by ideas from Stalnaker. Briefly put, subjunctives are evaluated in relation to complex structures composed of multicontexts representing not only the immediately relevant prospects, but also some less relevant prospects.

Some aspects of G's formal theory may be controversial; for example, one may take issue with his intuitions on the validity of some argument form or other. The associated philosophical ideas, however, are outright provocative. So, could one accept fully the formal account while refusing at least some of the philosophical ideas connected to it? In at least some cases, the answer is probably yes. The formal theory seems to work equally well if we understand the context to be an interlocutor's subjective 'take', or if we understand it to be the 'objective' CPC. So one might want to dispose of one or the other type of context, or even introduce a third notion of context. It seems entirely possible to introduce into G's framework a revamped version of 'common ground' (a notion that G argues against), fulfilling the formal requirements on contexts. This notion could perhaps be called the 'shared take' on the context, and would consist, roughly, of all sentences that the interlocutors both take to be in both their takes on the context. Such a notion could also help to explicate the notion of 'going without saying', which is not formalized in the present account.

I would also like to air three further worries concerning the notion of 'objective context'. First, there is the risk (acknowledged by G) that the concept of reference, despite efforts to the contrary, will rear its ugly head once we try to explain the relation between the context and the 'environmental circumstances'. Second, the notion of 'reliably achieving a goal', which is crucial in the definition of objective context, is left unexplained. Third, the language relativity of the objective context begs the question: Whose language? This is a problem insofar as interlocutors have (if ever so slightly) different languages. In conversation, these differences are dealt with by continuous mutual alignment with respect to language use, regulated by feedback signals indicating understanding and acceptance, or lack thereof. But in the case of 'objective context', there seems to be no such practice to rely on. Still, the assumption of a static and perfectly shared language is pervasive in formal semantics, so perhaps it is unfair to criticize G on this point.

In the long run, it falls on G (and anyone he can convince to join him in the fray) not only to show that his theory gets everything right that the received view got wrong, but also that it gets right everything that the received view got right. Given the amount of work carried out in the received view, this is quite a momentous task. The jury will be out for a long time yet. Meanwhile, I strongly recommend that anyone with a serious interest in formal semantics (let alone conditionals) read this book.

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Historical linguistics: Theory and method. By MARK HALE. Oxford: Blackwell, 2007. Pp. xiv, 269. ISBN 0631199917. $89.95 (Hb).

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Mark Hale's long awaited book does not disappoint. This provocative work confronts, and forces the thoughtful reader to confront, the nature of historical linguistics as a scholarly enterprise and its relationship to fundamental issues of language change in the light of current linguistic theory. Following a brief introduction, the heart of the book consists of five parts. Part 1 presents necessary preliminaries: H's definition of 'language', the relationship of 'texts' and 'languages', and what is meant by a 'descent relationship'. Parts 2–4 treat in depth phonological change, syntactic change, and reconstruction methodology, respectively. Part 5 offers brief concluding remarks.

Two dominant themes run throughout. First, H is firmly convinced through long personal experience of the basic efficacy of the traditional methods of historical linguistics and of the essential validity of their results (xi). He is equally persuaded that the only true object of linguistic analysis is the linguistic competence of the individual speaker, 'I(nternalized)-language' (7–9). One overarching goal of the book is to try to show how the techniques of traditional historical linguistics can (with some revision and elaboration) be grounded in an empirically and theoretically adequate model of language transmission. Second, H argues at length that previous attempts to eliminate the perceived disconnect between traditional conceptions of language change and modern synchronic theory have largely failed because they have been methodologically incoherent.

As H himself warns in the introduction, this book is decidedly not suitable as a textbook for a standard introductory course in historical linguistics (in my view not even at the graduate level, if used alone). Not only are many of the ideas presented unabashedly controversial, but H also assumes familiarity with the basic concepts and techniques of historical linguistics. Nor can the ingratiatingly straightforward prose style change the fact that the text deals with large, complex, and at times unavoidably technical topics. Parts of it are far from an 'easy read' (as an experienced historical linguist I found sections 10.3–10.4 on the intricacies of divergence, diffusion, and subgrouping rather heavy going). What H has given us is the ideal text for a graduate-level course that focuses on the 'big-picture' issues of language change (as I myself intend to use it) or for a follow-up course to a traditional introduction to historical linguistics.

Outstanding strengths of the book begin with the refreshingly lucid and uncluttered prose. Even more crucial than the clarity of the writing is the care with which H sets forth explicitly his underlying assumptions, frames his definitions of key concepts, and structures his argumentation. Readers may reject some of his claims, because they find his arguments inadequate or feel that he has ignored crucial evidence, but they are never in doubt as to what his claims are. His view of language change expressed here is highly coherent and internally consistent: change consists in innovation in I-grammars in first language acquisition resulting from the interaction of the PLD (primary language data) to which the learner is exposed and universal grammar (UG) (12, 34, 152–53).

The case studies on Marshallese historical phonology (Ch. 5) and on clitic syntax (Ch. 9) are well integrated into the overall argumentation, and H largely strikes the right balance in giving enough details to illustrate his points without dragging readers through unnecessary complexities of interest only to specialists. H also shows how historical linguists really work, as opposed to the deceptively mechanistic presentation of typical introductions; for example, he acknowledges the near circularity between philological and linguistic analysis (26) and the interdependence of the form of posited diachronic phonological 'rules' (83).

The clarity and rigor of the argumentation is the key to H's most signal accomplishment in this book. He has been able to sweep away layers of obfuscation, muddled thinking, ill-framed questions, and pseudo-problems, to identify the real accomplishments and failures of traditional historical linguistics, and to focus attention on the true questions to be answered regarding 'language change' (more properly 'language transmission'). This is no small feat and cannot be overemphasized. Given the ambitiousness of the undertaking, it is hardly surprising if he has only partially succeeded in providing answers to these questions.

H's methodology is ruthlessly reductionist, in two senses: (i) he seeks to reduce the points at which innovations in grammars can occur to the absolute minimum required to account for the available evidence, and (ii) he favors what he terms, following Noam Chomsky, a 'Galilean' approach (51)—to make headway in analyzing a complex natural phenomenon, one must be selective about the evidence one considers. I am generally sympathetic to a reductionist strategy, since in my experience it is easier to build up a model that proves to be underdeveloped than to strip down one that turns out to be overly complex. I should also stress that this method by its very nature contributes greatly to the success praised above with which H cuts through the irrelevant and superfluous and gets to the real heart of the matter.

The difficulty is that it is far from obvious in any given case just what one should initially include and exclude from consideration. And it is not simply a matter of revising one's initial model in the light of additional evidence. Order of presentation is not only crucial in the case of PLD in first language acquisition. To cite a thought experiment of the type H himself employs: I am firmly convinced that if Hittite had been known since the eighteenth century, while Sanskrit and ancient Iranian languages had been discovered only in the early twentieth, not only would the 'classical' reconstruction of Proto-Indo-European have been very different, but so would the way the new evidence was digested and integrated into the existing model, and hence the way the latter was revised. I see little chance that the result would have matched the model that is now crystallizing based on the real order in which evidence became available.

I must therefore harbor some misgivings when H categorically denies the possibility of a single grammar in the mind of the speaker that produces multiple outputs (190–92 with fig. 8.10). Is it in fact the case that ALL available evidence regarding variation in individual speaker behavior can be accounted for by either parallel grammars or a single invariant grammar and 'postprocessor'? H contends that it can, but if he turns out to be wrong about this, will the result of his more restrictive model revised to account for the new finding match what we would have obtained had we taken a different starting point? This is not a minor issue, since his rejection of a competence grammar with built-in variation is crucial to the 'fire-wall' he erects between innovation in individual speaker grammars and diffusion between speakers (39–40, 170–71) and the equally firm rejection of any ontological status for 'E(xternalized)-language' (9 and elsewhere). I suspect that some who are better versed than I am in phonological or syntactic theory may have a similar reaction to H's claims regarding the very limited loci of innovation in those components.

There is one glaring and disappointing omission in H's otherwise excellent presentation. The word *analogy* occurs exactly twice (127, 129) in references to works of others. In a 269-page book titled *Historical linguistics: Theory and method* the failure to discuss one of the most famous and problematic concepts of traditional historical linguistics is simply unacceptable. At the very least he owes readers some justification for the omission and a reference to where his views on the topic may be found. H's disclaimer (x, n. 4) is not remotely credible.[1] Those of us familiar with his and his colleague Charles Reiss's work know very well why analogy does not appear in H's book on language change. Other readers deserve to be told. H does not hesitate to refer to Hale & Reiss 2008 (53, n. 3). A single line would have sufficed to refer readers to Ch. 9 of that work. More immediately useful, perhaps, would have been a reference to Reiss 1997 and the indication that the thrust of that article is further developed in the jointly authored volume.

That H may feel he cannot offer a full explanation of the morphological changes traditionally ascribed to analogy is likewise no valid excuse for not mentioning the problem at all. H himself intimates that his account of the regularity of sound change does not cover everything (144). Likewise, his Ch. 9 on the diachrony of clitics offers a truly masterful account of the synchrony of the data discussed and an elegant (reductionist) explanation for how clitic typology arises, but he does not actually discuss how one synchronic clitic system evolves into another. No one would reasonably expect him to dispose neatly of all issues regarding analogy in language change either.

H largely succeeds in his effort to provide a theoretical grounding for the basic validity of the Neogrammarian view of regular sound change and of the results of the comparative method. Equally importantly, as H himself notes (xiii), he shows how a theoretically grounded view of

[1] And while one may not fully share H's pessimism regarding description of semantic change, he does in fact later offer the reader a brief explanation for its general absence (160).

language change can explain the failures as well as the successes of traditional methods and also guide research by pointing the way to the aspects of language change that give the most immediate promise of further progress. I do not entirely share his confidence that merely acknowledging that protolanguages are 'sets of [I-]grammars which are nondistinct [from each other] in their recoverable features' requires no significant changes in the day-to-day working methods of those of us who do language reconstruction (253).

One reason is that Indo-Europeanists at least base their reconstructions on data organized in terms of sociopolitically defined E-languages that are not remotely close to the I-language that according to H is the basis of true grammar change (9). And the rare cases where we have somewhat richer data (e.g. Ancient Greek dialects) confirm that the notion of 'the Greek language' or 'the Latin language' is as unreal as he argues. We nevertheless continue to reconstruct PIE on such a questionable basis (see Garrett 2006). Perhaps I am not properly conceptualizing the problem, but I would like to have seen a more explicit treatment of this issue.

As typically practiced, the comparative method also primarily models historical RELATIONSHIPS between languages (in structural terms), not historical events. Relative chronologies are, for example, a staple of traditional historical linguistics, but any working historical linguist knows that manipulating the form of posited phonological changes alters the relative chronology (or vice versa). When we then debate the relative merits of competing formulations (typically in terms of 'economy'), just what is it that we are modeling? In arguing against the 'majority rules' principle in comparative reconstruction, H concludes in his hypothetical case that we should admit that we cannot make a principled choice between **l* and **r* (242). I must reluctantly agree, but it goes against the grain. I suspect that taking H's conception of language change seriously precludes us traditional practitioners from continuing mere 'business as usual' and requires at minimum far more frequent admissions of indeterminacy.

Limitations of space and in my own expertise have prevented me from discussing many of the exciting and thought-provoking ideas in this very rich book. Along with H (260–61), I can only hope that this book incites others, of both traditional and theoretical orientation (especially students), to join in the effort to make progress in answering the many vital and fascinating questions about language change.

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The handbook of historical linguistics. Ed. by BRIAN D. JOSEPH and RICHARD D. JANDA. Oxford: Blackwell, 2003. Pp. xviii, 881. ISBN 9781405127479. $64.95.

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This handbook appeared as the twelfth volume in the valuable 'Blackwell handbooks in linguistics' series, which is still growing and which since the late 1990s has offered us insight into the state of the art of a wide range of subdisciplines within linguistics. It differs from the others in one significant way (apart from having a complete bibliography at the end rather than separate ones per chapter, which I think is more useful and saves space): it has a very long and detailed introduction covering as much as one quarter of the complete text (including thirty pages of