles 1997; 1999: 5–7). The Celtic word for 'bronze' was trisyllabic to begin with: it either had the latter suffix (*omii̯os) or it can be reconstructed as a matter adjective with the appropriate suffix (*omei̯os, Schrijver 1995: 287). In any case, OIr. *umae*, W *efydd* 'bronze' are not relevant to the question of a hypothetical post-PIE rule *-mi̯- > *-ni̯-.

The practice followed in this review will be to use the non-syllabified system of notation and the characters *w* and *y* when the reviewed author is cited, but to write explicitly syllabified PIE reconstructions and to use the traditional characters *u̯* and *i̯*, long-established in the discipline, wherever I talk about reconstructions.

Chapter 1 (11–20) bears the title "Loss of PIE *p* in Celtic and Related Matters". The chapter consists of two separate sections. The first one (11–14) examines the relevance of Lep. *uvamokozis* (Prestino) as proof for the survival of *φ < PIE *p* into the historic period, as argued by Eska 1998. I myself had been very optimistic about the import of the letter <v> in the Prestino inscription (Stifter 2002–3: 239 fn. 1), but I have become much more cautious in the meantime. The reasons, however, differ somewhat from those of Isaac.

Isaac argues that none of Eska's arguments for the interpretation of Lep. <v> 'digamma' = /φ/ is valid. This rare Lepontic sign was, like the writing system as a whole, taken over from a variant of the Etruscan script. In Etruscan, however, this sign stood for /w/, not for the bilabial fricative. If the Lepontians had wanted to write the phoneme /φ/, Isaac claims, "[…] the Etruscan alphabet did provide a model for writing /φ/, and it was not digamma, but rather, in the earliest period, the digraph digamma-eta or reversed, <vh> or <hv>" (12–13). This statement may come as a surprise to scholars of Etruscan, a language for which the presence in the phonemic system of a voiceless bilabial fricative is not part of the received wisdom. Instead, the phoneme written alternatively as <vh>, <hv>, <f> has been described by Steinbauer (1999: 27) "als labiodentaler Reibelaut". In a similar manner, Rix (2004: 947) speaks of a "labiodental". Stuart-Smith (2004: 36–37) also does not speak of a phoneme /φ/ in Etruscan. Instead she rather uncommittingly states that <vh> and <hv> represented "some type of labial fricative" (see also op. cit. 129). Only Bonfante & Bonfante (2008: 78) think of a "bilabial" sound, but in a manner that is more confusing than clarifying as to what they actually have in mind.[2] The upshot of all this is that in view of the probable absence in Etruscan of a phoneme /φ/, there is on palaeographic grounds *a priori* no objection to the proposal that <v> could have been utilised by the Lepontians for *φ, even less so when we consider the amount of experimentation going on in tailoring the Etruscan alphabet to the phonological requirements of the Lepontic language.

Isaac (14) concludes that the spelling <uvamokozis> is either an attempt to write "[uwamogostis] = /u.amogostis/", or that <uv> is a digraph representing *u̯, viz. /wamogostis/. As for the first of these options, Isaac (12) suggests – as one possible explanation – that the order of developments may have been (using the example of OIr. *fo* 'under', etc.): PIE *u̯po > *u̯φo > Common Celtic *u.o = *u̯u̯o = [uwo], becoming monosyllabic *u̯o independently in the individual Celtic languages only. This is wrongly conceived. Notwithstanding the possibility that an intermediate form /u.amo-/ with hiatus may have existed at some stage, it cannot have been pronounced phonetically as [uwamo-] in Proto-Celtic. Proto-Celtic or Early Celtic did actually possess a phonetic (because phonological) sequence [uwV] = *u̯u̯V. Examples of this are *du̯u̯o '2' < PIE *d(u)u̯ó(h₁) (Cowgill 1985), *dru̯u̯id- 'druid' < PIE *dru- 'truly (?)' + *u̯id- 'knowing', or *i̯u̯u̯anko-[3] 'young' < PIE *h₂i̯uHn̥kó-. The last word is especially important

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[2] "The Etruscans had a sound *f* (a bilabial, voiceless fricative, pronounced approximately as in English labiodentals: *find*, *soft*, *stuff*) for which the Greeks had no sign."

[3] As a side note, I regard Gaul. *iouinco-* 'young', a frequent element in the onomastic record, as reflecting PC *i̯u̯u̯enko- with an unexpected and not inherited full-grade *e in the suffix that must have been introduced from a related paradigm with full-

because in it *u̯ = [w] arose as a true hiatus filler after the loss of the intervocalic laryngeal. Patently, in these forms *uu̯V did not become **u̯V as would be expected by the order of developments proposed by Isaac as a possibility for *u.o (12), but the Insular Celtic languages show that the sequence essentially remained disyllabic. On the other hand, it is unthinkable that a hypothetical phonetic [uwo] as in Isaac's *u.o should be treated differently from phonological /uwo/ as in real PC *du̯u̯o. It must be concluded that the two sequences remained distinct in Celtic. There are only two explanations for this: either a segmental trace of *p like, for example, *φ was retained long enough into Celtic in words like PIE *upo, or the phonological hiatus in */u.o/ was realised as a phonetic hiatus [u.o] before it was collapsed to monosyllabic [wo]. A phonetic pronunciation [uwo] in Common Celtic is excluded.

There remains thus the possibility that the digraph <uv> represents *u̯ in initial position (against <u> = *u̯ word-internally, e.g. *ariuonepos* and *uvltiauiopos*). In support of this, Isaac cites a similar employment of the digraph, although word-internally, in the Etruscan name <auvileś> beside more usual <avileś>. In fact, there is a more apposite example for such a spelling closer to home: on an Etruscan inscription from Mantua from c. 300 B.C. the personal name *eluveitie* is found (Vitali & Kaenel 2000). In all likelihood this is the Celtic ethnonym *elu̯ei̯tii̯os* (cf. Lat. *Heluetius*) used as a personal name, with the Celtic vocative reinterpreted as the inflectional stem (a frequent phenomenon in language contact). Here, too, it looks as if the Celtic labial glide *u̯ had been represented graphically by <uv>, unless it is assumed that the word, which consists of reflexes of the two elements *pelu- and *pei̯tu-, was pronounced as /φeluφei̯tii̯os/ as late as the 4th c. B.C. In that highly unlikely case <uv> would again represent Celtic *uφ, but the lack of a letter corresponding to the initial *φ would be staggering. In summary I think that Isaac is right in thinking that *uvamokozis* is to be read as /wamogotˢis/, but some of the assumptions and hypotheses that lead him to the conclusion are incorrect.

The second section of chapter 1 (15–20) is explicitly devoted to the aim of refuting Schumacher's (2004: 509–510) refutation of Thurneysen's explanation of OIr. ·*ír* 'granted' (GOI 435). Thurneysen had traced back *ro·ír* either to PIE *peporh₃e or *peperh₃e (in modern notation), but had himself noticed the formal problems associated with either solution. Schumacher explicitly argues that neither PIE *epo nor *epe could have become Common Celtic *ī, but should have surfaced either as a hiatus sequence *i.o or as *é in Irish (Schumacher's eventual solution for ·*ír* is of no further import here). It is not entirely clear if Isaac wants to reinstate Thurneysen's morphological analysis of ·*ír*. What he sets out to do, however, is to demonstrate that Schumacher's ideas about the regular development of PIE *epo and *epe are wrong. Even at the risk of perpetuating an endless loop of refutations, I think that Isaac's refutation itself requires several remarks.

Schumacher's examples for PIE *epe ≯ Celt. *ī are *tepent- 'hot' > OIr. *té*, pl. *téit* and *tepesmo-* > MW *twym* 'warm'. In a short paragraph, Isaac rejects these explanations (15). Leaving aside OIr. *té*, *téit*, which indeed is ambiguous, as Isaac argues, a closer look must be taken at *twym*. For it, Isaac proposes a "derivation as < *tēmo- […] < *teχmo- < *teχsmo- < *tep-s-mo-, with *tep-s- as in OI *timme* 'heat' < *teχmiyā < *teχsmyā < *tep-s-m-ieh₂- […]" (15). Both derivations are incorrect. In the Common Celtic sequence *-χsm- (with *χ continuing any tectal or labial obstruent),[4] first the velar fricative was lost[5] before the sibilant itself was lost with compensatory gemination of the following nasal, i.e. *-χsm- > *-sm- > *-mm-.

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grade suffix. Most apposite is a hysterokinetic paradigm *i̯uu̯en- < *h₂i̯uh₃en-. This explanation entails that *hₓ in the Hoffmann-suffix is *h₁ (see also NIL 285 note 31).

[4] Assuming, of course, that *-Ksm- and *-Psm- developed alike. Until the opposite has been proven I operate with an identical behaviour of the two.

[5] Although ultimately irrelevant for the eventual outcome, it seems to me more probable that the fricative was dropped in the coda of a syllable before a following *s* rather than that it was absorbed by the following *s*. If one can be guided by spellings in Gaulish like *-reixs* and *siaxsiou* the fricative had not yet been absorbed by a simple *s* by that time. Admittedly, however, the guttural could have been reinstated secondarily in the latter words from paradigmatically related forms.

That the sequence of changes must have been like this and not like in Isaac's proposal is demonstrated by several verbal abstracts with the suffix *-smen-*, all without lengthening of the preceding vowel in British: OIr. *broimm*, MW *bram* 'fart' < *braχsman* (beside OIr. *braigid* 'to fart') (Stüber 1998: 62), OIr. *loimm*, MW *llymaid* 'draught, sip' < *luχsman* (beside W *llyncaf* 'to swallow') (Stüber 1998: 66; differently Schumacher 2004: 526–527: < *φlusman*), and, most appositely since it involves the vowel *e*, MIr. *seimm*, W *hemm* 'rivet' < *seχsman* from the root *seg^h* 'to hold' (Stüber 1998: 66–67; differently Schumacher 2004: 563: root *seg* 'to sow, plant'). This means that *pace* Isaac the long vowel in *twym* cannot be due to compensatory lengthening of a preform **teχm-* < **teχsm-*. Schumacher's explanation of *twym* has therefore not been disproven. It also follows from this demonstration that OIr. *timme* and W *twym* cannot continue a common preform, but must rather reflect two variant formations, *tepsmii̯eh₂* and *tepesmo-* respectively.

Schumacher's argument against PIE **epo* > Celt. **ī* is the OIr. gen. sg. *niad* 'nephew' (Ogam NIOTTA) which he traces back to PIE **nepotos* via the disyllabic intermediate stages **ni.oθah* < **ne.otos*. Isaac challenges this derivation on phonological and morphological grounds. His first – and valid – point is that the raising of **e* > **i* in hiatus before a back vowel *o/a* that brought about the surface forms of Old Irish must have happened shortly before the Old Irish period proper (16). He goes on to suggest that there is no cogent reason to reckon with a (much) earlier development of the same type with the same results, presumably under the tacit assumption that the meandering development of Early Common Celtic **e.o* > Common Celtic **i.o* > Primitive Irish **e.o* > Old Irish /i.ə/ implied by Schumacher is uneconomical and thereby unnecessary. But is such reckoning valid? The course of developments would be uneconomical only if there really existed no evidence to suggest otherwise. But I think Isaac is wrong here. In this respect, I do not think of the ending of the gen. sg. of *s*-stems and of the precursor of the OIr. anaphoric pronoun *suide* according to Schrijver's (1995: 387) explanation, both of which Isaac uses to deconstruct Schumacher's position. As long as it is unclear how PC unstressed **esV* developed in general in the prehistory of Insular Celtic and Irish (see Griffith forthc. for a new contribution to the old controversy), forms containing it cannot be used to demonstrate anything. There is, however, the special case of OIr. *siur*, acc. *sieir* 'sister' with its cognates Gaul. *suiorebe* and W *chwaer*, pl. *chwiorydd* < *su̯e-sor-*. On superficial inspection, all three of them display an <i>, and accordingly one may be inclined to regard this word as evidence for an early, perhaps Common Celtic raising of **e* > **i* in hiatus. With regard to this word, however, Isaac dismisses the relevance of the sister languages with reference to the fact that the *i* of the OIr. forms can be explained by Irish rules alone. While this position is methodically unassailable on the basis of received wisdom, the whole problem is a good illustration of how scholarly progress in a distant corner may have repercussions somewhere else. As I will argue in a forthcoming paper (Stifter forthc. b), there is the likelihood that, in a pan-Celtic development (perhaps to the exclusion of Celtiberian), **s* in the onset of second syllables was lost by dissimilation when the initial syllable began with an **s*, too. For the word at hand this means PC *su̯esor-* > *su̯e.or-*. Since all three branches for which there is evidence show an <i> in a hiatus before **o* or **a*, it is natural to assume that the raising of *su̯e.or-* > *su̯i.or-* should be ascribed to common inheritance. If this proposition is correct, the change **eso* > **i.o* in the word for 'sister' would have nothing to do with accentual position (thereby invalidating Isaac's objection at the bottom of p. 17). And if this proposition is accepted, the number of relevant examples of raising of **e* > **i* in hiatus can be increased by the plausible Gaulish case of *sioxti* < *se.oχt°* < *se-soC-t-* (see Schumacher 2004: 745–747).

Finally and most importantly for the problem under scrutiny here, it follows from what has been said above that the resulting form *su̯e.or-* (or *su̯i.or-*) would have had a very similar structure to *ne.ot-* (or *ni.ot-*) < *neφot-* for a very considerable length of time in the prehistory of Irish, and it will not do to simply dismiss it as irrelevant. It must be stressed that the

primary aim of this section of Isaac's book lies in the phonological objective of demonstrating that, contrary to Schumacher's claim that *epo > *i.o > OIr. <ia>, *epo rather developed directly into *ī. In view of the fact that Schumacher's derivation of OIr. *niad* directly from *nepotos could not be disproven on phonological grounds, it follows that there is, at the same time, no positive support for *epo > *ī. This is not to suggest that Isaac may not be right after all with his scepticism concerning the derivation of *niad*. But the strength of his considerations does not lie on the phonological, but on the morphological side.

Paragraph 3 (18–19) commences with the bold claim that if the stem *nepŏt- were actually reflected in Celtic, Celtic would be unique in IE in preserving it, whereas all other languages had levelled the original ablaut distinction between *nepōt- and *nepŏt- in favour of the long-vowel allomorph. While this may indeed be the case in many languages, nothing of that sort can be said about Old Indic acc. sg. *nápātam*, nom. pl. *nápātas*. The long *ā* could just as well continue *ŏ under the effect of Brugmann's Law as it could go back to analogically introduced *ō. Greek νέποδες 'descendants' (for δ < *t by contact assimilation with voiced endings see Schaffner 2005: 262–263) isn't even mentioned by Isaac. But even if it were true that no IE language retained the ŏ-grade this fact would only be of oblique relevance for Celtic. A suffix with ablauting *ō/ŏ/Ø has to be postulated for the proto-paradigm in any case. Whatever other languages did to this three-grade alternation, Celtic could have gone its own way. In other terms for close relations the Celtic languages have maintained the archaic suffixal ablaut, e.g. Gaul. nom. sg. *matir* < PIE *meh₂tēr, acc. sg. *materem* < *meh₂term̥, dat. pl. *matrebo* < *meh₂tr̥bʰos (uel sim.); OIr. nom. sg. *athair* ← *aithir < PC *φatīr < PIE *ph₂tēr, acc. sg. *athair* < *φateram < *ph₂term̥, nom. pl. *aithir* < *φatres ← *ph₂teres (McCone 1994: 280–283). The proof for the word 'sister' is a bit more long-winded: OIr. nom. sg. *siur* continues lengthened grade PC *su̯esūr, the W plural *chwiorydd* and Gaul. *suiorebe*[6] continue the full grade PC *su̯esor-, which may also be reflected by OIr. acc. *sieir*, and the OIr. gen. sg. *sethar* is in all likelihood a substitute for whatever the zero grade allomorph PC *su̯esr- would have yielded (McCone 1994: 283, Kim 2008: 160–161).

Furthermore, the ablaut ō/ŏ was retained in other Celtic words with the suffix *-ot-. In at least one word this ablaut alternation is directly attested in British and, with slight changes, in OIr. W *llyg* 'dormouse', pl. *llygod* 'mice' directly continues sg. *lukūt-s < *luk-ōt-s, vs. pl. *lukot-es. The OIr. cognate displays the ŏ-grade form in all cases outside the nom. sg., e.g. gen. sg. *lochad*, whereas in the nom. sg. *luch* < *lukut-s the suffixal vowel was secondarily shortened from *lukūt-s, probably under analogy from the other cases of the paradigm and in order to create a contrast to dental stems referring to human beings where the long-vowel allomorph was generalised (see below; Stifter forthc. a). If 'mouse' retained the ablaut alternation ō/o in Proto-Celtic and in Proto-Insular Celtic (if one accepts this notion), it is not presumptuous to assume the same for 'nephew'. There is, therefore, *pace* Isaac nothing inherently implausible in an ablauting paradigm *nepōt-/nepot- in Celtic and in the prehistory of Irish. It is a different question, though, whether the attested forms do actually represent those ablaut grades.

Isaac (19) assumes that Celtic, like, Latin, generalised the stem *nepōt- in the paradigm. His order of developments is approximately the following: nom. sg. *nepōts, gen. sg. *nepōtos > *ne.ūts, *ne.ātos → *ne.ūts, *ne.ūtos (by generalisation of the allomorph of the nominative). The attested Irish forms are all derivable from such a Celtic paradigm:

"*ne.ūtos >	*ne.ŭθah >	*ni.uθah >	*ni.oθah >	NIOTTA >	OI *niad*
	shortening	raising	lowering	= *ni.oθa" (19)	

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[6] The instr. (or dat.) pl. *suiorebe*, in which zero grade would be expected, points to the reduction of the original 3-grade ablaut ō/o/Ø to a 2-grade system ō/o, but this may be restricted to Gaulish.

It should be noted that the outcome would be the same if the input were *ni.ūtos*. Incidentally, Isaac does not mention the essential point that speaks most strongly for this explanation of NIOTTA, and which in fact clinches the whole matter in its favour (as Stefan Schumacher points out to me): the only attestation of this word in Ogam[7] is on the inscription CIIC 252 from Gurrane, Co. Kerry. The inscription reads:

DUMELI MAQI GLASICONAS NIOTTA COBRANOR[IGAS]

As -CONAS < PC *-kunos* shows, this text was written *after* the lowering of *u* and *i* to *o* and *e*. Since *crete* 'they who buy' < *k'r'e.ad'e* shows that prior to syncope old *i* before a back vowel must have been lowered to *e* even in hiatus, it follows that if *ni.oθ-* had been the pre-lowering input, the post-lowering, pre-apocope, pre-syncope output (which is the time when the Ogam stone in Gurrane was erected) should have been †NEOTA. Therefore it follows that NIOTTA at this particular point of time can only reflect a preform with *ū* in the second syllable. A *ū*, generalised from the stem allomorph of the nominative singular – congruent to Isaac's proposal – is the only reasonable guess. The quality of the input form of the vowel in the first syllable cannot be determined; it could have been *e* or *i*.

Despite Isaac's correct assessment of Ogam NIOTTA, I am not convinced that the substitution of the 'weak' stems *nepot-* and *nept-* by 'strong' *nepōt-* (in anachronistic notation) must have taken place at the period of the Celtic proto-language. The British forms can be accounted for by the ablauting paradigm sg. *ne.ūts*, pl. *ni.otes* in the traditional manner. The Welsh singular *nei* continues the inherited form directly (18), but its plural MW *neieint* is manifestly analogical and therefore irrelevant to the present discussion. The Breton plural *nied* instead of *niod*, expected from *ni.otes*, can be easily explained either by substitution of the plural suffix *-od* < *-otes* (preserved in Breton only in *logod* 'mice' < *lukotes*) by productive *-ed* < *-etes* (Trépos 1957: 41–42) or by secondary umlaut of *-od* > *-ed*, as if the plural had been **ni.otī*; the singular *ni* can have been abstracted from the plural *nied*.

Consequently, the analogically extended stem *ne.ūt-* or *ni.ūt-* (with *i* from *ni.ot-*) is only required for Irish. This ties in perfectly with a morphological rule that I have postulated for Irish (Stifter forthc. a.). I claim that sometime in the Proto- or Primitive Irish period there was a tendency to morphologically re-characterise dental stems. Nouns that refer to human beings generalised the long vowel suffix allomorph, originally at home in the nominative singular. This can be best seen in OIr. *fili*, *filed* (not †*feiled*) 'poet' < *u̯elīt-*, and *briugu*, *briugad* (not †*bregad*) 'hospitaller' < *brigūt-*. On the other hand, words for non-humans introduced short vowels even in the nominative singular (see 'mouse' above), in order to create a maximum morphological contrast between higher and lower animate nouns. Because of the reconstructable paradigm of 'mouse' in British, levelling towards one stem allomorph in the dental stems can only be postulated for the prehistory of Irish. I missed the significance of *nia* 'nephew' in my original description of this process, but this word is entirely in accordance with the hypothesis. This process offers a more cautious (because restricted to a single language) alternative to Isaac's suggestion of a wholesale generalisation of *nepōt-* already in Pre- or Proto-Celtic.

Isaac's claim that the regular nom. sg. *ni.ūh* was remodelled to *ni.ōh* in Proto-Irish (19) is not really necessary and would in fact be difficult to motivate. Isaac ascribes the *ō* to the influence of the oblique stem *ni.ŏθ-* (apparently after lowering). But on purely phonological grounds, the oblique stem must have shown an alternation between lowered *o* and *u*: *o* in gen. sg. *ni.oθah*, gen. pl. *ni.oθan*, dat. pl. *ni.oθaβih*, acc. pl. *ni.oθāh*, but *u* in dat. sg.

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[7] There is a second token of NIOTT in the Ogam corpus, but this is deceptive and of no evidential value for the present question. CIIC 202 (Coolmagort, Co. Kerry) NIOTTVRECC MAQI [….]GNI actually contains the OIr. name *Nad Fraích* and is a hypercorrect spelling for what is otherwise preserved in Ogam as NETTA-VRECC (CIIC 26) and NETA-VROQI (CIIC 271). CIIC 202 must belong to a very late period when gen. sg. *níad* 'warrior, hero' < *nēθah* and *niad* 'nephew' < *ni.oθah* had already fallen together.

ni.uθ, acc. sg. *ni.uθen*, nom. pl. *ni.uθeh*. It is a matter of personal choice which of the two allomorphs would have had the stronger influence on the nominative singular. Going by the attestations collected in DIL, nom. sg. *nia* (and variant *niae*) is attested late enough, namely in *Cormac's Glossary*, to allow for the regular OIr. and early MIr. development *-u# > *-ə# = <a> to have taken place already.

We can now proceed to Isaac's conclusion of the chapter in which he states that "[i]f there are counterexamples to *peporh₃e* or *peperh₃e* > *ēre* > Celtic *īre* > OI ·*ír*, they are not the forms cited by Schumacher (2004: 509–10)". I consider it safe, on the basis of my foregoing review, to conclude that these claims have not been proven. To the contrary, Schumacher's explanation of W *twym* 'warm' < *tepesmo-* remains plausible, and it has not been possible to demonstrate that the stem variant *nepot-* must have been tracelessly levelled away in favour of *nepōt-* already by the Proto-Celtic period.

On a more general note, the underlying objective of the two sections of chapter 1 manifestly is to defend that "[…] intervocalic PIE *p* was […], as usually thought, completely lost in Common Celtic […]", a theme that will become prominent again in chapter 3 of the book, and that is implicit already in the wording of the very first sentence of the chapter: "Some recent work has challenged certain accepted views about the outcome of PIE *p* in Common Celtic […]" (11). One may actually wonder what the current 'accepted view' is. In view of the fact that in a number of publications from the past years, whose authors explicitly operate with Proto-Celtic reconstructions (Schrijver 1995, Schumacher 2004, Eska 2004, Kümmel 2007, Matasović 2008), the phoneme /φ/ as the Common Celtic reflex of PIE *p* is accepted as a practical tool for phonological and morphological reconstructions and as a phonological fact, the question can be raised whether the modern received wisdom has not rather accepted that *p* was not completely lost in intervocalic position.[8]

I want to add a few arguments that a segmental reflex of *p*, be it /φ/, or /h/, or hiatus as a quasi-segment, was retained in Celtic languages for a very long time.[9] With the derivation of W *twym* < *tepesmo-* having been defended, the proposition of an early contraction of *epe* > *e.e* > *ē* even before inherited *ē* was raised to *ī* has become superfluous, as has the counterintuitive idea that *epo* > *e.o* contracted to *ē* at the same early period, whereas *e.ū* retained its hiatus.[10] Where other vowels are involved, the evidence for an early lack of contraction is even more apparent. For the sake of simplicity I will represent in the following discussion the reflex of PIE *p* by hiatus, without giving priority to any of the other possible reflexes. In the cases of *kapero-* 'goat' and *sapero-* 'wise person', the Welsh reflexes *caeriwrch* 'roebuck' and *saer* 'artisan'[11] keep the two vowels separate. This is to say that the contraction to the synchronic Welsh diphthong /ai/ occurred at a comparatively late date, after the original diphthong *ai̯* had been monophthongised to *ε*, which in turn yielded W *oe*, Bret. *oa*. Likewise the unusual paradigm of OIr. nom. sg. *cauru*, gen. *cáerach* 'sheep' can be best explained if the former is derived from *ka.erūχs* < *kapero-h₃kʷ-s* (with *u*-infection in the second syllable effected by the final *ū*), the latter from *ka.erāχah* < *kapero-h₃kʷ-os* (without *u*-infection). Whereas in these instances there is no positive piece of evidence that PIE *p* was reflected by anything else than hiatus, there is, as Stefan Schumacher reminds me, one context where one is almost forced to reckon with a bilabial fricative reflex of *p* up to the time when the Insular Celtic languages developed their systems of initial mutations. This context is initial *sp-*. The divergent outcomes of it in the individual languages (OIr. unlenited /s/,

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[8] Other scholars' publications, like Irslinger 2002 or NIL, do not operate with Proto-Celtic reconstructions and thereby avoid taking a stance on this matter.

[9] It should be noted here that the explanation of OIr. *sith-*, W *hyd* with laryngeal loss from *seh₁ti-* given in chapter 2 of the book actually implies that Isaac believes that *p* was completely lost earlier in Celtic than the laryngeals!

[10] It is nowhere made explicit in chapter 1 why it should be likely at all that *e.o* < *epo* contracted to *ē* at an extremely early date, whereas *e.ū* < *epō* remained disyllabic up to the Old Irish period.

[11] Unless it is a loan from Irish *sáer*.

lenited /f/ against W /f/, e.g. OIr. *seir*, dual *di pherid*, W *ffêr* < *speret*- 'heel') testify to a sound that was not [w] (in which case W *chw*- would be expected), nor [b] (presumably **sb- > *zb- > *ðb-* could be expected), nor [p] (in which case it should have merged in Welsh with the reflex of **sk^u*). By logical exclusion, [φ], [β] and [f] remain as reasonable possibilities. Of these, [φ] is the typologically expected product of weakening of **p*; [f] continues **φ* with articulatory assimilation to the preceding **s*; [β] could be best explained by identification with the marginal allophone **β* that arose by voicing assimilation of **φ* before **r* and **l*. For all of these, the development **s_ > s* is quite uncontroversial in Old Irish in unlenited position (cp. **#st* and **#su̯ >* OIr. *s*). Likewise for all three of them it is not implausible for **#h_* to have yielded *f* in lenited position in both Irish and British.

One minor remark on footnote 28 (19): OIr. dat. sg. *ciunn* 'head' does not directly continue **k^{u}innū < *k^{u}ennū*, but the intermediate stage **k^{u}innū* regularly resulted in *cunn*, the basis of the split paradigm *conn* 'leader, chief'. The dat. *ciunn* of nom. *cenn* is analogical after the more frequent o-stem pattern exemplified by *fer*, *fiur* 'man'.

Chapter 2 "A Rule of Laryngeals in Celtic" (21–59) is the longest and most complex in this book. Because of its complexity, it is not possible to discuss and evaluate all minutiae in the same manner as in the first chapter. Books could be written about the laryngeal problems of Celtic. Isaac sets out to discover nothing less than a "regular sound-law" (21) that will account for the divergent outcomes in Celtic of PIE **CUHC* and **CR̥HC*. According to received wisdom these "sometimes turn out as **CŪT* and **CR̄aT* respectively, and sometimes as **CŬT* and **CRăT*". Isaac prefers to rephrase the question as why "the loss of the laryngeal […] is] accompanied by compensatory lengthening in certain circumstances […], but not in other circumstances […]" (21, 25). Unlike most scholars who have contributed to the subject, Isaac assumes "that **CUHT > *CŬT* and **CRHT > *CRăT* are the same phenomenon, with the same explanation" (26). Isaac presents past explanations of the problem very briefly, so briefly in fact that, for people unfamiliar with the scholarly discourse and without access to the original publications, his discussion must border on obscurity. He then quickly moves on to the presentation of the relevant material (27–29). The basis of his investigation are "formations with dental suffixes, usually deverbal", taken from Irslinger 2002, "with a handful of additions" (26). Information about the roots and their laryngeals is also taken from LIV, but from the first edition from 1998, not from the much revised second edition from 2001. A number of forms are excluded from the discussion because they are doubtful or ambiguous in certain respects (27). Of these, at least OIr. *claidid*, W *claddu* 'to dig', W *clawdd* 'dyke, ditch, fence' have been excluded – with reference to Schrijver 1995: 171 – wrongly (see Schumacher 2004: 412; Schumacher's objections have been accepted by Schrijver in the meantime, pers. comm. Stefan Schumacher). As shall be shown below, a considerable number of other relevant forms are not mentioned at all. The list of forms is immediately followed by a preliminary hypothesis about the cause for the divergent treatments of the inputs. Isaac's conjecture is that the development of the laryngeals is affected by "suprasegmental roundness spread over a stretch of phonetic material" (33), or, very simplified, **h$_3$*, and also **h$_2$* (in the guise of a labialised allophone [ħ^w]) tend to disappear by dissimilation (i.e. are lost without lengthening of a preceding short-vowel) in certain labiality-imbued contexts. He then goes on to discuss, exemplify and refine this theory.

There is nothing inherently implausible in the idea, apart from the fact that it fails to account for a great number of forms that *a priori* would be expected to fall under the scope of the rule, i.e. forms like OIr. *dán* 'gift' < **dóh$_3$nu*-, *áth* 'ford' < **i̯áh$_2$tu*-, *scáth* 'shadow, reflection' < **skéh$_{2/3}$to/u*- (cp. Irslinger 2002: 125–127), *súil* 'eye' < **suh$_2$li*-, etc. To be fair, Isaac does

provide explanations why he thinks some of these words would not be affected by his rule,[12] although not all of these words are specifically mentioned. What follows is a lengthy discussion that heaps specification after specification upon the original rule, until a very complex formulation is reached (43–44) that is able to account for all examples within the material collection from which Isaac started his investigation. In the following, I want to concentrate on various cornerstones of the chapter.

As a working hypothesis, Isaac replaces the usual algebraic notation of the laryngeals by a phonetic interpretation (30):

"*h_1 = [h]   *h_2 = [ħ]   *h_3 = [ʕʷ]"

an interpretation that is fairly reasonable (see Kümmel 329–336, esp. 336, for the 'best guesses' as to the phonetics of the laryngeals). The question must be asked, though, whether it is necessary, or even advisable, to operate with phonetic interpretations at all. Isaac justifies his procedure by saying that "*only through some consistent interpretation of the phonetics of the 'laryngeals'* can a solution to the problem […] be found" (31; emphasis in the original). The logic of the argument escapes me. If there was regularity behind the distribution of forms with or without long-vowel reflexes of CUHC or CR̥HC, this regularity would appear under any type of investigation, irrespective of an algebraic or phonetic notation of the laryngeals. It would seem to me a 'cleaner' methodology to attempt a solution by a purely algebraic approach first, and then to try to find a phonetic explanation for it, rather than to prejudge the direction the solution will have to take already by the choice of the analytic tool. An example for the arbitrariness of this kind of phonetic approach is the following: after having established that [ʕʷ] = *h_3 is lost in the vicinity of roundedness, Isaac draws attention to the fact that OIr. *mrath*, W *brad* 'betrayal' < *$mr̥h_2tó$- displays the same short-vowel reflex, although otherwise the rules for loss of *h_2 are not the same as those for *h_3. His solution for this word is that because of the labiality of the initial *m and the *o of the final syllable, *h_2 was allophonically articulated as [ħʷ] in this particular environment (33).[13] By using an exclusively phonetic interpretation of the developments, Isaac escapes the necessity to set up a rule *h_2 > *h_3 under very specific conditions, a rule that would otherwise be very hard to support and to justify. In this context it may be remarked that in the case of OIr. *críth* 'buying' < *$k^urih_2tú$- (if it is a *u-stem, as per Irslinger 2002: 93), a form which exhibits a comparable amount of labiality, no such allophonic rounding is assumed as for *$mr̥h_2tó$- (36). I fail to see why. Reference to the intervening *i is no phonetic explanation for the different behaviour (for instance, [i] could be allophonically rounded in the environment and could thus pass on the labiality of the *k^u), it is just a condition formulated *ad formam*.

It is ironic, though, that the starting point for Isaac's investigation belongs neither to CUHC nor to CR̥HC, and does not involve any kind of labial sound either. It is an instance of the structure CEHC – at least according to his own preferred etymology –, viz. OIr. *sith-* 'long', W *hyd* 'length', allegedly < *$seh_1tí$- (39–40). In order to integrate this word into his theory, Isaac introduces another lip feature, spreadness, and postulates that *h_1 = [h] possessed phonetic spreadness = [h]. Conceding that I am not a phonetician, I noticed several incongruities in the argument. The first one relates to the phonetic nature of [h]. Isaac writes: "So, while /h/ may not have been characterised phonologically by distinctive spreadness, it may have been articulated thus [h]" (38). One thing that is a standard assumption in phonetics is that the glottal fricative [h] has no phonetic features of its own, but can acquire features by co-articulation

[12] According to Isaac's rule, the first three of the just mentioned forms would not be affected because the rule does not operate on mid vowels unless the laryngeal is immediately followed by another labial element (40). This is one of the many constraints on the rule, constraints whose phonetic rationale is not always easy to grasp.
[13] I fail to see why *ah_2 in *$skuh_2$-$táh_2$- (> OIr. *scoth* 'flower') should be regarded as rounded in order to facilitate [h] > [hʷ] (44).

from neighbouring sounds. This means that the secondary articulation 'spreadness' of [h], if we concede it for argument's sake, can only be the result of spread from a neighbouring vowel, the immediately adjacent *e suggesting itself for the purpose. Spreadness, or labial spreading, is a secondary articulation. It is a non-active, only concomitant feature that – as I understand it – primarily refers to vowels (but I acknowledge that it may extend allophonically to other sounds). For example, labial spreading is a phonetic feature concomitant to non-rounded vowels in languages that oppose rounded vs. non-rounded front vowels, like German or French [ø] (rounded) vs. [e] (non-rounded, spread). In languages that have no such contrast – the majority of the world's languages – the feature spreadness is of no relevance. So we are forced to assume that in PIE or post-PIE spreadness was specified in the vowel *e although there was no contrast to a rounded front vowel. What is most important for the present argument, however, is that spreadness is the opposite of lip rounding. Isaac then goes on to say that /sehtí-/ = [sehtí-] was equivalent to [seetí-]. This is indeed plausible; a stage where the laryngeal takes over the phonetic specification of the preceding vowel and is realised as a voiceless vowel is what has to be assumed for standard IE laryngeal developments anyway. But then Isaac claims that in addition to this the form was also equivalent to [seːtí-]. Now this is incongruous and stretches credibility beyond limits. It is already hard to believe that the sequence [eh] could be regarded as [eː], i.e. a long voiceless vowel, at the same time. But the real crux is the diacritic < > below the *e*. This diacritic stands for 'more rounded' in IPA. Where does this roundedness come from? There was no roundedness present in the word to start with, and, as said above, spreadness, the feature claimed for [h], is the exact opposite of roundedness. Unfortunately, Isaac doesn't explain his phonetic interpretation here. So the reader is left on his own to come up with a phonetic explanation for the whole process. To me, the matter boils down to this: [h], which has no specific features as such, receives spreadness from the preceding [e] which, in turn, is devoiced and labialised (rounded) because of [h]'s spreadness, a feature which is the opposite of roundedness. Be that as it may, the triad [sehtí-] = [seetí-] = [seːtí-] is used as input for the Proto-Celtic sound change *ē > *ī, which, according to Isaac, produces the triadic output [sihtí-] = [siití-] = [siːtí-]. It is to this form that Isaac's rule of laryngeal loss without compensatory lengthening in labial contexts applies. But labiality (spreadness or rather roundedness?) is only present in the form because Isaac claims it to be, without any external piece of evidence. Furthermore, the invocation of a threefold phonetic equivalence that combines the phonological presence of a laryngeal with a phonetic long vowel (which could only be the result of the loss of the laryngeal) in order to have the form undergo a long-vowel sound change *and* retain the laryngeal to be lost without lengthening afterwards is *ad hoc*. Formulating a rule in such terms deprives it of the possibility of falsification and ultimately renders it circular. I feel forced to conclude that phonetic realism cannot be invoked for this kind of explanation.

One other word that probably contains the sequence *-h₁ti-, but which fails to show laryngeal loss and which is not mentioned in the book, is OIr. *áith* 'kiln' < *h₂eh₁ti- (Irslinger 2002: 197–198). If the circular *u̯l̥h₁ti- and *-(p)r̥h₁tí- (36–37; see below) are left out of the calculation, what remains is a very considerable complication of the rule that has been introduced to account for a single form, viz. *sith-*. This is uneconomic as per Occam's Razor and it is weakly founded in view of the fact that other explanations of *sith-* etc. are available. There is another issue connected with the etymology of OIr. *sith-* etc. The complicated and phonetically implausible explanation is only necessary if the proto-form is really an *i*-stem *seh₁tí-, for which – although it is possible – there is no evidence (except for the circular phonological reason developed by Isaac). The foundation for the reconstruction *seh₁tí-, however, is not very strong at all. In the initial survey of all those Celtic, Italic and Germanic forms that directly or indirectly belong to the etymological family of *sith-* (21–24), Isaac acknowledges that for Celtic both the roots *seh₁- and *sei̯h₁- would serve to explain the attested forms. Critical to his decision for accepting one of these two root variants to the exclusion of the other are the

adjectival forms OIr. *sír*, W *hir* 'long' and their irregular comparatives *sia*, *hwy* and superlatives *siam*, *hwyhaf*. He writes:

> "But in *$seih_1$-, while the positive grade and the nominal are derived from the zero-grade, the comparative and superlative are derived from the full grade, giving anomalous ablaut suppletism in the paradigm of comparison. It is only in *seh_1- that all Celtic forms are derivable from the same ablaut grade of a single root." (24)

This is a very curious argument. It is a cornerstone of IE morphology that different grammatical categories are derived from different ablaut grades of a root. The synchronically irregular adjective *sír/hir* with its manifest affinities to the so-called Caland-system is a very obvious case where archaic ablaut behaviour would be expected to be found in the first place. What is more, zero-grade positive grade of the adjective vs. full-grade comparative and superlative is the regular pattern found, for example, in Ved. *yúvan-* 'young' < *h_2iu-Hon-, comp. *yávīyas-*, sup. *yáviṣṭha-* < *$h_2iéu$-(i)ies-/-isth_2o-, *tigmá-* 'sharp' < *(s)tig-mó-, comp. *téjīyas-*, sup. *téjiṣṭha-* < *(s)téig-iies-/-isth_2o-, or in Greek κρατύς < *krt-ú-, comp. κρέισσων ← *$krét$-ies-, and it is no more anomalous or remarkable than those. If anything, this archaic type of ablaut rather looks like evidence in favour of basing the reconstruction on this paradigm. Nevertheless, Isaac uses this argument to discard *$seih_1$- and to restrict himself solely to the base *seh_1-. If, alternatively, the proto-form were *sih_1-tú/í-, Proto-Celtic *situ/i- with short vowel could be easily explained by a Dybo-style laryngeal loss in the structure CUHC. Finally, it must be noted that Isaac's attempts at *sith-* imply the retention of the laryngeals until a period *after* the complete loss of *p and after *$\bar{e}$ > *$\bar{i}$ (cp. also 43 and explicitly 55).

The chapter culminates in a section where the laryngeal values of roots with hitherto unspecified larngeals are conjecturally determined by interaction with Isaac's newly posited rule (35–37, 54), even before its final formulation has been made. The drawing of conclusions like the identification of hitherto unknown laryngeals should only be undertaken when a rule has been established beyond doubt. In the case of a very complicated rule like the present one this can only happen after the rule has been subjected to peer evaluation. To cite an example: because his theory demands it Isaac identifies the laryngeal of the root *$uelH$- 'to be strong, to have power' as *h_1. McCone (1991: 16) suggested *h_2 on the implicit hypothesis that the weak *a*-inflection of the OIr. verb *follnathair* 'to rule' was the reflex of an originally strong ablauting paradigm in *$-n\bar{a}$-/-nă- < *$-neh_2$-/-nh_2- (McCone 1991: 12). Isaac makes reference to this divergent opinion not where it would have been most appropriate, that is, in the section where the matter is discussed and the solution proposed (36–37), but only in a footnote much later (54) in the summarising section.

The chapter concludes with a number of afterthoughts and appendices. The first (47–48) contains speculation about the IE word for 'man' *$uiHro$-.[14] Isaac's rule forces him to specify the laryngeal as *h_3. According to him, *uih_3ro- should be derived from root *$ueih_3$- 'to wither' in the sense of the 'withering one, mortal'. Now there is not the least bit of evidence outside of Isaac's theory that this root had *h_3, and even its existence as a separate root is under doubt. In LIV 665, it is registered as *$uei(H)$- with a question mark. De Vaan (2008: 677) cautiously assigns the alleged Latin dervatives of this root to *$ueih_1$- 'to plait, weave'. Isaac's idea boils down to the suggestion that for semantic reasons Celt. *$uiros$-, Lat. *uir*, Germ. *wiraz*, Ved. *vīrá-*, etc. do not belong to the same etymon as Toch. A *wir* 'young' (cp. NIL), a proposal that runs against all available old IE sources that allow semantic insights.

The second afterthought (49–50) relates to the etymology of OIr. *enech*, W *wyneb*,[15] Bret. *eneb* 'face' < appr. PC *$enik^uo$-. Isaac notes the applicability of his rule of laryngeal loss to explain PC *$enik^uo$- < PIE *$(h_1)eni$-h_3k^uo-. Not mentioned is W *modryb* 'mother's sister' etc. < *$mah_2trV(h_3)k^uih_2$ which has a suffix with a comparable shape. The historical phonology and morphology of the attested Celtic words for 'face' is beset with problems. In Old Irish,

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[14] A full discussion of *uiH-ró- can be found in NIL 726–729.
[15] Because of his belief in a very early loss of *p, Isaac cannot explain the *wy* of *wyneb* from *$(h_1)ep$-$(h_1)eni°$ (cp. NIL 380).

the dat. pl. is unexpectedly *inchaib*, for which Isaac offers an *ad-hoc* explanation: the vowel of the second syllable was $*i^u < *i$, labially affected by the ensuing labiovelar. Depending on the context this vowel was responsible for the different outcomes in the paradigm. I have nothing original to contribute to this problem, but I want to add that in the discussion of *enech*, *inchaib* it would be useful to also include OIr. *inad* 'place, spot' < $*(h_1)eni\text{-}sedom$ or $*(h_1)eni\text{-}pedom$ and *inathar* 'intestines' < $*(h_1)eni\text{-}h_1e/oh_1tro\text{-}$ with comparably unusual vocalism.

In an appendix (56–59), Isaac discusses the Latin word *uir* 'man' and proposes a language-internal explanation for the short vowel, according to which it is the result of a relatively late Osthoff-type shortening.

In summary, it must be said that this chapter builds upon a vast and inextricable number of premises. If only one of those premises were removed, the theory would collapse like a house of cards. One premise is the phonetics of the laryngeals. Although Isaac expressedly calls his phonetic interpretations a "working hypothesis", it is clear that the whole theory stands or falls with their plausibility. By the end of the chapter, all reservations have been forgotten and the results are presented as facts. But as long as the phonetics of the laryngeals are basically unclear or under wide dispute, it is idle to argue on the basis of their phonetic features. Another complex of premises is of course the etymologies themselves. For example, in the case of $*b^huh_2tah_2\text{-}$, $*b^huh_2tí\text{-}$, $*b^huh_2tó\text{-}$ (32) the reconstruction with $*h_2$ is uncertain because it is based on a circular argument (see Kümmel 2007: 335 fn. 272). Another example: if *CṚHT did give *CRāT regularly in Celtic (as Isaac concedes) it becomes a matter of taste if one derives PC *blātu- from zero-grade $*b^hl̥h_3tú\text{-}$ or from full-grade $*b^hléh_3tu\text{-}$. Both reconstructions are morphologically viable (cp. Irslinger 2002: 86), but the zero-grade form would directly refute Isaac's theory. His decision to operate with the full-grade form to exemplify his rule is therefore circular. But even with his preferred reconstruction $*b^hléh_3tu\text{-} = [\text{ˈbloʕʷtu-}]$ there remains the feeling of arbitrariness because it contradicts the inner logic of his rule. According to the rule, $*h_3$ is lost in the context of "suprasegmental roundness, spread over a stretch of phonic material" (33). There is certainly a great amount of this to be found in [ˈbloʕʷtu-]. The expected loss of the laryngeal after $*ŏ$ (generic or coloured by the laryngeal) would in any case be paralleled by that assumed for $*h_2oh_3mó\text{-} > $ PC *ŏmo- 'raw'. And yet what we get is PC *blātu- with a long vowel. Isaac addresses this contradiction (40–41) by setting up an extra rule that laryngeals are not lost after mid vowels, unless they are immediately followed by a labial consonant. Making rules like this is *ad hoc*, or, as I would rather call it, *ad formam*. It seems to me that the objective of the chapter has never been made sufficiently clear. While in the introduction the author presents as his goal the clarification of the development of *CṚHC and *CUHC, the starting point for his investigation is *CEHC, and indeed many of the examples he discusses are *CREHC. The basis of his examination are formations with dental suffixes, taken from Irslinger 2002 (26). This leads to a strong emphasis on roots of the structures *CṚHt and *CUHt. Incidentally, since a considerable subgroup of these consist of *u*-stem abstracts, it comes as no surprise that a preponderance of labial vowels can be observed in the chosen material, leading to a distortion of the picture. Forms of other structures are brought into the discussion in a rather random fashion. However, the concluding summary contains roots of all different shapes, like CUHC (C being any consonant). Isaac lists 81 examples, but a whole series of significant and relevant items have been passed over in silence. Some of those words provide support for the rule: Ir. *mún* 'urine' < $*muh_1no\text{-}$ (LIV 445–446 s.u. $*mi̯eu̯h_1\text{-}$ 'to move'). Other words are ambiguous because the colour of the involved laryngeals is uncertain. Nevertheless, they could contain potential counterexamples to Isaac's rule:[16]

OIr. *úr*, W *ir* 'fresh, green, etc.' < $*puHró\text{-}$, an adjectival formation to the root $*peu̯H\text{-}$ 'to purify' (LIV 480). If this root can be connected with the PIE word for 'fire', $*peh_2u̯r̥$, in the

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[16] The obvious solution within Isaac's system would be to reconstruct all of them with $*h_1$.

meaning 'purified by fire', the laryngeal should have left behind no compensatory lengthening upon its loss according to Isaac's theory.

OIr. *rún*, W *rhin* 'mystery, secret' < *$ruHneh_2$-. Various etymologies have been proposed (see Lühr 2000: 216–217). If the root is *$reu̯H$ 'to rip', *h_1 is excluded (LIV 510), *$h_{2/3}$, however, should have been lost without lengthening according to Isaac's theory.

Gaul. *-dunum*, OIr. *dún*, W *dinas* 'hilltop fortress' < *$d^{(h)}uHno$-. Watkins 1990 proposed an etymology with *h_2, which, however, is not generally accepted.

OIr. *blár*, W *blawr* 'grey' < could be from *$b^hléh_2ro$- or *$b^hl̥h_2ró$-. If it is the latter, the treatment should have been like *$m̥rh_2tó$- > *$mrato$-.

However, it requires not much effort to find genuine counterexamples to almost all subrules:

OIr. *claidid*, W *claddu* 'to dig', W *clawdd* 'dyke, ditch': These can be derived from *$kl̥h_2-d^{(h)}$- (Schumacher 2004: 412). No short vowel should have resulted from this preform according to the predictions of Isaac's rule. The apparent full-grade in *$klādo$- (W *clawdd*) is analogical according to Schumacher (loc. cit.).[17]

The 3sg of W2 (= A2) stative verbs in *-eh_1-ti, e.g. *$h_1rud^héh_1ti$ 'is/becomes red', should have been shortened to PC *$ruditi$ in a *$seh_1tí$-type process. In Irish, this should have resulted in an S2 (= B2) verb [†]*ruidid*, ·*ruid*. I acknowledge, though, that the long vowel could have been reintroduced through intraparadigmatic pressure.

W *pridd*, OIr. *cré* 'clay': Many problems beset this word (see Griffith 2009 for the latest contribution to the scholarship about this word), but the Welsh form seems to presuppose *$k^u̯reh_1i̯^°$ with a shape that is reminiscent of that of *$seh_1tí$-, but without loss of the laryngeal without compensatory lengthening.

OIr. *rúsc*, W *rhisg(l)* 'bark' < *$ruHsko$-. The root of this word is most likely *$reu̯H$- 'to rip' (Matasović 2009: 317), the laryngeal of which cannot have been *h_1 (LIV 510). *$h_{2/3}$, however, should have been lost without lengthening according to Isaac's theory.

The OIr. personal name *Sadb* is traditionally derived from *$su̯eh_2du$- 'sweet' (a morphologically explicit explanation by Stüber 2006: 223–224). One may compare the frequent use of the element *suadu-* in Gaulish onomastics. McCone (1996: 63) assumes a Osthoff-type of shortening for PC *$su̯ādu̯ā$- > Proto-Irish *$su̯adu̯ā$-, but pace DLG 284 there is nothing in Gaulish that tells us that the *a* in names like *Suadulla* was long. For all the evidence we possess, the adjective 'sweet' could have been *$su̯adu$- with short *a* already in Proto-Celtic. Isaac's rule predicts laryngeal loss with compensatory lengthening. Alternatively, the short *a* could be ascribed to a secondary, analogical ablaut *$ā/a$ within Celtic (such a morphological explanation, however, could be proposed for a great number of 'irregular' laryngeal reflexes).

OIr. *súil* 'eye' is traditionally derived from *suh_2li-, a derivative of the PIE word for 'sun', *$séh_2u̯l̥$. Irrespective of the etymology, a pre-form *$suHli$- with any laryngeal should have been shortened by Isaac's rule, cp. the structurally parallel forms OIr. *cuil* 'gnat', W *cylion* 'midges' < *$kuH-lí$- (46).

This list contains only examples that came to mind. A systematic search would doubtlessly bring to light many more relevant forms.

The foregoing discussion should have made clear that Isaac's formidable task of finding a unitary solution for 'irregular' laryngeal treatments in Celtic is beset by a series of methodical obstacles. In fact, I think it is an impossible task. If previous, and current, studies of the problem of the laryngeal reflexes in Celtic have taught us anything, then that the matter cannot be approached by one Grand Unified Theory. Unglamorous as it may appear, I believe it will remain the most reasonable approach to see in the laryngeal treatments of Celtic the result of many separate phenomena. Some may be due to 'übereinzelsprachlich' developments like Dybo's Rule. Others may be due to different reflexes of the laryngeals in different environ-

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[17] A similar relationship exists between OIr. *blad*, W *blawdd* 'fame, glory, triumph'.

ments, like a difference between #CR̥HC and #R̥HC (cp. Irslinger 2002: 26).[18] In some cases short vowels may be due to secondary ablaut (secondary or morphological zero-grades), as surmised by Schaffner (2001: 502–503).

Of all existing explanations of the problem of *CR̥HC, it seems to me that Schrijver's idea (1995: 168–191; endorsed by Schumacher 2004: 135–138) has the greatest potential of accounting for the extant forms. It is interesting to observe that Isaac's criticism of Schrijver's explanation is not based on that theory's lack of explicatory power, but on an aprioristic conviction about the phonetic nature and behaviour of the laryngeals, which, according to Isaac "must be interpreted as strictly consonantal" (26). What does this mean? In the context, this statement seems to be directed against the assumption that laryngeals as such could have a vocalic reflex in the attested languages (what vocalic reflexes can be found would rather be the result of anaptyctic vowels, according to Isaac). But the world's languages have examples of consonants that were vocalised under appropriate conditions (see Kümmel 2007: 92–93, and 327–336 for thoughts about the vocalisation of the laryngeals), and the one thing that is clear about the nature of the PIE laryngeal is that they did not pattern with stops and resonants in all their behaviour. It is also noteworthy that in a different context Isaac does not hesitate to assign a laryngeal a consonantal and a vocalic value at the same time (39).

In the final analysis, it has to be said that Isaac's working hypothesis that the different types of laryngeal losses are just aspects of a single phenomenon is nothing more than a hypothesis, and that the theory has to be rejected. It is too complicated, it has largely been formulated *ad formam* and consequently lacks an inherent logic, it is based on an insufficient material basis, and despite its complexity it does not explain a good number of examples.

Chapter 3 is "A Relative Chronology of Sound Change from Proto-Indo-European to Celtic" (61–74). The aim of the chapter is modest and ambitious at the same time. It is outwardly modest in that Isaac purports to clarify the chronology of a select number of phonological developments. As is natural for a relative chronology of sound changes, many, or even most, of them are uncontroversial. The ambition of this chapter, however, lies in the fact that in a rather silent way a number of new proposals are introduced. Apart from the brief "3.1. Introduction" (61), the chapter consists of four sections: "3.2. The Relative Chronology in Symbolic Notation" (61–62) includes 25 sound changes in their presumed chronological order. The numbers 12. and 13. are both twice employed, for entirely unrelated changes. The justification for this is given only at the very end of the chapter, where Isaac explains that the relative order of these changes is "abolutely indeterminable" (74). Nevertheless, for purely didactic purposes it would have been useful to choose a consistent sequential numbering. After all, even the sequence of the indexes 12a., 12b. and 13a., 13b. suggests a succession. Then follow the sections "3.3. The Relative Chronology in Words" (63–65), "3.4. The Examples" (65–69), totalling 41 words, and finally "3.5. Notes on the Order of the Rules" (69–74). All sections are arranged and written more or less in the style of a conference handout, without very elaborate discussions and explanations. This is a pity because occasionally it could have been made clearer what Isaac had in mind, or some ambiguities could have been resolved.

I will now comment on select topics in the order in which they appear in the chapter.

Rule nr. 4 is the metathesis of "*DK > *KD" (D = any dental occlusive, K = any dorsal occlusive; 62 and 74).[19] The rule, which as such is fairly uncontroversial, occupies a significant role in Isaac's hypothesis in the following chapter 4 about the genetic prehistory of Celtic and about its precursors' geographical positions. Nevertheless I want to examine the rule and some of Isaac's conclusions here. This is the rule that produces, for example, OIr. *duine*, W

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[18] A few remarks in the introductory section reveal (27) that Isaac acknowledges the fundamental difference between #R̥HC and #CR̥HC. In fact, he doesn't allow examples of the former type to his discussion. This sober position, however, is flatly contradicted on p. 43 where Isaac says that his rule of laryngeal loss without compensatory lengthening operated after the complete loss of *p, which means that the input for OIr. *rath* etc. 'grace' was "*ørh₃-tó- = [rˤʕʷtó-]", not *pr̥h₃tó-.

[19] See Eska forthc. for a treatment of this cluster in the framework of Articulatory Phonology.

dyn 'human being' < **donįos* < **γdonįos* (Gaul. -XTONION /γdonįon/) < **gdonįos* < **dgon-įos* < PIE **dʰgʰomįos* 'belonging to the earth'. Isaac stresses the parallel with the Greek τίκτω-rule (e.g. τίκτω 'I give birth' < reduplicated **ti-tk-ō*); in fact, he explicitly speaks of an exclusively shared innovation of Greek and Celtic. However, as Lipp (2009: II 8, 315–317) shows, the similarities between the Greek and Celtic developments are superficial only. Whereas in Greek the τίκτω-rule is unconditioned and operates on heterosyllabic and tautosyllabic DK alike (e.g. **tʰkʰōn* > χθών, **ar.tkos* > ἄρ.κτος, **tit.kō* > τίκ.τω), in Celtic the metathesis affects only tautosyllabic DK. The relevant forms are, on the one hand, PIE **dʰgʰomįos*, exemplified above, and PIE **h₂ṛt.kos* > **ar.tkos* (note the shift of the syllable boundary that accompanies the resolution of the syllabic resonant) > **ar.ktos* > **ar.χtos* > **ar.tos* > Gaul. *arto-*, OIr. *art*, W *arth* 'bear'. On the other hand we find pre-Celtic **pṛd.keh₂* > **φrit.kā* > **rik.kā* > W *rhech* 'fart', and pre-Celtic **h₁rudʰ.kiįeh₂* > **rut.kiįā* > **ruk.kiįā* > OIr. *ruccae* 'shame, disgrace (< **blushing, reddening)' without metathesis. On the pre-Celtic level, **h₂ṛt.kos* and **pṛd.keh₂* showed the same division into syllables, but they must have been syllabified divergently as **ar.tkos* and **φrit.kā* immediately before operation of the metathesis. The different outcomes inform us that the metathesis can only have occurred *after* one or both of the specific Celtic sound changes **#h₂/₃ṚC* > **#aRC* and **CṚC* > **CRiC* had taken place. The ordering of rules in Isaac's relative chronology must be adapted accordingly. What is now nr. 4 must be placed after 5 or 6. This definitively tips the scales in support of an independent rule of dental-dorsal metathesis in Celtic alone. The similarities with Greek are typological only, not genetic. Speculations about the prehistories of Celtic and Greek are accordingly unfounded.

Regarding the change in rule nr. 6, "**L* > **Li* / *C__T*", Eugen Hill gave a paper at the 13th *International Celtic Congress* in Bonn 2007 in which he defended the idea that **L* became **Li* also before nasals, e.g., OIr. *tlenaid* 'to take away' < **tḷn(e)h₂ti*.

Isaac assumes two separates rules concerning the fate of PIE **p*. First, in rule 12a., **p* > **b* /_R, then in rule 14. (which is actually the fifth rule after 12a.) **p* > **φ* in all remaining instances. This separation is counterintuitive, and the first of the two rules is phonologically (!) implausible and its argumentative basis is wrong. But one after the other. First of all, the development **p* > **b* / __R would be structurally isolated and unmotivated. One might as well expect **t* > **d*, **k* > **g* and **kᵘ* > **gᵘ* in the same environment; wherever such voicing in leniting positions occurs, it usually comprises the entire plosive series, not just the labial member (see the typological data in Kümmel 2007: 49–53).

In fact, the rule **p* > **b* is posited to explain a single word, viz. Isaac's example nr. 31, PC **gabro-* 'goat' (68, 71), where an unexpected **g* contrasts with forms in **k* in other IE languages, e.g. Lat. *caper*, OIcl. *hafr* 'goat', Gr. κάπρος 'boar', etc. The Celtic form is traced to a pre-form **kpro-* (the idea goes back to Falileyev & Isaac 2003: 8 fn. 25) > **kbro-* (voicing of **p* before **r*) > **gbro-* (voicing assimilation of **k* > **g* before **b*) > **gabro-* (anaptyxis). The corrolary of this etymology is that the vowel **a* of the word for 'goat (etc.)' in several IE languages goes back to anaptyxis (or 'schwa secundum' in older terminology) into a zero-grade form **kpro-*. This cannot be correct. In Greek, the vowel inserted into heavy consonant clusters was *i*, not *a* (Lejeune 1972: 208–209, and, in greater detail, Klingenschmitt unpubl. 41–50). Greek κάπρος 'boar', therefore, speaks decisively for the reconstruction of a PIE proto-form **kapro-* with original (non-ablauting?) **a*. This may find further support if Ved. *kápṛth-* 'penis' and ModPers. *kahra* 'kid' belong here as well (see Mayrhofer 1992: 302), but the Greek evidence alone suffices to eliminate the hypothetical **kpr-* (see also Schaffner 2001: 129–130). In modern IE linguistics, most scholars have learned to live with the marginal phoneme **a*, which has a special preference for the position after sounds of the velar series. There is therefore no reason to reconstruct anything other than **kapró-* for the proto-language. It follows from this that the initial **g* in PC **gabro-* cannot have arisen through immediate contact with a following voiced occlusive. Its appearance must be accounted for differently. A discussion of all previous suggestions can be found in Graf (2006: 44–52). Among

these, Pokorny's idea of influence from a form continuing PIE *$g^h a\mathit{i}do$- 'goat' is at least worth considering. But even if this explanation should be rejected, the sporadic change *ka > *ga, not unheard of elsewhere, cannot be ruled out.[20] In Latin (and Italian) it can be found especially in loanwords from Greek (e.g. *gamba* beside *campa* 'leg' < *καμπή, the towns *Gallipoli*, *Gaeta* < Καλλίπολις, Καιέτας) and from other languages (e.g. *gattus* beside *cattus* 'cat'). In Italian dialects, the tendency towards sporadic voicing of *k* before *a* and other dark vowels became even more pronounced; sometimes those variants were adopted into the standard language (e.g. *gastigare* beside *castigare* 'to chastise', *gonfiare* 'to inflate' < Lat. *conflare*, etc.; see Rohlfs 1949: 250–252). From the Iberian Peninsula, an apposite example is the name of the ancient country of *Callaecia*, now the province of *Galicia*.

This means that *$kapro$-/$gabro$- can and must be accounted for differently. If there never was a pre-form *$kbro$- < *$kpro$-, the necessity for positing the change *p > *b / _R in the first place vanishes. There is actually no real evidence for phonetic [b] in words like *$gabro$-. The Insular Celtic languages, e.g. OIr. *gabor*, W *gafr*, have /β/ or /v/ in this position. All that we have for Gaulish are spellings with <b> like in *Gabrosentum*, but for all that can be conjectured about the phonetics of Gaulish, <b> in word-internal position could represent phonetically lenited [β]. Consequently, it is phonologically much easier to combine the apparent change of *p > *b with the general weakening of *p, and I am convinced that this is also the phonetically more realistic description. It is the general consensus that the first step in the weakening of PIE *p in Celtic was the bilabial fricative *φ (Isaac's change nr. 14.), which is in line with a wide-spread typological tendency (cp. Kümmel 2007: 57). Approximately the following development can be sketched: before resonants, the newly developed *φ acquired a voiced allophone [β], which was subsequently re-interpreted as the phonetically lenited allophone of *b. If the language already had [β] as an allophon of *b in its system, this re-interpretation would have been most natural. If the language did not yet possess lenited allophons of the voiced obstruents, the emergence of *β, marginal at that point in time, may have been one of the triggers for more wide-spread phonetic lenition. I would suggest changing the chronology in the following way:

> 12a. *p > *φ
> after 12a.:   *φ > *β / V__R
>   *φ > *u / $V_{\text{[-front]}}$ __n
> after 12b.:   *φ > *x / __ {t,s}V

Alternatively, instead of the last change, one could posit *P > *K / __T [P = any obstruent with labial articulation, T = any obstruent] previous to 12a., or instead of Isaac's rule nr. 9., viz. "*K^w > *K / __C".

Another point involving the development of *p is in need of greater precision. I do not think it will do simply to state that the difference in diachronic behaviour between, for example, PIE *$kh_2pnó$- > *$kh_2ǝpno$- > *$kh_2ǝ\varphi no$- > *$ka\varphi no$- > *$ka\mathit{u}no$- > OIr. *cúan* 'harbour', and pre-Celtic *$tpnV$- > *$tǝpnV$- > *$tǝ\varphi nV$- > *$tǝnV$- > *$tanV$- > W *tân* 'fire' is due to a difference in timbre of *schwa* in the different consonantal environments (72). Although this formulation is very vague and noncommittal, it is of great phonological consequence because it demands the unrealistic assumption of two distinct *schwa*-phonemes. There must be other ways to explain the difference. Either the laryngeals did not develop an anaptyctic *schwa*-sound in the way Isaac assumes, meaning that the sound was phonologically *a from the beginning, or there was something else – perhaps irregular – going on in the word for 'fire'. My guess is the latter,

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[20] There is also the well-known variation between the roots (descriptively) *$g^h ab$-, $g^h eb^h$- and *kap- for 'taking, grabbing' in Western IE languages. Joseph Eska points out to me (pers. comm.) that this and other root variations of the same kind reconstructed for PIE might be explained by positing a variation between phonemic status and phonetic realisation.

even more so since beside W *tân* we find OIr. *ten*, *teine* (once *tainid*). This looks like the reflex of an ablaut-relationship *$tepnV$-/*$tpnV$-. Perhaps what happened was the following: *$tepnV$-/*$tpnV$- > *$te\varphi nV$-/*$ta/\partial\varphi nV$- (for the present argument it is insubstantial whether the anaptyctic vowel was *∂ or *a) > *$tenV$-/*$ta\varphi nV$- (*φ > Ø /$e\_\_n$; there is no counterexample to this rule) > *$tenV$-/*$tanV$- (analogical loss of *φ in the zero-grade form).

Rule nr. 20., "*∂ > *a" becomes unnecessary if, as I have tried to demonstrate in the review of chapter 2 above, Isaac's context-sensitive rule of laryngeal loss without compensatory lengthening is invalid. Instead, the rules nr. 5., "*φ > *∂ / $C\_\_C$", and nr. 13a., "*φ > *∂ / $T\_\_T$" should be reformulated to *Ø > *a.

Regarding rule nr. 21, "*-Vns > *-Vs", see the recent article by Griffith 2005.

The presentation of the development *$h_2\bar{e}gst$ > *h_2axst in example 3 (66) is unfortunate. It should have been remarked that the alleged shortening of the vowel is not regular but must be due to morphological regularisation. For a completely different solution see Schumacher (2004: 192).

The development of PIE *$d^hg^h\bar{o}m$ 'earth' > OIr. *dú* 'place' (example 7, p. 66) is unlikely to be regular. It is plausible that OIr. acc. sg. *boin* 'cow' and acc. sg. *don* 'place' continue PIE *$g^u\bar{o}m$ < pre-PIE *$g^u\acute{o}u\dot{m}$ and post-PIE *$d^hg^h\bar{o}m$, remodelled from *$d^h\acute{e}g^h\bar{o}m$ < pre-PIE *$d^h\acute{e}g^homm$, with an early shortening of *-$Vm\#$ > *-$Vm\#$ (see Stifter 2008: 278–279). In that case, the OIr. nom. sg. *dú* must be analogical after the pattern of other *on*-stems (similarly Lipp 2009: II 75).

Example nr. 12 (66), PIE *h_2ueh_1nto- 'wind', is problematic for several reasons. On several occasions in the book reference is made to the derivation of OIr. *fet* 'whistle' from this word (e.g. 23 fn. 39, 73). This etymology is weak for two reasons. Not only can a semantically much more attractive etymology be found for this word, i.e. from the Celtic root *$suizd$- 'to blow' (Schumacher 2004: 611; the *f* of *fet* must either be due to the generalisation of the lenited variant, or to the influence from related verbs like *do·infet* 'to blow'), but OIr. *léicid* 'to leave, let' with long *$\bar{e}$* from *$link^u\bar{\imath}ti$ ← *$link^ueti$ is a very serious obstacle to deriving *fet* with short *e* from *$uinto$- < *$u\bar{\imath}nto$- < *$u\bar{e}nto$- < *$h_2ueh_1\partial nto$- < *h_2ueh_1nto- (Schrijver 1993). There is, then, no proof that *h_2ueh_1nto- gave *$u\bar{e}nto$- and then *$u\bar{\imath}nto$- in Proto-Celtic, since the only uncontested successor of this word, W *gwynt* 'wind', could conceivably continue PC *$u\breve{e}nto$-. Lipp's (2009: II, 142–143) reconstruction of a vṛddhi-derivative *$h_2u\bar{e}h_1nto$- as basis of the Welsh word is unnecessary for the same reason. Another word that has some bearing on the matter is PC *$m\bar{\imath}ss$- 'month' < *$m\bar{\imath}ns$-. It looks as if the *$\bar{\imath}$ in this form continues *meh_1ns-, but this is deceptive. The *$\bar{\imath}$ could have originated in a nominative corresponding to PIE *$m\acute{e}h_1n\bar{o}s$ and could have been generalised from there to the rest of the paradigm. In any case, Isaac's account of the development of *h_2ueh_1nto- is inconsistent with that of another word. If, in his hypothesis, the sequence *$e.\partial$ < *$eH\partial$ in *$ue.\partial nto$- < *h_2ueh_1nto- and the sequence *$e.o$ < *epo in *$e.ore$ < *$peporh_3e$ (> OIr. *·ír* 'granted') were contracted to *$\bar{e}$ so early as to become input for the rule *$\bar{e}$ > *$\bar{\imath}$, then it is hardly credible that *$e.\partial$ < *$ep\partial$ should not be treated the same way. This, however, is what Isaac's explanation of OIr. *té* 'hot' < *$te.\partial nt$- < *$tep\dot{n}t$- entails (example nr. 15, p. 66), *pace* the remarks on top of p. 71.

Chapter 4 "The Origins of Celtic" (75–95) directly continues a theme of the preceding chapter. Taking the PIE word for 'yesterday' *g^h-$d(i)ies$ as his starting point, Isaac demonstrates conclusively that the different treatments of this word in Italic (Lat. *heri, hesternus* < *$g^hes(i)$ ← *g^hd^hes) and Celtic (OIr. *indé*, W *doe* < *γdes < *g^hd^hes) cannot be combined in a coherent, common chronology of early sound changes and thus are incompatible with the concept of an Italo-Celtic genetic unity immediately after the breakup of Proto-Indo-European. He then goes on to suggest that various early sound changes and what he identifies as morphological innovations instead relate Celtic to other branches of IE at a period previous to those sound changes that Celtic shares with Italic. Rather than being evidence of numerous consecutive contacts with different established linguistic branches, Isaac thinks that these common-

alities should better be analysed as remnants of an early period when identifiable dialects had not yet formed. The proposed proto-form for 'yesterday', viz. *g^h-d(i)i̯es, is vital to the argument. This is indeed the widely accepted reconstruction, consisting of a particle *g^h and a case form of a root noun belonging to the root *dei̯- 'to be bright' (cp. NIL 69–70, 73–74). Lipp (2009: II 189–198) sets up a very different proto-form, viz. a PIE locative *d^hg^hi̯es 'in the brightness of the day', belonging to the hypothetical root *d^heg^h- 'to be bright'. This root is supposed to underlie also the PIE word for 'earth' *d^heg^hom-, and the Germanic words for 'day'. If this should be the right etymology of 'yesterday', this would affect Isaac' theories in many details, but I think the main line of argument, i.e. the discrepancy between the Italic and Celtic forms, would be left untouched.

Surveying the various matters discussed in this chapter, I am left with the impression that Isaac is mostly troubled by problems of his own making: a very narrow definition of the concept of Italo-Celtic; and rather idiosyncratic ideas about several apparent deep-level isoglosses that connect Celtic with other branches of Indo-European apart from Italic and about what they can actually tell us. Turning to the latter topic first, I think that many of the chronological – and partly geographical – problems encountered and discussed by Isaac vanish if one does not require that any phonological change in Celtic that has a parallel elsewhere must be related, phylogenetically or by contact. In fact, it can be argued in almost all cases that the processes which Isaac believes to be related must have been independent of each other. It is strange that Isaac apparently did not consider the possibility of independent, typologically similar processes. For example, as regards the metathesis of dental-dorsal clusters, a change allegedly shared with Greek (81), the relevant clarifications have already been presented in the section on the previous chapter. The changes in the two languages are not related because the details of the rules differ. The same can be said about the application of Rix's Law, that is, the treatment of *#HṚC. Because of similar results in the two branches, Isaac compares the rule in Celtic to that in Tocharian (82). The rules have a superficial likeness in that *h_1 is lost without a trace, but *h_2 and *h_3 are vocalised as *a. However, it must be kept in mind that a fundamental difference between the behaviour of *h_1 on the one hand and *h_2 and *h_3 on the other is observeable in other contexts in Tocharian as well. In the traditional doctrine about laryngeals in Celtic, nothing of that sort has been noted. Even if one were to accept Isaac's colour-sensitive rule of laryngeal loss without compensatory lengthening in Celtic (which I do not do, as argued above), there are no additional parallels with Tocharian to be found in this rule.

Another case in point is the loss of aspiration in voiced obstruents, a development also found in Iranian, Balto-Slavic and Albanian, as well as Anatolian and Tocharian (the latter not mentioned by Isaac). For Isaac this is a change that spread by contact through the languages that have it (90, 93).[21] In Iranian, this is patently a relatively late change (post-Proto-Indo-Iranian). In Balto-Slavic, nothing can be said about the chronological placement of the rule, but it is at least noteworthy that a difference in treatment between the so-called *mediae* and *mediae aspiratae* has left a trace in the effects of Winter's Law, which might indicate a rather recent than early date. Nothing can be said about the chronology of this rule in Albanian. In any case, since the received post-PIE phonemic system with *tenues*, *mediae* and *mediae aspiratae*, which all of those languages must have inherited, seems to be a typologically marginal and therefore unstable one, a restructuring of that system was natural. Reduction of the *mediae* and *mediae aspiratae* series into a single unaspirated one is a trivial option and can easily have happened independently in several language families.

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[21] In order for this to work in a geographical sense, the change must have spread somewhere in eastern Europe (93 fn. 144). Although I do not subscribe to the idea of loss of aspiration as an isogloss, I nevertheless want to add that *a priori* the scenario is not absurd for Celtic. The traditional placement of Celtic in western Europe (France, Germany) is linked to the spread of distinct cultural styles, western Hallstatt and La Tène, and to the fact that the best evidence for Celtic languages – albeit from a much later date – stems from there. This traditional argument silently equates archaeological cultures with languages and with ethnic ascriptions, something that is methodically inadmissable. A more easterly origin of Celtic is just as possible.

If someone insisted that this rule be shared with another branch of Indo-European, for argument's sake it could as well be maintained that it was related – not genetically, but through contact – to one aspect of Grimm's Law in Germanic. The line of arguments could run thus: like Germanic, Proto-Celtic was affected by the change of *mediae aspiratae* > voiced fricatives. Assuming that in certain clusters (most prominently after homorganic nasals) the fricatives became (non-aspirated voiced) obstruents again through natural phonetic processes of delenition, and assuming also that Proto-Celtic already possessed voiced fricatives as allophones of plain *mediae* in intervocalic position, this would have led to an allophonic distribution of phonemic voiced stops and fricatives that would have looked largely arbitrary. One essential step towards regularisation of the distribution would have been to give up the phonetic opposition between voiced stops and voiced fricatives altogether and to redistribute them as positional variants of one another. Since in Proto-Celtic the voiced fricatives were only allophones of the voiced stops in leniting positions, the result of this complex chain of developments looks like a simple change $*D^h > *D$. It should be kept in mind that the foregoing scenario is purely hypothetical and was construed only to demonstrate that the sound change $*D^h > *D$ in Celtic can be aligned just as easily with changes closer to home, and that there is no need to connect it with superficially similar changes in Balto-Slavic and Iranian.

At the end of the chapter Isaac formulates a reasonable principle according to which "the morphological patterns of a language are diagnostic of chronologically deeper relationships than its phonological patterns, a difference of chronological depth which reflects a contrast between genetic relationships and those of contact" (94).[22] He uses this principle to support an early, deeply rooted relationship of Celtic with "Indo-Iranian, Greek, Balto-Slavic and Phrygian, in various combinations" (92), to the exclusion of Italic, but in my opinion Isaac does so wrongly and one-sidedly. He states that the mentioned branches share the innovation of morphologically overdetermined categories, e.g. the sigmatic aorist and the reduplicated desiderative. Notwithstanding the question of what the significance of 'innovation' is in regard to the common grammar of reconstructable Proto-Indo-European,[23] Italic has been conspicuously left out of the group of languages which share in these 'innovations'. It should be general knowledge that Italic, too, participates in the system of sigmatic verbal formations. Another morphological piece of evidence adduced by Isaac in favour of grouping Celtic with the above-mentioned languages is the relative pronoun *$H\underline{i}o$-, which he claims to be a late innovation, affecting only a segment of the IE languages (93). Regarding its evidential value, I can only insist on the validity of Hettrich's (1988: 501–503) rejection of its late dialectal emergence, despite Isaac's dismissal of Hettrich's position (93). This item is spread over a vast area (Indo-Iranian, Balto-Slavic, Greek, Phrygian, Celtic), cutting across all usually assumed post-PIE dialectal groupings. Basing himself on a study of relative clauses in Vedic, Hettrich (1988: 776–786) arrives at the conclusion that both *$H\underline{i}o$- and *$k^u i$-/$k^u o$- were used as relative pronouns already in PIE, the one for appositive relative clauses, the other one for restrictive clauses. *$H\underline{i}o$- was subsequently lost by an unremarkable process of functional simplification in several branches. Under normal circumstances, an item attested in all those branches of Indo-European would be reconstructed for the protolanguage without hesitation (see the methodology proposed in Mallory & Adams 2006: 107–110).[24] I do not see why this should not be the case here, too. Isaac's point is further weakened by the fact that to this list of languages

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[22] The ideas of Matzinger (forthc.) about the methods of assessing the evidential value of innovations are in a similar vein.

[23] The relevant categories are all reconstructed for the grammar of Proto-Indo-European in LIV. The transparency of categorial morphemes says nothing about their distribution in the attested branches of Indo-European. When a category, however innovatory, can be reconstructed for the protolanguage, all variants of Indo-European must have shared in it. If that category is absent in the individual languages, it must have been lost in the meantime.

[24] The fact that the pronominal stem *jo- is also found in Finnic languages adds nothing to the present question. Since it cannot be reconstructed for the Proto-Uralic language, it is of no moment for the reconstruction of a hypothetical unitary Indo-Uralic protolanguage. Finnic *jo- is best explained as a loan from Proto-Indo-Iranian, but *$H\underline{i}o$'s presence in that branch of Indo-European had been beyond doubt anyway.

Lusitanian can perhaps also be added. For all we can tell from its fragmentary status, it represents a separate branch of IE. Whether it has a particular proximity to Italic or Celtic is a matter of dispute. On the fragmentary text Arroyo de la Luz III (Villar & Pedrero 2001) there appears the form IOM. The editors put raised dots as word separators before and after IOM. If this should be the correct reading,[25] the form looks – to all intents and purposes – like another instance of the pronominal stem *$H\dot{\imath}o$- (cp. Celtiberian *iom*), in yet another western IE language. Against the background of the massed array of putative phonological and morphological proof assembled to show the extra-Italic connections of Celtic, it is noteworthy that the one morphological innovation that certainly patterns Celtic with Italic to the exclusion of all other languages has not been mentioned, viz. the formation of the superlative (Cowgill 1970).[26]

We can now turn to Isaac's other issue, that of the concept of Italo-Celtic as a genetic unity. The chapter is steeped in Isaac's premise that the Italo-Celtic hypothesis is an invalid concept. But what is the Italo-Celtic hypothesis in the first place? In a strict sense, it means that a particular linguistic group that broke away from the rest of the Indo-Europeans was the sole ancestor of the later Italic and Celtic languages. This intermediate group developed identically, without dialectal variation, for a certain period, until it split up into the future Celtic and Italic groups. To put it metaphorically: if the IE languages are a genealogical tree, there is a clearly identifiable Italo-Celtic subnode immediately after the Indo-European proto-language, and this subnode is as real as the Indo-Iranian subnode and the Balto-Slavic subnode. And the 'Balkans-Indo-European' subnode, for that matter, comprising Greek, Armenian, Phrygian, Albanian, Messapic, and possibly Tocharian. The case for such a subgroup is not a bad one (cp. Mallory & Adams 2006: 109, Matzinger forthc.). These languages are connected by non-trivial phonological and morphological isoglosses and by a shared lexicon, and, with the exception of Tocharian, they are and probably have been in reasonable geographical proximity to another. This intermediate language group is not mentioned by Isaac, but if it is a real entity, it poses a threat to some of his claims (e.g., p. 85). Several of the 'innovatory' traits of Greek, Tocharian, Albanian of which Isaac speaks in this chapter and which he regards as isoglosses with Celtic, are not shared by other languages of that intermediate group, for example the metathesis of dental-dorsal clusters. This leads to the conclusion that they must have been individual, unrelated developments after the breakup of the Balkans-Indo-European group, at a time and in an area when and where contact with Celtic most likely was not possible any more. On the other hand, by virtue of being there in Greek and Phrygian, other features like the relative pronoun *$H\dot{\imath}o$- must have been inherited in the other languages as well.[27]

Be that as it may, to return to the definition of 'Italo-Celtic', the concept in a wider sense means that those linguistic entities that were later to become the Celtic and the Italic languages shared in a series of common developments, not only phonological but also morphological and lexical. These developments may have occurred in a *Sprachbund*-type situation, thereby allowing for independent, 'dialectal' developments before and during the common period.

It is evident that by Isaac's rigid standards Italo-Celtic can only refer to the first of these concepts. In his understanding, a single divergent development means the separation of the two branches and demonstrates the invalidity of the concept (76), a requirement fulfilled by the different treatments of 'yesterday' in the two branches. Thus he believes to have demonstrated that the concept of an Italo-Celtic genetic unity must be abolished.[28] Even though on a theore-

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[25] Unfortunately, the editors nowhere discuss the form and the reliability of this reading; neither is the form mentioned in Prósper 2002. It cannot be wholly excluded that IOM is simply a nominal inflectional ending. For another alternative interpretation, the Latin formulaic abbreviation of *Ioui Optimo Maximo*, there is no evidence whatsoever.

[26] A few more items on the border between morphology and lexicon will be advanced by Stüber (forthc.). She speaks of a short period of Italo-Celtic unity. See also Kortlandt 2007.

[27] By this calculation, not many languages remain for which *$H\dot{\imath}o$- cannot be posited directly or indirectly. The whole idea of an innovatory, post-PIE introduction of this pronoun consequently suffers from a *reductio ad absurdum*.

[28] Isaac uses additional circumstantial arguments to refute the notion, arguments which I must admit not to have understood. For example, I do not see why the postulate of an Italo-Celtic linguistic unity entails an ethnic continuity for 2.000–3.000

tical level rigidity of that sort is valid, the question must be asked how appropriate its application is in such matters, how reasonable it is, and what is gained by it? The practical value of the idea can be tested when it is applied to modern languages. According to Isaac's rigid exegesis of linguistic unity, any irregular modification of the phonological corpus of a single lexeme will suffice to split a linguistic entity in two. The inherited word for 'buttocks' or 'anus' in British English is /ɑ:s/ < PGerm. *arsaz* < PIE *h_1orsos*. In American English, this has become /æs/ by a not-rule-driven shortening and fronting of the vowel. Consequently, it must be illicit to speak of a single undivided English language any longer. This is absurd. English today is certainly divided into many different dialects and standards, and it is well possible that under test-glass conditions these variants could develop into mutually unintelligible idioms. But it is also a fact that all existing standards still remain mutually intelligible, and a situation is easily conceivable where future sound changes will affect all variants of English alike. It would be splitting hairs to maintain that the variants of English today are in a situation of language contact instead of genetic unity. Likewise, the dialectal divergence in a single word is of negligeable practical significance. It hardly justifies rejecting an Italo-Celtic unity, unless it could be shown that a sufficiently large number of other differences distinguished the two branches at that period. What is needed are terms for concepts that describe realities. It is beyond question, and even Isaac acknowledges it (e.g., pp. 79, 88), that Italic and Celtic must have been in close contact for a certain period. 'Italo-Celtic' is the best label available for this. In the final analysis, it doesn't matter a lot if we conceive of Italo-Celtic as a distinct post-PIE genetic subnode or as an area of new linguistic convergence after a period of separation, a intervening period which, in any case, cannot have been very long. This means that at that time the structural and lexical similarities between the two groups still must have been very considerable. The result will be virtually the same in both scenarios and only the subtlest analytic tools will show up the differences. In this sense one may conceive of Italo-Celtic as a virtual genetic node. In short, I do not think that the invalidity of the concept 'Italo-Celtic' has been demonstrated conclusively in this chapter. Whether this concept is justified or not must be argued by other methods.

In the final capter 5, Isaac sets out to review "The Rule of Palatalisation in Proto-Irish" (97–113; the significance of the singular will emerge below). In the first part of the chapter, he demonstrates that the relative ordering of the established palatalising processes cannot be securely determined (97–102). Therefore the names given to the palatalisations by McCone (1996: 115–120), i.e. 1st, 2nd, and 3rd palatalisation, are potentially misleading. Isaac proposes to rename them 'internal palatalisation', 'palatalisation by final *ī*', 'palatalisation by internal *ī*', 'initial palatalisation' (102). Besides being more explicit (the first two of them correspond to McCone's 1st palatalisation (101)), Isaac's terms have the additional advantage of being more iconic than their numeric counterparts. Then Isaac turns to the true objective of this chapter. He "question[s] the primary postulate of multiple palatalisations" in the prehistory of Irish (102 fn. 149). Instead he thinks that the Irish palatalisation "is not a matter of multiple palatalisation 'rules' at all, just a unitary rule of palatalisation, operating continuously over an extended period, with other rules of change occasionally bringing into existence new segmental configurations which form new environments for the operation of the rule, giving the false impression of the successive operation of multiple rules" (102–103). This single, unitary palatalisation rule that covers all contexts is formulated on p. 107. Since this task is not possible within the framework of received Irish phonology, Isaac has to operate with one extra phonetic assumption. He posits that there was no merger of inherited *e* and lowered *i* in unstressed syllables. Instead, the former had already been allophonically lowered to [æ] before the latter

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years (88–89). The idea of identity of language and ethnic affiliation was generally given up in linguistics a long time ago. There were different speech communities, but in what way these relate to ethnic, political, cultural, religious, etc. groups – which, of course, cannot be identified in the extant record anyway – is of no moment for linguistics.

came to fill the gap left by [e]. [æ] had a palatalising effect only after front vowels and when syncopated, while [e] palatalised also after non-front vowels. In that manner Isaac manages to explain the difference between OIr. *calad* 'hard' < **kalæθah* < **kaletos* and *aile* 'other' < **ale.ah* < **al(i)i̯os*.

The chapter finishes with a suggestive hypothesis about a phonemic opposition between non-palatalised and palatalised consonants in early British as well (112–113). As Stefan Schumacher informs me, this is indeed what must be assumed. Forms like PN *Emreis* < **Ambros-i̯oh*, *meirch* 'horses' < **markī*, or *eirch* '(s)he asks' < **arkīt* are best analysed as being the reflexes of words with phonemically palatalised word-final consonants. Whereas in South-West British non-palatalised and palatalised consonants simply merged without leaving a trace of the original opposition (e.g. MBret. *merc'h*, MCorn. *ergh*), a difference remained in Welsh in that the palatal on-glide before the palatalised consonants was phonemicised as *i*. Furthermore, the palatalised endings of loans into Irish like *Notlaic* or *oróit* can be best explained by the assumption that the British words ended in noticeably palatalised consonants.

Of the five chapters of this book, I found the last one to be the most convincing at first reading. Isaac's rule of palatalisation achieves a much greater economy of linguistic description. Several related, but separate phenomena (what could be called a linguistic conspiracy) can be collapsed into a single, structurally streamlined process.

But at a second glance, it appears that this structural simplicity is bought at the price of structural imbalance elsewhere in the phonological system. Isaac says that any unstressed **e* was realised as [æ]. Such a conditionless shift is remarkable for two reasons. The development of **[e] > *[æ]* is lowering, although Isaac only speaks of "weakening" (104), which in the context is a misleadingly vague term. Elsewhere in Proto-Irish, i.e. in the cases of **i* and **u*, lowering in unstressed syllables only occurs before **a* and **o* in the following syllable. It is not self-evident that mid-high **e*, which otherwise is not structurally susceptible to lowering effects in Proto-Irish, should show a more radical lowering than the high vowels **i* and **u*. Other vowels that underwent metaphony in Proto-Irish, notably **i* and **u*, must have possessed a relatively broad phonetic range that eventually resulted in their phonemic split to **e* and **o* before back vowels and in retention of their high quality before front vowels. Another instance of a broad phonetic realisation is that of **u* which vacillates between a back position and a fronted allophone [ʏ] before a palatalised consonant. That being so, it is difficult to picture why **[æ]*, itself an allophone of **e*, should have occupied a comparably narrow phonetic range, even more so since at that time a vast stretch in the oral tract between [i] and [æ] would have been 'phonetically unclaimed territory', if I may say so. In particular, should one not rather expect that **[æ]* on its part would have been allophonically raised to **[e]* (or thereabouts) before a following **i*, only to merge eventually with the result of lowered **i* at least in this context? We might expect, for example, that in the framework of Isaac's sound rules **kaneti* [kanæti] > **[kanæθi]* > **[kaneθi]* > †*cainid* 'sings', a form which, incidentally, would fit much better into the verbal system than the irregular, but actually attested *canaid*. Finally, a fundamental phonetic argument that lends plausibility to the traditional view is the inherently greater palatalising effect of [j] and [i] than of [e]. Keeping in mind that the overarching motto of the book is phonetic realism, I think that in this respect the new proposal has less of it than the traditional view.

There remains one more point, which does not compromise Isaac's argument as such but which must be discussed in greater detail. It is most unfortunate that one of the central items for exemplifying the chronology of Irish sound changes and of the palatalisations is the OIr. personal name *Luicrid*. Isaac's account (99–100, 108–109) is erroneous in several substantial respects. It builds on the assumption that that name developed from its Primitive-Irish form **luɣuxʷriθ-* (attested as Ogam LUGUQRIT) to *Luicrid*, which, according to Isaac, is the *lautgesetzlich* reflex. With reference to Uhlich (1993: 125), *Lucraid* with non-palatalised internal cluster, which is also attested, is said to be a synchronically analogical form, re-analysed after

the uncompounded base *Lug*. It must be admitted that the presentation of the material in Uhlich's book is not very clear in this particular case. One can get the impression that the two variants *Luicrid* and *Lucraid* stand side by side in the early Irish sources. A search for the relevant attestations, however, reveals this not to be the case at all. Jürgen Uhlich informs me that early historical texts have only spellings that directly express non-palatalised *Lucraid*, or which are compatible with such a form (see the relevant entries in *Corpus Genealogiarum Hiberniae*, *Annals of Ulster* 753), whereas 'palatalised' *Luicrid* is found only in two late manuscripts (Annals of the Four Masters 748; *Martyrology of Donegal*) where it can be due to mechanical palatalisation in the process of copying (cp. §30 of Uhlich's book). The teleology of deriving *Luicrid* from *$lu\gamma ux^w ri\theta$- is therefore faulty as such.

But even within this argument there are clear errors. In order to get from the starting point *$lu\gamma ux^w ri\theta$- to his next chronological stage *$lu\gamma \ddot{u}x^{w'}r'i\theta^{(')}$-, Isaac has to invoke "1st Palatalisation" and "phonetic fronting of *u > *$\ddot{u}$ before the new palatal consonant, Greene (1974: 134)" at the same time (99). This is patently wrong. First, it is very unlikely that a cluster like *$x^w r$ would have been affected by the first, i.e. internal, palatalisation at all, the scope of which were single consonants and the groups *nd*, *mb*, *ng*. Secondly, and much more seriously, in Isaac's formulation of the involved sound changes, the two changes, first palatalisation and fronting of *u > *$\ddot{u}$, condition one another in a circular manner: *u becomes *$\ddot{u}$ because it is in contact with a palatalised consonant, which in turn becomes palatalised because it stands after front *$\ddot{u}$! An easy way out of the dilemma would have been to use McCone's (1996: 119) formulation of the fronting rule whereby *u became *$\ddot{u}$ "before a front vowel *i/e* in the following syllable". Incidentally, this formulation is most probably incorrect in two respects. The only piece of evidence that supports the inclusion of *e in McCone's formulation of the rule is OIr. gen. *Luigdech* < putative *$lugu$-*dek*-*os* 'best of/through Lug (*uel sim.*)', cp. Ogam LUGUDEC, LUGUDECA, LUGUDECCAS etc. But nothing prevents us from regarding the *E* of the Ogam forms as reflecting lowered *i, the name then continuing *$lugu$-*dik*- 'pointing out Lug (*uel sim.*)' (see Ziegler 1994: 198). However, even the formulation "*u > *$\ddot{u}$ before *i in the following syllable" cannot account for the evidence. As Aaron Griffith informs me, by this formulation of the rule, Proto-Celtic *$\underset{\,}{u}idub\underset{\,}{i}om$ 'billhook' (for the reconstruction see Balles 1999: 10) > Proto-Irish *$\underset{\,}{u}i\delta u\beta\underset{\,}{i}\underset{\,}{i}an$ would have been expected to come out as †*fidbe* with palatalised *db*, not as the actually attested OIr. *fidbae*. The palatalisation of *β was prevented by the rule that after rounded vowels there was no palatalisation of labial or guttural consonants (McCone 1996: 116).[29] *Fidbae* proves that Greene's original formulation must be right. It can be put like this: unstressed Primitive-Irish *u > *$\ddot{u}$ > palatalising *$\breve{\imath}$ only when a palatalised consonant follows. Be that as it may, under no circumstances would palatalising *$\breve{\imath}$ be expected to develop from the second syllable of *$lu\gamma ux^w ri\theta$-; *Lucraid* is the expected and attested outcome, with resulting consequences for Isaac's extended argument about the chronology of Irish sound changes.

Summary:

As the foregoing rather critical discussion dictates, the résumé about the present book must be critical, too. Although it contains various good ideas, useful insights, and right conclusions, too much is built on wrong interpretations and shaky foundations to leave a positive echo. In view of the author's statement at the beginning that he "did not write this book" (9) (meaning that there was no original intention to write a book, but that it simply grew into one), it would have benefitted the work to create more coherence and to write it as a book.

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[29] In fact, McCone speaks of "stressed and rounded" vowels, but *fidbae* is good evidence that the constraint is valid also in unstressed position.

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