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Philological coda. Noise: an appreciation

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Plainly, the effort to apply to historical language study the insights to be derived from synchronic linguistic analysis is fraught with difficulties. The problem is usually conceptualized – as it is by several of the contributors to the present collection of studies – as a difficult marriage of disciplined methods to obstreperous data, a mismatched union somehow to be mediated by the Uniformitarian Principle. To understand the issues properly, then, it would seem a prerequisite to be able to identify what, exactly, the Uniformitarian Principle is. Yet that question itself has no simple answer, in part because the question can be interpreted in at least two ways, both of them bearing directly upon the aims of this collection.

One meaning of the question is this: how is the Uniformitarian Principle to be formulated? Elizabeth Traugott (this volume) calls attention to two rather different ways of conceptualizing the principle, one as a statement about the 'likelihood' of 'roughly' identical 'linguistic state[s] of affairs (structure, inventory, process, etc.)' over time (Lass 1997: 29), the other as a judgment that '[t]he linguistic processes taking place around us are the same as those that have operated to produce the historical record' (Labov 1972: 101). One respect in which the two formulations differ thus pertains to the nature of the comparanda, which may be either statal or processual. Both types of historical comparison are to be found among the contributions to the present collection, for example those by Petré and Traugott, respectively. But the two formulations of the principle also differ in respect to the degree of interpretive flexibility they appear to license: the former is a statement about probabilities, the latter is not. The distinction is of the first importance, and for more than one reason. One is that a statement about probabilities inevitably plays a fairly plainly identifiable role in empirical studies, whereas the proper empirical function of the Uniformitarian Principle will be rather more difficult to identify if it is unrelated to probabilism, as discussed below. Another reason is that the two formulations are not and could not be equally specific: considerations of probability naturally entail a higher degree of subjectivity. If the Uniformitarian Principle really ought to be regarded as a statement about probabilities, it will by necessity be less robust a tool, its formulation demanding a higher degree of vagueness – as should be evident in the two formulations given. That is to say, it must be qualified with the vague stipulation *ceteris paribus*. It should be said at the outset that such qualification of the principle does seem necessary, as acknowledged repeatedly between these covers when it is pointed out that present and past social conditions as the context for language use may not be entirely comparable (so, e.g., De Smet & Van de Velde, this volume), and as demonstrated elegantly by

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Bergs (2012) in a study highlighting the possibility of anachronistic thinking as a result of assuming homogeneous social factors across time. A formulation of the principle that does not overgeneralize will thus require acknowledgment that whereas some factors contributing to language use must be regarded as invariable over recorded time, others may afford only inexact comparisons.

Other formulations of the Uniformitarian Principle are of course possible. Winters (2010: 26), for example, since her concern is historical cognitive linguistics, sees the relevant aspect of uniformitarianism as pertaining to the way humans categorize. Yet even if the Uniformitarian Principle could be rigorously defined, there would remain the other uncertainty inherent in the question about what, exactly, the Uniformitarian Principle is: what kind of entity is it? That is to say, what is its proper role in historical linguistic research and argumentation? Is it an axiom? Or is it a hypothesis? Or is it something else? It cannot be an axiom in the sense of the word *ἀξίωμα* employed in classical philosophy, as a proposition accepted without demonstration, given the points just raised about necessary vagueness of formulation and the danger of overgeneralization. More plausibly it might be called an axiom in the sense of the word employed by modern logicians, as an initial premise, a purely formal statement rather than a fact. But an axiom in that sense is a tool of deductive reasoning, whereas historical linguistics, as a social science, requires induction.[1]

The basis for induction of course is the formation and testing of hypotheses. Might the Uniformitarian Principle, then, despite its name, be called a hypothesis? Several of the contributions to this collection do in fact treat it as such: the aim of Pentrel (this volume), for example, is to offer evidence either to support or to disconfirm the principle, checking 'whether processing principles discussed for PdE temporal adverbial clauses are also valid for historical texts, providing evidence for language specific diachronic universality in terms of the uniformitarian principle', and Petré (this volume) even calls the principle a hypothesis. Yet there are compelling reasons not to regard it as such. If it is a hypothesis, it is an extraordinarily feeble one. As pointed out above, so many allowances must be made for differing social conditions across time that it is hard to imagine a realistic test sufficiently unambiguous in design that the hypothesis could fail decisively. There is also the consideration to be taken into account that more than one factor may play a role in determining the course of a given instance of language change, and when the data do not support the hypothesis that a factor known to influence change in the present day was also an influence on a given earlier change, the Uniformitarian Principle may be acquitted on the basis of the supposition that other contributing factors played a greater role, for reasons that cannot be determined with assurance at this historical remove. This is not very different from what, for instance, Pentrel concludes about the conditioning of the position of temporal adverbial clauses in Samuel Pepys's diary. More than one of the contributions to this collection in fact finds only partial confirmation of expectations formed on the basis

[1] For an accessible account of reasoning in the sciences, including discussion of deduction, induction, and Occam's razor, see Gauch (2012).

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of the Uniformitarian Principle, yet none of them asserts (nor should it assert) that the principle is thereby, wholly or in part, invalidated. It should be apparent, then, that if the Uniformitarian Principle is a hypothesis, it is unfalsifiable, and therefore as a hypothesis it is unscientific. It must play a role in the conduct of diachronic linguistic research other than that of a hypothesis. From this conclusion there follows a principle for the application of causes of language variation derived from synchronic studies to the analysis of historical change, as in (1):

(1) It should not be the aim of a study in historical linguistics on the basis of analyzed data either to confirm or to disconfirm the Uniformitarian Principle.

What roles are left for the principle to play? An apt comparison is drawn by De Smet & Van de Velde (this volume) to Occam's (or Ockham's) razor, or the principle (or law) of parsimony. The principle is formulated in various ways, perhaps the commonest being *Entia non sunt multiplicanda praeter necessitatem* 'Entities are not to be multiplied beyond necessity'. That is to say, the simpler of two hypotheses is to be preferred, provided they are of equal explanatory power. The Uniformitarian Principle is thus a less general version of Occam's razor, since it also prescribes parsimony, forbidding the needless multiplication of linguistic entities (including processes) beyond those recognizable in present-day languages. The razor is commonly understood to be a device for distinguishing and evaluating competing hypotheses; hence the name 'razor'. Like the Uniformitarian Principle, it is commonly referred to as a principle (not a hypothesis), and yet despite the similarity, the Uniformitarian Principle does not appear to be regarded by anyone as having the function of refereeing a choice between or among competing hypotheses. The seeming discrepancy has a straightforward explanation: in science Occam's razor is not used as a diagnostic of end results but as a heuristic – that is, as a preliminary step, a practical technique for choosing which hypothesis to test. This is also more or less how the Uniformitarian Principle is used in the contributions to this collective effort to apply cognitive linguistics to historical change in English: cognitive linguistic factors recognized in modern languages (e.g. event order/iconicity, compositionality, information structure) are selected for testing as possible phenomena influencing change in the historical record. That is to say, the search for explanations does not begin randomly but is constrained by the heuristic technique of testing known variables rather than positing new ones. Hence, a second proposition about the Uniformitarian Principle may be formulated, as in (2):

(2) The proper role of the Uniformitarian Principle in historical linguistics is as a heuristic.

It was said at the outset of this discussion that the difficult problem of how to apply insights derived from synchronic studies to data from earlier stages of the language is usually conceived as somehow mediated by the Uniformitarian Principle. If the proposition offered in (2) is not mistaken, the Uniformitarian Principle has nothing to do with that problem. The problem arises not from any questionable assumptions about linguistic differences between the past and the present but from the nature of the

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data available, as a consequence of the media in which the older data are preserved and the social practices attending their transmission.

The features that complicate the interpretation of historical data are myriad. Labov (1994: 11) mentions 'hypercorrection, dialect mixture, and scribal error'. Others include mismatches between orthographic systems and phonemic inventories, non-indication of prosodic features, anonymity of sources, linguistic limitations imposed by different media and genres, artificial spelling conventions, and the inability to query informants. Before the rise of linguistic structuralism and the concomitant turn to synchronic analysis as the dominant mode of linguistic inquiry, elaborate methods were devised to tease linguistic insights from records of the past. It was recognized that the analysis of older records demanded of researchers familiarity with remarkably diverse kinds of technical expertise in order to derive the most useful information from historical texts – kinds, that is, many of which would not now be regarded as 'linguistic' in the narrow sense of the word. Examples include 'the study of manuscripts (including palaeography, codicology, the study of how manuscripts are related to one another [i.e. stemmatics], and scribal practices), orthographic systems (including orthoepy), poetic metre, rhyme, translators' practices, and numismatics, among other concerns,' and collectively such may be described as 'the extralinguistic contexts of [historical] linguistic data' (Fulk 2016a: 95). The relation of such contexts to data is the proper concern of what may be called, in the context of historical linguistics, philology.

For historical reasons, philological principles also underlie the editing of texts. By philological means it is possible, for example, to decipher unknown languages, to reconstruct authorial versions of texts corrupted in transmission, to assign dates to manuscripts, to identify the meaning of unknown words, to determine the date of composition of anonymous texts, and other editorial tasks. Editorial philology thus plays an overt role in the construction of historical corpora when, for example, manuscript features are encoded in a corpus (on which see Fulk 2016a: 103–7). But philology more generally plays a pervasive, if usually covert, role in every study in historical linguistics, since, almost without exception, such studies rely on edited texts (either directly or as inclusions in corpora), and they interpret the linguistic components of texts on the basis of tools compiled by philological methods, tools such as grammars and dictionaries, which in turn rely on the work of textual editors, who in their turn rely on multiple philological resources: see Fulk (2016b: 21–4) for discussion.

Given the pervasiveness of the philological underpinnings of historical language study, it may seem a hopeless task to remove philological concerns entirely from any approach to language history. Several of the studies in this journal issue nonetheless do an impressive job of tailoring their approach in order to exclude the subjectivities and unknown factors that philology is designed to deal with. It is indeed a remarkable accomplishment to design tests that will elicit objective results from a body of historical artifacts so resistant to non-subjective evaluation. It needs to be borne in mind, however, that enforcing objectivity can have the result of limiting severely

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what sort of information can be drawn from the historical record. When Pentrel, for example, sets out to measure the roles of event order/iconicity, adverbial length and implicit meaning in the linear ordering of temporal clauses, it might be wondered (as the present writer wondered) why no mention is made of what would seem an even more salient factor, information structure; only belatedly is it acknowledged that information structure proved too difficult to specify objectively in a diary of the seventeenth century. The careful structuring of the test applied is perhaps most rigorous of all in the study by De Smet & Van de Velde, and yet to achieve such results the period studied had to be limited to what appears (to the eye of an Anglo-Saxonist and an Indo-Europeanist, at least) the very recent past, 1950–2005. The point is not to detract from such studies or to devalue their findings, but to acknowledge what must be obvious: more impressive than the insights they provide into language history is their ingenious design, enabling them to produce any objective results at all from material of this nature. Historical records do not like being interrogated in this fashion, and they will resist to the bitter end. Historical records prefer inductive methods.

If, then, bringing insights from synchronic studies to bear on historical material entails limiting the kinds of phenomena that can be distinguished and narrowing the historical range of inquiry, in some cases effectively setting much of the historical component of historical linguistics off limits, the benefits of doing so ought to be made explicit. What do these procedures contribute to the higher aim of historical linguistics? And what is that aim? We have seen that it cannot have anything to do with gauging the validity of the Uniformitarian Principle. In recognition of this, and in a canny effort at offering a more compelling rationale, at least one of these studies suggests that the higher purpose of historical linguistics – or at least of applying synchronic findings to the historical study of language – is to validate the findings of synchronic studies.[2] There are reasons to doubt this. Practical experience suggests that the non-historical linguist who requires the validation of historical support is at least a *rara avis*, if not a *monstrum*. And probably most historical linguists would not care to regard the highest aim of their discipline as purely ancillary. Rather, rationales similar to that in (3) are what are usually offered in the literature:[3]

(3) The aim of historical linguistics is to identify and account for language change.

This is not an unreasonable assessment, but it does raise a particular difficulty. It must be conceded that synchronic studies are better at identifying the nature and causes

[2] '[T]he historical sciences have something that is unique and envied by all the others: real-time data. After all, if we really want to know whether theories based on synchronic evidence have universal application, we have to confront real-time data' (De Smet & Van de Velde, this volume).

[3] Some typical formulations of this sort may be cited: 'Historical linguistics seeks to investigate and describe the way in which languages change or maintain their structure during the course of time' (Bynon 1977: 1). 'Historical linguistics is the branch of our discipline that studies language change' (Anderson 1989: 189). A somewhat more nuanced definition of historical linguistics: 'The study of change in individual languages and in language generally' (Matthews 1997: 162). These quotations derive from books that happen to be on the author's shelf. Seeing as these books are all from the last millennium, the author should probably do some housecleaning.

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of change than diachronic ones. There would be no point in applying the insights derived from synchronic studies to historical material if the case were otherwise, and the studies in this collection demonstrate repeatedly that historical corpora resist yielding all the same insights about change in English that can be derived from the study of variation in the present-day language. The question then arises whether historical studies are necessary at all. If no need can be identified that is not satisfied by non-historical approaches, there must be yet a broader aim to historical linguistics than the one stated in (3) if the study of language history is to be regarded as an enterprise of worth. An alternative to (3) is offered in (4):

(4) The aim of historical linguistics is to interpret the past from a linguistic perspective.

(4) is at once both more specific and more general than (3). It is more specific in that in (3) historical linguistics appears to take language in the abstract as its purview, whereas implicit in the idea of interpreting the past is the acknowledgment that most studies in historical linguistics pertain to the history of a specific language or language group. In perhaps all other respects (4) is more general than (3), since it includes the possibility of (3) as an aim. A notable feature is that it emphasizes the historical nature of the enterprise. By now it should be plain that any valid statement of the aims of the discipline ought to acknowledge that the concerns of philology – which, being contextual, are historical concerns – are the concerns of historical linguistics. Reasons were offered above why even studies that aim to eliminate all the factors with which philology is chiefly concerned are nonetheless ineluctably built on a foundation of philological preliminaries.

This last point highlights a certain distorting effect in the rhetoric of the so-called bad data problem. The phenomena that are sometimes referred to in this volume as 'discouraging' and 'bad data' and 'noise' are the very aspects of historical records that are of most interest in the pursuit of philology. In philological perspective they are thus good data. It may be tempting to view this point of disagreement as a simple, binary opposition between philology and the rest of historical linguistics, but some of the contributions to this very volume reveal that to be a false dichotomy. The study by Cole is the aptest demonstration that in applying cognitive concepts to the history of English, philological concerns may play a vital role, since hers is both a quantitative and a qualitative study of Old English pronoun reference that takes into account 'a crucial interaction between the cognitive constraints and issues pertaining to style, such as the author–writer relationship, text type, subject matter and the conventionalism propagated by text tradition'. Likewise, the study by Hoffmann, though quantitative, makes deft use of contextual considerations (fidelity to the Latin source, grammatical training of the author) to explain exceptions to the expected ordering of clauses in Middle English.[4] These studies show with particular clarity that philological concerns

[4] In complementary fashion, studies of philological problems not uncommonly incorporate insights from linguistic theory. Perhaps the plainest examples of this are to be found in studies of alliterative and other meters in English: examples are Minkova 2003, Cable 2016 and Russom 2017.

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need not be an impediment to linguistic analysis, but, to the contrary, they can be a useful explanatory tool even in quantitative studies. Accordingly, it may not be unreasonable to suggest that the rhetoric of 'bad data' is overblown. At the least, it reveals a parochial way of viewing the field, considering that probably the majority of historical linguists, like Cole and Hoffmann, do make productive use of those aspects of the historical record that differentiate it from language in current use. As De Smet & Van de Velde remark, 'our interest in studying the past lies precisely in the fact that the past was different'. Most historical linguists, then, probably would not like to see diachronic linguistics remade in the image of synchronic, or to see their historical interests lamented as 'noise'. From these observations (and in acknowledgment of the chiefly visual rather than aural character of historical linguistics) there follows a final, general historical linguistic dictum (5):

(5) Watch your language.

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