

**Antoine Meillet and the Comparative Method: On Shared Aberrancies as Evidence of Genetic Relatedness**

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Any method for establishing genetic relatedness by identifying similar constructions in different languages must provide a specification of similarity. As long as the identification of regular sound correspondences is our measure of similarity in linguistic form, there can be no avoiding appeal to sound correspondences in genetic arguments. This is true even for the type of evidence termed *shared aberrancy*. If early comparative historical linguists thought otherwise, as is often suggested, then by modern standards their proofs are invalid. Yet I argue that many such claims regarding early 19th comparativist practice involve a distorted and selective view of the history of linguistics. I also argue that claims regarding Meillet's supposed privileging of shared aberrancy over sound correspondence in the early 20th century result from a misinterpretation of his notion *fait particulier* 'singular fact'.\*

## 1. Attacks on the *Standard Comparative Method*

It is, I think, no exaggeration to claim that, for generations of comparative historical linguists over the past century or more, there has been reasonable consensus regarding what the comparative method is and how to use it to demonstrate that languages are, or are not, likely to be genetically related. That consensus, which I term the *standard comparative method*,<sup>1</sup> centers on the identification of cognates by identifying regular correspondences in the (phonological) form of constructions<sup>2</sup> with similar meanings. It is this standard comparative method that is set out in the manuals and textbooks from which most of us would have been trained, from the historical section of Bloomfield's (1933) *Language* through to relevant chapters of the *Handbook of Historical Linguistics* (Joseph and Janda 2003).<sup>3</sup>

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\* This paper grew out of a talk entitled *Meillet's Magic Bullet: On the Logic of Genetic Arguments Using Morphological Evidence* written by Alan Dench and myself for the symposium *Language in Time: Language Evolution and Change*, (University of Western Australia, 24-27 June 2002). I am grateful to Alan for his collaboration in that earlier effort, and to the other participants at the symposium, particularly Lyle Campbell and William Labov, for their critical comment. Alan Dench, Mark Ellison, John Henderson, Luisa Miceli, Marie-Eve Ritz, and the usual anonymous readers provided me with much advice and comment of various sorts as the present paper was taking shape. The shortcomings that remain are entirely my own.

1 Nichols (1992:3, 311) also uses the term standard comparative method, but in a sense rather different from mine. Her views are discussed at length in sections 5.1 and 6 below.

2 I use the term *construction* as in constructionist approaches to grammar, to subsume both word and morpheme (and possibly more complex sequences). The constructions at issue will all be at least in part substantive, rather than fully schematic.

3 This standard comparative method is often associated with the Neogrammarian movement of the last quarter of the 19th century. As Poser and Campbell (1992:219) note, that view of history risks masking the fact that the importance of regular, consistent sound correspondences, as opposed to isolated correspondences restricted to a handful of cases, had been recognised throughout the 19th century. In 1818 Rask wrote: "When...one finds agreements between two languages, and that to such an extent that one can draw up rules for the transition of letters from one to the other, then there is an original relationship between these languages;..." (Lehmann 1967:32) What distinguished the Neogrammarians was their insistence that sound changes, and the correspondences consequent thereon, are not just frequent, but exceptionless. The doctrine of exceptionless sound laws did not appear out of nowhere in the 1870's, but was the elevation of a heuristic of long standing to the status of a theoretical principle. That said, in the discussion to follow I will use the term *regular* to refer to Neogrammarian sound changes and sound correspondences.

In recent years that consensus has come under attack on two fronts. The first, from proponents of the inspectional mass (or multilateral) comparison as employed in Greenberg (1987) for example, has been widely debated both amongst comparative historical linguists and in the wider academic community. The second has gone largely unremarked, probably because it has not been perceived as particularly contentious. It is perhaps more dangerous for that reason. I refer to the increasingly frequent assertion that the identification of *shared aberrancies*, particularly in paradigmatically related forms, is either a (possibly preferred) alternative to the standard comparative method or even the sole legitimate determinant of genetic relatedness. The former view is often ascribed to Meillet (as, for example, in Campbell (2003:268ff) and Poser and Campbell (1992:215)) by virtue of an identification of Meillet's notion *fait particulier* 'singular fact' with *shared aberrancy*. The latter view is one that assigns to the standard comparative method only a subsidiary role in subgrouping languages whose genetic relatedness has already been determined through the identification of shared aberrancies. So far as I am aware, it has only been asserted in Nichols (1996), but is no less insidious for that fact, since, again to the best of my knowledge, it has gone unquestioned.<sup>4</sup>

The acceptability of mass comparison and the primacy of shared aberrancy in determining genetic relatedness are of course very different claims. Yet I think both fail for the same reason.<sup>5</sup> Neither those who use mass comparison nor those who currently argue that the identification of shared aberrancies is an alternative to finding sound correspondences provide a specification of what *similar* means in comparing constructions in two languages. The standard comparative method, by contrast, does provide that specification.

The logic of mass comparison has been convincingly debunked in a torrent of critical papers, of which Matisoff (1990) is a particularly good exemplar. There is no need for me to add to the volume of that literature here.<sup>6</sup> My purpose here is rather to defend the standard comparative method as the fundamental tool by which comparative historical linguists determine genetic relatedness against any claim that it has a secondary role, as

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4 In their introduction (Durie and Ross 1996:3-38) to the volume in which Nichols' paper appears, the editors provide a positive evaluation of her claims and do not appear to regard them as contentious. I have found seventeen other citations of that paper, excluding those by the author herself, by thirteen different authors. Only in Gensler (1999:609) have I found any skepticism towards her claims and methods, and there only with regard to the issue of identifying borrowings, a problem Nichols herself acknowledges.

5 It is perhaps interesting to note, particularly in light of the early history of interest in grammatical evidence in comparative linguistics (as described in section 5.2 below), that recent proponents of mass comparison like Greenberg and of morphological evidence in genetic arguments, like Nichols, are both well-known typologists whose research has focussed on identifying and counting patterns of relatively superficial similarity in large numbers of languages, rather than in writing detailed grammars of individual languages or providing detailed cross-linguistic comparison of individual grammatical phenomena. The relation between Greenberg's typological interests and mass comparison are reasonably transparent. That between Nichols' typological measures of diversity and her interest in shared aberrancies as genetic evidence is rather more tenuous, except that both involve quantitative, statistical evaluation.

6 For a recent short overview of the issues, see McMahon and McMahon (2005:19-26).

compared with the identification of shared aberrancies. The essence of my defense is that the very identification of shared aberrancies depends on the standard comparative method.

To mount that defense, two issues must be clarified. First, to set the ground rules, it is necessary to specify the criteria that any successful genetic linguistic argument must meet. I undertake that specification in section 2 and, at the same time, show in outline form how the standard comparative method addresses those criteria. Second, it is necessary to delimit the range of evidence that has been identified as shared aberrancy in genetic arguments. In that regard, section 3 draws a contrast between *shared aberrancies* and *morphological similarities*. That distinction is useful despite the fact that the two cases overlap, because their value as genetic evidence rests on different premises. Shared aberrancies have been valued because they are regarded as patterns of similarity so unusual that they are unlikely to have arisen by chance. Morphological similarities have been valued largely in the belief that morphology is unlikely to be borrowed. I postpone consideration of shared aberrancy as a filter for chance resemblances till section 6, in the context of a discussion of Nichols' (1996) statistical interpretation of that relation. The use of morphological evidence as a filter for borrowings is discussed at some length in sections 4.2-4.4, in relation to Meillet's views on the matter.

Much recent discussion of the primacy of shared aberrancy as genetic evidence takes the form of appeals to authority. Those appeals are of two types. The first involves claims about how early comparative linguists, particularly in the first half of the 19th century, actually worked. Arguments of this sort assert that the evidence then used to establish families like Indo-European or Semitic came either from shared aberrancies or morphological similarities in general. The second involves appeal to major figures in the history of comparative historical linguistics, supported by numerous quotes on the subject of genetic evidence from their published work. The person most often cited is Antoine Meillet.

Antoine Meillet is widely regarded as the pre-eminent Indo-Europeanist of the first half of the 20th century and as the major theoretician of comparative historical linguistics in that period, and perhaps in the entire history of the field. So his views on the subject of grammatical evidence in genetic arguments should carry some weight, and are discussed at length in section 4. Section 4.1 considers relevant aspects of Meillet's views on the nature of language, in particular, on the contrast between *grammar* and *vocabulary*. In section 4.2, I show that Meillet's concerns regarding evidence in genetic arguments were no different from our own; he advised against accepting superficial resemblances as genetic evidence and was particularly sensitive to the possibility that borrowings be misidentified as cognates. If he valued cognates between grammatical morphemes over lexical, it was because he believed the former resistant to borrowing. In view of Meillet's

concerns, section 4.3 considers the problem of borrowability in more detail, contrasting those circumstances under which standard comparative method can and cannot identify borrowings. Meillet's views on grammatical change and on the use of typological evidence in genetic arguments are examined briefly in section 4.4.

In my view, Meillet was essentially a good Neogrammarian, who maintained the necessity of identifying sound correspondences in any putative cognate set. In particular, I will argue in section 4.5 that the identification so often made between Meillet's notion *fait particulier* and *shared aberrancy* is largely spurious, and based on a misinterpretation of Meillet's views. I demonstrate that for Meillet a singular fact was not an aberrancy or an irregularity but any set of arbitrary pairings of form and meaning relatable through regular sound correspondences.

As an argument form, appeal to authority is never valid, though one might initially be disposed to give such arguments a status proportional to the status of the figure to whom appeal is made. But even the most respected figures can be wrong; in the end, the arguments of both the famous and the not-so-famous must rest on their merits as *arguments*. In section 5, I will argue that the claims often made regarding 19th century practice in fact betray a rather selective view of the history of comparative study in that period, and quite possibly an anachronistic interpretation of the nature of early 19th century interest in morphology.

## **2. The Comparative Method and Genetic Arguments**

### *2.1 Goals of the Comparative Method*

Despite being labeled a *method*, and despite the consensus regarding how it works, introductory manuals in historical linguistics seldom provide anything approaching an algorithmic specification of the comparative method as a means of processing comparative data to yield a determination of genetic relatedness. It is more often presented through classic or typical examples of its use, or in terms of those postulates about the nature of language and language change on which it is grounded. The comparative method is itself often localized historically, as the product largely of 19th century European scholarly activity regarding language relatedness and change. One might almost characterize the comparative method as just what linguists do and believe, in order to satisfy the goals of comparative historical linguistics.

At least those goals are fairly straightforward:

- i. to determine which sets from amongst the world's languages are genetically related
- ii. for those sets of languages that prove to be genetically related, to reconstruct whatever aspects of the proto-language comparison has revealed

In principle, one can make determinations of genetic relatedness without reconstructing

anything at all, though I would probably want to argue that reconstruction is always implicit in any satisfying claim of genetic relatedness, whether one actually presents the starred forms or not. Meillet is firm regarding the place of reconstruction in genetic arguments. He states that "the proof is not complete until one has confronted morphological system with morphological system and until one has seen how it is possible to pass from the initial system to the later systems." (Meillet 1925:44) That said, the mechanics of reconstruction are largely irrelevant to the issues of concern here.

Two languages are genetically related, by definition, if they share a period of common history,<sup>7</sup> a period in which they were a single language, where *single language* is understood in terms of notions like *mutual intelligibility* and *speech community*. If the number of languages compared is greater than two, *subgrouping* issues arise; do Hebrew and Aramaic, for example, share a period of common history distinct from Arabic, though all three are genetically related? The evidence necessary to demonstrate subgrouping is different from that necessary to demonstrate genetic relatedness, as all comparative historical linguists appreciate. To demonstrate genetic relatedness, one seeks evidence of the shared *retention* of constructions from the period of history common to all the languages compared. To demonstrate subgrouping, one seeks evidence of the shared *innovation* of constructions *after* that period of common history.

What is often not appreciated is the extent to which these are different activities. To demonstrate the genetic relatedness of a set of languages one must only identify similarities attributable to a period of common history. The subgrouping problem is usually conceived as a tree-selection problem, but the only tree implicit in a claim of genetic relatedness is a default one, with a single branching node. Crucially, the two activities are ordered. In order to demonstrate that similar constructions reflect a shared innovation, one must first eliminate the possibility that those constructions reflect a shared retention from a yet earlier period of common history. So one must already have identified the shared retentions, and thus demonstrated genetic relatedness, before subgrouping can begin.<sup>8</sup> These considerations will be shown below to bear on claims about the relative value of the different sorts of comparative evidence we are considering.

## 2.2 *The Structure of Genetic Arguments*

The determination of genetic relatedness proceeds in two steps. The first is to identify

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7 The concept *genetic relatedness* is itself not unproblematic, given that a language is not an organism passing on genetic material, but a set of behaviours and the knowledge underlying them. It is those behaviours that are copied and that are subject to change. Conceptual problems arise when one attempts to define *common history* in terms of the individual constructions that constitute a language, because one can just as easily characterise the relationship between a borrowing and its source as common history as that between two constructions descended from some ancestral construction in the usually intended sense. As compelling as that issue might be, it can thankfully also be ignored for our present purposes.

8 I consider the nature of subgrouping arguments in more detail in Harrison 2003, §3.2.

cross-linguistic similarities. The second is to demonstrate that the most likely account of those similarities is retention from a period of common history.

### 2.2.1 Measures of Similarity

The problem of just *how* similar two constructions have to be to merit attention in genetic arguments is often sloughed off to other sub-disciplines of linguistics, as a matter for phonological theory on the one hand and semantic theory on the other. Semantic theory offers us little help however, and certainly does not provide anything approaching a measure of semantic distance in terms of semantic features or some other such construct. We are for the most part as much left to our own devices now as when Meillet observed that:

"[t]he agreement in meaning should be as exact and as precise as the agreement in phonological form (according to the rules of correspondence)." (Meillet 1925:52)

and proceeded to provide an account of the semantic relation between Latin *ovicula* (< *ovis* 'sheep') and French *ouaille* 'congregation, flock' in terms of knowledge of Christian metaphor.<sup>9</sup>

We fare better on the formal side, with phonetic and phonological theory. Yet it is seldom appreciated that, given the standard comparative method, we need not now rely, and have never had to rely on the results of phonetic and phonological theory for a measure of phonetic similarity. As I have argued elsewhere (Harrison 2003:219-20), the *regularity assumption* of the standard comparative method provides a stand-in for a measure of phonetic similarity. Given the regularity assumption, it doesn't matter how phonetically similar the elements in correspondence are, but only that the correspondence be regular. Meillet may well have recognized this fact too, when in the passage just cited he juxtaposed 'agreement in phonological form' and 'rules of correspondence'. Indeed, the regular sound correspondences discovered through the course of the 19th century undoubtedly provided much of the data on which theories of phonetic similarity were built.

Critiques of mass comparison often note that it fails to ensure that the forms compared are indeed comparable. Thus, McMahon and McMahon conclude that:

"Greenberg's mass comparison...generates a vast amount of data but the intrinsic problem is whether we can determine that in any given case we are really comparing likes with likes." (McMahon and McMahon 2005:21)

By contrast, as just observed, the standard comparative method *does* provide a check on similarity of *form* at least, through the regularity condition on correspondences. Any sort of evidence of genetic relatedness that is proffered, including that from shared aberrancies,

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9 Note that in French a flock of sheep is *un troupeau de moutons*.

must provide a similar check, as noted by Hock (1986:536):

"Shared aberrancies may be morphological/lexical, as in the English/German correspondences ... [good, better, best; gut, besser, best-]. ... Notice however that suppletion by itself is not sufficient. ... What is important is that the morphemes involved in the suppletion must be characterized by systematic recurrent correspondences. ..."

In the absence of any measure of similarity, whatever conclusions are drawn from putative similarities are immediately suspect.

### 2.2.2 Accounts of Similarity

The second step in a genetic argument is to eliminate (or at least, render most unlikely) all accounts of the observed similarities except inheritance from a period of common history. Those alternative accounts are chance, borrowing, and (the limited possibilities afforded by) the nature of language.

Natural similarities are easily eliminated if we restrict attention to similarities in constructions in which the form-meaning association is arbitrary; in effect, to non-onomatopoeic constructions that are (in constructionist terms) at least partially substantive. This condition also removes from consideration any grammatical similarities of a purely typological sort, including similar word order typology and similar paradigm structure (in terms of the identity of the parameters of the paradigm and the number and function of their values, though crucially not the substantive exponents of those values). For as much as he stressed grammatical similarities (see §4.1 below), Meillet was careful to exclude such typological considerations. He wrote that:

"...it is necessary to distinguish here between the general processes and the specific details of the forms....[I]n certain languages relations between words and the various uses of words are marked by special forms which the words themselves assume, while in other languages relations and uses are marked by additional elements, by particles or accessory words, and by word order....[O]ne may not make use of these general types at all to prove a 'genetic relationship'." (Meillet 1925:36-7)

After rejecting such typological similarities as strict suffixation and vowel harmony as evidence for a genetic relationship between of Finno-Ugric and Turkic (1965:90-91), Meillet continues:

"When, in his article in *Anthropos*, VIII (1913), p. 389ff, entitled *The Determination of Linguistic Relationship*, an eminent Americanist Mr. Kroeber protested against the use of general correspondences in morphological structure in establishing the genetic relatedness of languages, he was entirely correct...Grammatical correspondences, and those alone, provide rigorous proof [of genetic relatedness], but on condition that

one appeals to the material detail of the [linguistic] forms and that one establishes that certain specific grammatical forms found in the languages under consideration have a common origin."<sup>10</sup> (Meillet 1965:91)

in which he stresses the need for substantive, and not just formal grammatical evidence.

Meillet appears to have rejected purely formal, typological evidence on the basis of what Harrison (2003:224) labels the *poverty of choice* argument. In discussing the evidence for the genetic relationship between English and French in the article just cited, Meillet states:

"In this paper we take no account of any general similarities in structure that the languages considered might show...all that does not imply a common origin because...the orderings of the words in question offer too little variation for any correspondence to be significant..."<sup>11</sup> (Meillet 1965:90)

With respect to word order, the poverty of choice argument begins with the observation that there are precisely  $n!$  possible orders in which any  $n$  linguistic objects can occur. For  $n \leq 3$ , as in the case of major constituent typology, that number is too small to rule out chance as an account of similarity. But for  $n > 3$ , the number of permutations increases quickly, and one might well begin to question the possibility of chance. Hymes (1955, 1956)<sup>12</sup> suggests that similar relative category order might be evidence of genetic relatedness for this reason. That such arguments fail in principle is not due to the temporal instability of such ordering constraints, as Meillet<sup>13</sup> and others often suggest, but because of the *iconicity* of such schematic constructions. So long as one can give an account of any relative ordering in terms of some small set of conceptual or syntactic parametric options, as current syntactic theory (and our own experience) suggests, then the fact that any two languages happen to have made the same choice is not particularly surprising. The choice falls within very tight limits that are naturally, and not arbitrarily set. As Meillet observes elsewhere:

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10 Where the translations of Meillet are my own, I footnote the French original:

"Quand, dans son article de *Anthropos*, VIII (1913), p. 389 et suiv., intitulé *The Determination of Linguistic Relationship*, un américaniste éminent, M. Kroeber, a protesté contre l'emploi des concordances générales de structure morphologique pour établir des parentés de langues, il a eu entièrement raison. ...Les concordances grammaticales prouvent, et elles seules prouvent rigoureusement, mais à condition qu'on se serve du détail matériel des formes et qu'on établisse que certaines formes grammaticales particulières employées dans les langues considérées remontent à une origine commune"

11 "Dans cet exposé on n'a tenu aucun compte des ressemblances générales de structure que peuvent présenter les langues considérées...tout cela n'implique pas une origine commune, parce que ... les ordres de mots en question n'offrent trop peu de variations possible pour que les concordances soient significatives..."

12 I am grateful to Lyle Campbell for pointing out these references to me. On other cases in which purely schematic similarities might be evidence of genetic relatedness, see note 53 below.

13 For a more detailed appreciation of Meillet's views on the use of typological evidence in genetic arguments, see Croft (2004:8ff).

"It is clear that, to establish the genetic relatedness of languages, it is necessary to abstract away from everything that is explicable by general conditions, common to the totality of languages."<sup>14</sup> (Meillet 1965:89)

The standard comparative method attempts to eliminate chance and borrowing as accounts of cross-linguistic similarity by trawling through the results of change, particularly sound change, for instances of frequent regular correspondence, on the grounds that such correspondences are unlikely to be the result of either chance or borrowing. But detecting borrowings is a particularly problematic matter. From the very beginning of modern comparative historical linguistics, scholars have been led to glaring misanalyses by their failure to distinguish borrowings.<sup>15</sup> In spite of numerous demonstrations of the capacity of the standard comparative method to identify borrowings in many circumstances, comparativists have remained wary of the errors that borrowing may induce. Many if not most of the assertions of the priority of grammatical evidence over lexical are motivated by that fear. We treat the problem of borrowing in more detail in section 4.3 below.

### 3. Shared Aberrancy and Morphological Similarity

As already observed, the term *grammatical* is applied in discussions of genetic arguments to two rather different sorts of evidence, which I will distinguish here as *shared aberrancy* and *morphological similarity*. Some discussions in the literature highlight either one or the other. In a relatively recent textbook one finds a reference to the former:

"In reconstructing morphology, some of the most valuable evidence of all, when we can find it, consists of shared anomalies -- unusual morphological idiosyncrasies common to two or more languages. These, when they turn up, constitute very powerful evidence that the languages are related and that the anomalies must have been inherited from the parent language." (Trask 1996:237)

and in a recent survey article, what is apparently a reference to the latter:

"It is generally agreed that shared morphology is the surest proof of genetic relatedness. Phonemes have no meaning in themselves and their organization into systems and their phonetic realizations are all too open to areal influences. Lexicon moreover is always open to infiltration by borrowing. Such problems are far less common with morphology and that is why it will be our chief concern here." (Hayward 2000:87)

But occasionally they are not distinguished. In introducing her notion *diagnostic linguistic*

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14 "Il va de soi que, pour établir une parenté de langues, il faut faire abstraction de tout ce qui s'explique par des conditions générales, communes à l'ensemble des langues."

15 For a catalogue of some of the more notable egregious errors of this sort in the early history of comparative historical linguistics, see Poser and Campbell 1992:26-30.

evidence, Nichols writes:

"...the evidence taken as probative of relatedness is not individual items but whole systems or subsystems with a good deal of internal paradigmaticity, ideally multiple paradigmaticity, and involving not only categories but particular shared markers for them. Such evidence may include structural categories together with their (phonologically specific) markers, or lexical categories with some of their (phonologically specific) member lexemes." (Nichols 1996:48)

The first two examples she identifies as grammatical (ibid.:50-51) are the Germanic *good~better* alternation and the Indo-European case-gender paradigm whose nominative affixes she gives as \*-s, \*-m, and \*-H. For her they are of the type because of their similar probabilities of occurrence (see section 4 below).

Since Meillet is usually regarded as authoritative on such matters, it is important to attend to his views. Let me anticipate the shape of that discussion by noting that it is very difficult to separate Meillet's views regarding the nature of grammatical evidence from his motivations in using that evidence in genetic arguments. We focus here on three strictures Meillet placed on evidence of genetic relatedness:

- i. that typological similarities not be used as evidence for genetic relatedness
- ii. that lexical borrowings not be mistaken for inherited items, and mistakenly used as evidence for genetic relatedness
- iii. that genetic relatedness not be established on the basis of vague lexical similarities, but only in accordance with the principles of the comparative method (including the demonstration of regular sound correspondence)

I will argue that Meillet's *faits particuliers* 'singular facts' are not, as is often supposed, equivalent to what we now understand by *shared aberrancies*, but encompass a much broader range of evidence for cognacy, of which shared aberrancies are only one sort. I will also argue that Meillet's emphasis on *des faits particuliers* was motivated not by any belief that we must seek shared aberrancies to demonstrate genetic relatedness, but because his particular facts were *particular* and not *general* (that is, typological).

Meillet also argued that morphological evidence is the best evidence of genetic relatedness because, for him, "[m]orphology ... is the most stable thing in language" (Meillet 1925:36). In section 4.4, I will show that Meillet did not mean that morphology is in any way resistant to change. In fact, he gives numerous instances of just such changes. Rather, I suggest that what Meillet found significant about morphology was that it is resistant to *borrowing*. For that reason Meillet believed morphological evidence to be a privileged sort of evidence for genetic relatedness, but only insofar as it meets the other requirements of the comparative method, including the demonstration of regular sound correspondences. Finally, the claim that Meillet rejected lexical evidence will be shown to

be spurious. To be sure, individual lexical items might readily be borrowed, but might also be shown convincingly to be inherited. What Meillet rightly objected to was vague evidence, not lexical evidence.

#### 4. Meillet on Grammatical Evidence in Genetic Arguments

##### 4.1 Meillet's View of Language

To evaluate Meillet's approach to morphological evidence we must first appreciate that he did not understand morphology to mean what we take it to mean today. For Meillet:

"Each language has three distinct systems which are connected with each other in definite ways, but are capable of varying to a great extent independently of each other: morphology, phonology, and vocabulary.

Morphology, that is, the set of processes by which words are modified and grouped to form sentences is the most stable thing in language." (Meillet 1925:36)

In some places Meillet contrasts *morphology* and *vocabulary*, and in others *grammar* and *vocabulary*, but it is clear that he had the same contrast in mind. That *morphology* and *grammar* are coextensive for Meillet is clear from the following:

"...[T]he little auxiliary words with the same grammatical value are not reducible to common origins: thus Eng. *the* and Fr. *le*, Eng. *of* and Fr. *de*, Eng. *to* and Fr. *à*, Eng. *we* and Fr. *nous*, Eng. *you* and Fr. *vous*, Eng. *he* and Fr. *il* have nothing in common."<sup>16</sup> (Meillet 1965:90)

and:

"What conclusively establish the continuity between one common language<sup>17</sup> and a later language are the particular processes of expression of the morphology. For example, it is not uncommon that the relation of dependence between two substantives is expressed by a particle either placed in front like French *de* or placed behind like English *-s*. But the fact that this particle has the form *de* or the form *-s* is characteristic; ... One must add that the use of *de* may disappear from a French dialect or of *-s* from an English dialect without these dialects ceasing to be French or English. Only positive facts have a conclusive value." (Meillet 1925:39-40)

in which French *de* 'of' is described first as having 'grammatical value' and then as an 'expression of the morphology'. In the second of these citations, Meillet considers the French preposition *de* and the English enclitic *-s* to be morphological constructions of the

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16 "...les petits mots accessoires de même valeur grammaticale ne sont pas réductibles à des origines communes: ainsi angl. *the* et fr. *le*, angl. *of* et fr. *de*, angl. *to* et fr. *à*, angl. *we* et fr. *nous*, angl. *you* et fr. *vous*, angl. *he* et fr. *il* n'ont rien de commun."

17 Meillet uses the term *langue commune* 'common language' where we would understand *proto-language*.

same order and function.

One might want to identify Meillet's view of morphology with the one familiar to some of us from classic American structuralism, in which morphology subsumes syntax. It is more instructive though to focus on Meillet's contrast between morphology (grammar) and vocabulary. Remarks like the following reflect Meillet's thinking on what we would now term *grammaticalization*:<sup>18</sup>

"Mr. Schuchardt says, with reason, that the distinction between lexicon and grammar is not absolute. The fact that the 2nd person singular pronoun is in Latin *tū* is a lexical fact; in current French, *tu* is no longer an independent word, it is but the marker of the 2nd person singular of verbs. From the Latin *tū*, which is an independent word, to the French *tu*, which is a grammatical element, there has been a slide, and one cannot fix the moment when *tu* stopped being a word and became a grammatical marker; there was an imperceptible transition from one value to the other...."<sup>19</sup> (Meillet 1965:107-8)

and suggest that for Meillet the morphology/vocabulary distinction was primarily a functional or semantic one, akin to the grammatical/lexical distinction drawn for example by Leonard Talmy, as in:

"Together, the grammatical elements of a sentence determine the majority of the structure of the CR [cognitive representation – SPH], while the lexical elements together contribute the majority of its content. The grammatical elements of a sentence, thus, provide a conceptual framework or, imagistically, a skeletal structure or scaffolding for the conceptual material that is lexically specified." (Talmy 2000:21)

One final matter that requires comment is Meillet's use of the term *la grammaire comparée* 'comparative grammar', as in his *Sur la Méthode de la Grammaire Comparée* (Meillet 1913 1-15). Though Meillet did contrast phonology and morphology as distinct subsystems of language, as noted above, and often used the term *morphology* interchangeably with *grammar*, it is clear that he did not regard comparative grammar as the comparison of morphological paradigms or syntactic patterns, to the exclusion of all else. Indeed rather more of the paper just cited is concerned with regular sound correspondences than with morphological or other grammatical elements. For Meillet, it seems that *grammar* entails *system*. He did not regard the lexicon as systematic in this sense (see §4.2 below), and saw synchronic phonology as a system independent of synchronic morphology.

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18 Heine (2003:576) notes that Meillet coined the term *grammaticalisation*. Meillet treats that phenomenon in some depth in a number of other papers. See, for example Meillet (1912).

19 "M. Schuchardt dit, avec raison, que la distinction entre la vocabulaire et la morphologie n'est pas absolue. Le fait que le pronom singulier de 2e personne est en latin *tū* est un fait de vocabulaire; en français actuel, *tu* n'est plus un mot autonome; ce n'est que la caractéristique de la 2e personne singulier des verbes. Du latin *tū*, qui était un mot autonome, au français *tu*, qui est un pur element grammatical, il y a eu glissement, et l'on ne peut marquer le moment où *tu* a cessé d'être un mot pour devenir un caractéristique grammaticale; il y a eu transition insensible d'une valeur à l'autre. ..."

Nonetheless, he viewed comparative grammar as the study of all that is systematic in the history of the languages compared, including regular sound correspondences.<sup>20</sup> It is perhaps this ambiguity in Meillet's use of the term *grammar* that has led some to conclude that his references to grammar, in comparative contexts, exclude sound correspondence when it is clear that they do not.

#### 4.2 Grammatical Evidence and Borrowing

As we have already observed, for Meillet "[m]orphology ... is the most stable thing in language" (1925:34), while "[a]s far as vocabulary is concerned, it is the most unstable thing in language." (op. cit.:48) What bothered Meillet most about this instability of vocabulary, in the context of genetic arguments, is the ease with which vocabulary is borrowed. This concern is clear in what are perhaps his most often referenced remarks on grammatical evidence:

"Grammatical correspondences, and those alone, provide rigorous proof [of genetic relatedness], but on condition that one appeals to material detail of the [linguistic] forms and that one establishes that certain specific grammatical forms found in the languages under consideration have a common origin. Lexical correspondence never provides absolute proof because one can never be certain that such correspondences cannot be explained as borrowings."<sup>21</sup> (Meillet 1965:91)

He makes his concerns even clearer in the lengthy discussion of borrowing in the section preceding that from which the preceding quote is drawn. Meillet says:

"Pronunciation and grammar form closed systems; all the parts of each of those systems are linked one to the other. The phonetic system and the morphological system thus lend themselves little to admit *borrowings*. In effect it is rare that one borrows from another language either a phoneme (a speech sound) or a grammatical form; ...By contrast, words do not form a system; at the very most they form small groups...each word exists so to speak in isolation. So one can borrow from foreign languages as many words as one wants...In sum, the lexicon is the domain of

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20 In a LINGUIST thread on the comparative method some years ago, Karl Teeter (1994) observed that "for Meillet the word 'morphologie' means essentially what we now call grammar, which includes syntax and phonology." I might speculate, rather gratuitously, that it was Meillet's understanding of *comparative grammar* that Teeter had in mind.

21 Note that the French original contrasts *les concordances grammaticales* with *la concordance de vocabulaire*:

"Les concordances grammaticales prouvent, et elles seules prouvent rigoureusement, mais à condition que on se serve du détail matériel des formes et qu'on établisse que certaines formes grammaticales particulières employées dans les langues considérées remontent à une origine commune. La concordance de vocabulaire ne prouvent jamais d'une manière absolue, parce qu'on ne peut jamais affirmer qu'elles n'expliquent pas par des emprunts."

This citation overlaps that of §2.2.1 above.

*borrowing*."<sup>22</sup> (Meillet 1965:84)

He then considers apparent exceptions to the claim that grammar is immune from borrowing, and argues that all such cases *begin* as lexical borrowing, the borrowing of individual words or phrases in which the grammatical element is present. He concludes:

"It happens-- to a small extent and in very special circumstances--that one borrows from a foreign language small words with grammatical value; true grammatical forms are seldom borrowed. Thus one is always led to the same conclusion: what is borrowed is essentially elements of the lexicon."<sup>23</sup> (Meillet 1965:87)

So we see that the relative weight Meillet gives to grammatical over lexical evidence in genetic arguments is not due to some general, almost mystical primacy of the grammatical over the lexical, but is a consequence of very practical considerations, his belief that the lexicon was always open to borrowing while grammatical morphemes are relatively immune to it. And, as we will demonstrate below, nowhere does he assert that proofs of the cognacy of grammatical morphemes are exempt from the necessity of showing that those morphemes are related through regular sound correspondences.

Those who have argued that Meillet rejected lexical evidence in some broader fashion take him out of context. For example, Nichols (1996:97) cites the following:

"While one can initially observe lexical similarities between two or more languages to indicate in which direction one must look, it is not from there that definitive proof comes; the vocabulary serves only to orient the research; proof comes from elsewhere."<sup>24</sup> (Meillet 1965:97)

What Nichols fails to note is that those remarks follow a discussion of spurious genetic claims based on *concordances...vagues* "vague correspondences" of the sort we might now associate with mass comparison. With a distinctly modern ring, Meillet (op. cit.:96) declares that "one cannot base a doctrine on a personal feeling."<sup>25</sup> And on the question of

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22 "La prononciation et la grammaire forment des systèmes fermés; toutes les parties de chacun de ces systèmes sont liées les unes aux autres. Le système phonétique et le système morphologique se prêtent donc peu à recevoir *des emprunts*. En fait il est rare qu'on emprunte à une autre langue soit un phonème (un son du langage), soit une forme grammaticale; ... Au contraire, les mots ne constituent pas un système; tout au plus forment-ils des petites groupes; ... chaque mot existe pour ainsi dire isolément. Aussi peut-on emprunter à des langues étrangères autant de mots que l'on veut;...En somme, le vocabulaire est le domaine de l'*emprunt*."

23 "Il arrive--en une mesure du reste assez faible et dans des situations très particulières--qu'on emprunte à une langue étrangère des petits mots de valeur grammaticale; on n'emprunte guère de vraies formes grammaticales. Ainsi l'on est toujours ramené à la même conclusion; ce qui s'emprunte, ce sont essentiellement des éléments de vocabulaire."

24 "Si donc on peut d'abord constater des ressemblances de vocabulaire entre deux ou plusieurs langues pour indiquer de quel côté il faut chercher, ce n'est pas de là que peut venir une démonstration définitive; le vocabulaire ne peut servir qu'à orienter la recherche; la preuve se trouve ailleurs."

25 "...on ne peut pas fonder une doctrine sur un sentiment individuel."

borrowing he is so firm here that we initially suspect him of having fallen into error; "one has no means of showing that they [the resemblances--SPH] do not arise from borrowings"<sup>26</sup> (op.cit.), he says. Here he must again be referring to the identification of borrowings by mere inspection. It is difficult to believe that Meillet meant literally that borrowings are never identifiable, since he demonstrates elsewhere (1965:105f), for Persian loans in Armenian, just how it is possible to do so. This Armenian case is considered further below.

It is only fair to point out that in the paragraph immediately following the selection Nichols quotes, Meillet claims that "in the case of languages that have very little grammar, if almost all the grammar properly speaking resides in some rules for the relative ordering of words, as in certain languages of the Far East or Sudan, the method doesn't apply". But crucially, he continues: "And then the question of genetic relatedness is practically insoluble, *as long as we have no criteria permitting us to conclude ... that the lexical similarities that they show are not due to borrowing.*"<sup>27</sup> (1965:97) [emphasis mine]

#### 4.3 On Borrowability and the Identification of Borrowings

Meillet considered substantive grammatical evidence to have particular value in genetic arguments only because he accepted the following two hypotheses:

1. Grammatical constructions cannot be borrowed.
2. Lexical borrowings are very difficult to distinguish from directly inherited lexical items.

That perspective on the importance of grammatical evidence was not Meillet's alone of course. Not only was it shared by many of his contemporaries, Edward Sapir for example (as cited in Harris and Campbell (1995:121) and Poser and Campbell (1992:217), for instance), but it seems to have been in the wind from the very beginnings of modern comparative historical linguistics, as evidenced by the following passage from Rask (1818):

"Grammatical agreement is a far more certain indication (than is vocabulary) of relationship or original unity; for one finds that a language which is mixed with another very rarely or never takes over changes of form or inflection from this, but on the other hand the more readily loses its own." (Lehmann 1967:31)

Meillet's hypotheses regarding borrowing thus appear to have been part of the

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26 "...on n'a aucun moyen de montrer qu'elles ne proviennent pas d'emprunts."

27 "...si l'on est en présence de langues qui n'ont presque pas de grammaire, si presque toute la grammaire proprement dite tient en quelques règles de position relative des mots, comme dans certaines langues d'Extrême Orient et du Soudan, le procédé ne s'applique pas. Et alors, la question des parentés de langues est pratiquement insoluble, aussi longtemps qu'on n'aura pas trouvé de critères qui permettent d'affirmer ... que les ressemblances de vocabulaire qu'elles offrent ne sont pas dues à des emprunts."

comparative linguistic tradition almost from the outset. However lengthy their pedigree, that alone does not make them correct. We are responsible for reassessing them in terms of current knowledge, all the more so because they are so readily treated as matters of conventional wisdom.

We observe first that hypotheses 1 and 2 above are independent; the relative borrowability of one sort of construction has no bearing on the identifiability of constructions of another sort as borrowings.<sup>28</sup> Meillet himself (1965:87) cited a number of examples of the *effective* borrowing of grammatical constructions: the Persian relative marker *ki* borrowed into Turkish, and the Latinate *-ariu* derivational suffix borrowed into German as *-erei*. Similar examples abound. Yet Meillet denied that in these cases the constructions were borrowed as grammatical constructions. He argued rather that they entered the target language as lexical borrowings, and only subsequently became what we would term productive. As already noted, Meillet maintained that true grammatical forms, by which he seems to have meant inflectional morphology, are not borrowable.

I have no reason to doubt that we now have more data on borrowing than was available to Meillet, as well as a richer typology of language contact situations and contact-induced change types. As Thomason and Kaufman (1988) have demonstrated, in principle anything can be borrowed. In one extreme case described by Thomason (1983), the Cushitic language Ma'a (Tanzania) has had a complete Bantu grammar, including the inflectional morphology, grafted onto it. That analysis has recently been disputed by Mous (2003), however, who argues that the Ma'a situation is a consequence of language shift, the adoption by Cushitic speakers of the Bantu language Mbugu. Ma'a emerges in Mous' analysis as a *register* of Mbugu characterized by large amounts of Cushitic lexicon.

Whatever the facts of this or other reported cases of large-scale grammatical borrowing, the conventional wisdom on grammatical borrowing is correct in relative terms at least. Grammatical constructions are much less often borrowed than are lexical, and with the exception perhaps of some cases of widespread bilingualism non-native morphology is more often, if not always, introduced through borrowed lexical items.<sup>29</sup> That said, it does not follow that morphological (grammatical) evidence is the only trustworthy evidence of genetic relatedness. That would only be the case were it impossible to distinguish borrowed from inherited lexical items by means of the standard comparative method.

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28 One might argue that the hypotheses would not necessarily be independent were they claims about constructions of the same type. For example, one might argue that the identifiability of lexical borrowing is impeded in many languages by the sheer number of such borrowings, given that lexical constructions are so readily borrowed. On this issue, see further below.

29 Note that here I am referring only to the borrowing of substantive grammatical constructions, and *not* to instances of schematic calquing, either of a piecemeal sort or on the massive scale for which Malcolm Ross has coined the term *metatypy* (see Ross (1996) and (2006)). Contact-induced changes of this sort are considered briefly in section 4.3 below.

A classic case is that of Armenian, as treated in Hübschmann (1875), who took issue with the then current view that Armenian subgroups with Indo-Iranian and instead proposed that it is a separate first order branch of Indo-European. Hübschmann demonstrated that the misclassification of Armenian was based on the failure to distinguish three strata in the Armenian lexicon, a native one and two large sets of Persian loans, one from Old Persian and the other from modern Farsi. His work has been cited as an instance of the triumph of grammar over lexicon, as in Poser and Campbell (1992:226) who write:

"Hübschmann's discovery of the correct position of Armenian within the IE family was due to his recognition that words are so easily borrowed as to be poor indicators of genetic affiliation, vastly inferior to morphology..."

citing Hübschmann himself:

"If now we have become suspicious of the lexicon, we may turn with greater confidence to the grammar; for in all living languages this is surely the palladium that a foreign influence cannot touch. How wild is the lexicon of Afghan and New Persian, or English, and how clearly does the grammar teach us that in the former we have Iranian at hand, in the latter Germanic!"<sup>30</sup>

One can be forgiven at this point for jumping to the conclusion that Hübschmann based his analysis on the position of Armenian on grammatical evidence. But after discussing Armenian noun and verb morphology, he concludes:

"Result: In the inflection of Armenian we cannot demonstrate any specific Iranian characteristics; on the contrary it differs in an important point with Aryan and agrees with Balto-Slavic.

Since the inflection does not give us enough information about the character of Armenian, we will turn to the phonology." (Lehmann 1967:171)

The bulk of Hübschmann's essay deals with the distinct reflexes of Indo-European phonemes in the different strata of the Armenian lexicon, and it was on this basis that he concluded that Armenian is a distinct branch of Indo-European.<sup>31</sup> Here again it would appear that it is not the use of lexical evidence *per se* that is condemned, but the cursory inspectional assessment that many of Hübschmann's predecessors had given of the evidence.

Hübschmann's method for distinguishing borrowed from inherited lexicon has been used

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30 I cite the translation in Lehmann (1967).

31 Poser and Campbell seem to deem it significant that the publication of Hübschmann's paper antedated that of the Neogrammarian manifesto, in the *Morphological Investigations*, by one year. That fact does not of course mean that Hübschmann was unaware of Neogrammarian principles or unsympathetic to them. But whether or not he believed in regular sound change is irrelevant to the manner in which he used sound correspondences to sieve borrowings.

over and over again on a range of languages. Another classic case is that of Rotuman (as reported in Biggs (1965), in which there are two substantial lexical strata, one borrowed from Polynesian and one directly inherited. The question of whether or not Rotuman was an (Eastern) Oceanic language, related to Fijian, the Polynesian languages, and the Micronesian languages, was never at issue. The problem was how to account for the existence of two distinct sets of sound correspondences between Rotuman and other Oceanic languages, sets that Biggs noted were overwhelmingly mutually exclusive by lexical item. Biggs explained the Rotuman data by demonstrating that one set was what one would expect of borrowings from a Polynesian source. The other set had no obvious source so was assumed to reflect inherited forms. In a similar fashion, I have been able to distinguish inherited Micronesian from borrowed Polynesian lexical items in Gilbertese (Harrison 1994).

There are at least two factors that serve to mask the difference between borrowings and inherited items, and make it difficult to distinguish them through competing sound correspondences. First, and particularly when the two languages are quite closely related, there is no guarantee that the reflexes of segments of the proto-language will differ sufficiently to permit all borrowings to be identified. Second, in situations in which many speakers of the target language are bilingual in the source language, a 'metalinguistic awareness' of the usual sound correspondences may emerge, and lead speakers of the target language to nativize borrowings. In both these situations, many borrowings may be undetectable. In my experience, at least some borrowings *will* be detectable, and as a result we are still able to determine that there has been borrowing, though we might underestimate its extent. Whether any error would be of a sufficient order to lead to genetic misclassification is, I think, difficult to determine in the abstract.

It is also sometimes claimed<sup>32</sup> that borrowings of sufficient time depth may be undetectable, presumably because the accretion of later changes would completely mask the contrast between inherited and borrowed items. One wonders, though, whether at such time depths enough evidence of *any* sort would survive to enable a strong genetic argument to be made. I suspect not.

One sort of historical circumstance in which borrowings are not at all distinguishable from inherited items is exemplified by the linguistic situation in New Caledonia. Grace (1981) reported on the analysis of the substantial data sets he was able to collect for two New Caledonian languages, Canala and Grand Couli. These languages have identical phonemic inventories of 24 consonants and 18 vowels (oral and nasal). From an initial inspection of the lexical data approximately nine hundred potential cognate sets emerged. Further analysis revealed 140 consonant correspondences (56 with more than 5 tokens)

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32 For example, by Teeter (1994), a view with which Thomason (1994) apparently concurs.

and 172 vowel correspondences (67 with more than 5 tokens). There was little evidence of conditioned change, to permit any merging of the observed correspondence sets.

As certain as Grace was that Canala and Grand Couli (and other New Caledonian languages) are genetically related, the comparative method sheds no light on that relatedness. From Grace's observations one must conclude either that there are no regular correspondences or that the proto-language had an unnaturally large phonemic inventory.

What is relevant to our present discussion is Grace's (1990) account of how the New Caledonian situation arose. One possible account would challenge the regularity of sound change, and argue that what we observe in the New Caledonian case is a large number of sound changes, all prematurely deceased. But attacking regularity is unproductive, akin to beating the proverbial dead horse, since regularity is not an empirical hypothesis but a methodological necessity. Grace's alternative account is more interesting. He suggests that the New Caledonian situation is a consequence of long-term and multi-directional lexical borrowing, resulting in a situation in which the languages of the area appear to have dipped almost randomly into a common lexical pool comprising all the inherited lexical items of all the languages of the area.

It is in language contact situations of this sort, and not in the perhaps more common Rotuman-type cases, that the standard comparative method fails so spectacularly, because we cannot hope to sort out borrowed from directly inherited items. The languages may have a common ancestor, as Grace believes in the New Caledonian case, or they may not. The standard comparative method simply has nothing to say on the matter. Just how common such historical circumstances are is another, empirical matter.

#### 4.4 *The Stability of Grammar*

For all Meillet stressed the stability of grammar and the primacy of substantive grammatical evidence because of his belief in its immunity from borrowing, he was quite aware that grammar is open to profound change. In one context, Meillet exemplifies the effects of language change through the case of the single French lexeme *père* 'father', noting the phonological, grammatical, and semantic differences between that lexeme and its Indo-European source. Regarding grammar, he notes that:

"...the grammatical form has changed completely, since the Indo-European word had more than fifteen different forms according to number and case and the French word is invariable (the plural *s* is purely orthographic)."<sup>33</sup> (loc. cit.)

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33 "...la forme grammaticale a changé du tout au tout, puisque le mot indo-européen admettait plus de quinze formes différentes suivant les nombres et le cas et que le mot français est invariable (l'*s* du pluriel est purement orthographiques)."

Elsewhere Meillet says that these sorts of changes, that we might now label *typological*, can in fact be quite rapid. He begins his much referenced discussion of proofs of genetic relatedness (Meillet 1925, ch. 3) with a discussion of language typology, in which he identifies older Indo-European languages like Sanskrit and Latin as *inflectional* (in particular, suffixational). While noting that "[u]p to the present time even the most evolved languages of the Indo-European family have preserved some of this old type: in French, for example, the substantive has become invariable ... but the verb has still much inflection" (Meillet 1925:37f), for him "[t]he Romance languages, the majority of Germanic languages, and the Iranian languages no longer really merit today the name *inflectional*." (op. cit.:38). He concludes that "even the most conservative [modern--SPH] Indo-European languages have a type completely different from Common Indo-European. The structures of the various Indo-European languages spoken today are very different from the structure which Common Indo-European had and, besides, are very different from each other. Consequently, it is not by its general structure that an Indo-European language is recognized." (Meillet 1925:38)

Meillet analyses French as moving toward prefixation, his examples showing that he viewed proclitics as prefixes, or perhaps incipient prefixes. In the passage in question, Meillet then notes i) that the Latin person-number verbal suffixes have in French largely been replaced by proclitic pronouns and ii) that the Latin inflectional genitive construction, in which a genitive and a non-genitive noun can appear in either order, has in French been replaced by a postposed analytic genitive introduced by what he identifies as the particle *de*. He concludes: "Thus it is not with such general features of structure, which are subject to change completely in the course of several centuries, and moreover do not have numerous variations, that one can establish linguistic relationships." (op. cit. 39)

It is in this context that Meillet asserts:

"What conclusively establish the continuity between one *common language* and a later language are the particular processes of expression of the morphology." (loc. cit.)

We might make two observations at this point. First, as the first historical linguist, to best of my knowledge, to write at length about grammaticalization phenomena, Meillet understood quite well that grammatical constructions (along with their substantive markers) can readily be replaced, and that new substantive grammatical markers are relatively easily created from the resources available in the language in question. For Meillet, the stability of grammar is not so much that it is resistant to *change* as that it can't be borrowed or is at least resistant to *borrowing*.<sup>34</sup> This view is I think confirmed by

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34 Meillet wrote at least two papers dealing with the sort of linguistic convergence often termed *drift*, the phenomenon he characterises as "parallélisme des changements de structure générale, divergence des innovations portant sur les moyens matériels d'expression" (parallelism in the broad structural changes, divergence in the innovations giving rise to the material forms of expression) (1965:63). He seems to regard

Meillet's observations, in the very discussion to which we have been making reference, concerning the survival of the northern French 3rd person singular subject proclitics in the face of pressure from the standard language:

"Singular facts of this sort are often stable...[they] are learned from infancy; they become habits of which one is not aware and are capable of remaining when everything else is modified." (op. cit.:40)

Indeed, so easily can the basic typological cut of a language be altered that Meillet rejects typological similarities as evidence of genetic relatedness. That second observation is particularly important in understanding what Meillet meant by *singular fact* and *particular process*.

#### 4.5 Shared Aberrancies and *Faits Particuliers*

Meillet frequently wrote of the particular importance of what have come to be termed shared anomalies or shared aberrancies in demonstrating genetic relatedness. For example:

"... it is not necessary for proof [of common descent] to demand that all grammatical forms be explained; it suffices to establish that notable portions of the ancient morphology survive in the language in question. Nowhere is it easier to establish linguistic descent than amongst the Indo-European languages, because the common language on which rest the languages of this family had a very complex morphology, of a complexity far surpassing the usual and of which many residual forms persist in each languages; *for example, the irregular verbs of Greek, the strong verbs of Germanic, etc.*"<sup>35</sup> (Meillet 1965:93, emphasis mine)

In a discussion of Romance numerals, Meillet observed:

"Where apparent similarities have indicated the right path, it often happens that some singular detail brings confirmation. It is significant, for example, that there is a distinction between the masculine and feminine for *un, une* and not for the other numerals." (Meillet 1925:16)

This observation directly follows his demonstration that the Romance numerals are not

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such parallel developments to be natural, in the sense that they follow from similar linguistic circumstances (op. cit.:73) and does not consider grammatical borrowing to be a factor in explaining such cases. Though Meillet did recognise the phenomenon of lexical calquing (1965:127f), from Greek to Latin and thence to other European languages, for him it seems to have been restricted to the lexicon.

35 "...il n'y a pas lieu pour faire la preuve d'exiger que toutes les formes grammaticales s'expliquent; il suffit d'établir que des portions notables de la morphologie ancienne subsistent dans la langue considérée. Nulle part il n'est aussi aisé d'établir une parenté de langues qu'entre les langues indo-européennes, parce que la langue commune sur laquelle reposent les idiomes de cette famille comportait une morphologie très compliquée, d'une complication que passe beaucoup la normale et que de nombreux restes de ses formes ont subsisté dans chaque langue; ce sont par exemple les verbes irréguliers du grec, les verbes fortes du germanique, etc."

only superficially similar in form, but that they instantiate the regular sound correspondences found throughout the lexica of Romance languages. It is clear, to me at least, that the 'apparent similarities' to which Meillet refers are not these regular sound correspondences, but rather the inspectional similarities that become evidence of genetic relatedness only when shown to be patterned. The grammatical gender anomaly is additional evidence, the 'frosting on the cake' as it were, which for Meillet puts the matter beyond doubt.

References such as the above have led some commentators to identify Meillet's notion *fait particulier* 'singular fact, singular detail' with our current *shared aberrancy*. Campbell makes just such an identification when he states that "Meillet favored 'particular processes,' 'singular facts,' 'local morphological peculiarities,' 'anomalous forms,' and 'arbitrary' associations (i.e., "shared aberrancy")" (2003:269). Yet the very quote Campbell uses to justify that assertion is one that suggests that, for Meillet, a singular fact was not simply an aberrancy, and that some facts are more singular than others:

"The more singular the facts are by which the agreement between two languages is established, the greater is the conclusive form of the agreement. Anomalous forms are thus those which are most suited to establish a 'common language'.

The fact that French *il est, ils sont, je fus* agree with Latin *est, sunt, fui* is such to make it clear that French is a new form taken by Latin. ..." (Meillet 1925:41)

There is no doubting that Meillet regarded the anomalies observed in the French and Latin paradigms for 'to be' as singular facts of those languages. Yet he speaks of singularity here as a gradient property of which, I will argue, irregularities such as these are but the extreme case.

The most often cited example of a shared aberrancy, and a singular fact, from Meillet's work is the case of the 3rd person of 'to be' in a number of Indo-European languages:

	3s	3p
Sanskrit	ásti	sánti
Latin	est	sunt
Gothic	ist	sind

The aberrancy here is the use of the Indo-European e-grade stem allomorph in the singular and the Ø-grade in the plural. From the context in which Meillet introduces this example in *Sur la Methode de la Grammaire Comparée* (Meillet 1965:25), we are led to conclude that the singularity or particularity of his *faits particuliers* is not their irregularity but their arbitrariness. Meillet was not opposing *regular* and *irregular*, but *arbitrary* (in Peircean terms, *symbolic*) and *general* (schematic, typological, natural, or iconic).

The aim of the paper in question, Meillet says, is "to describe the process of reasoning

used by comparativists and to examine its probative value."<sup>36</sup> (1965:19) He encapsulates that process as follows:

"The reasoning is of the following form: one observes in such and such languages such and such forms of expression that correspond more or less exactly; these correspondences cannot be explained unless at a certain date there was a common form of which all the similar forms ... are the continuations."<sup>37</sup> (op. cit.:20)

After providing examples from the 1s and 2s independent pronouns of Romance, he continues over the following five pages:

"There is no general reason by which the person who is speaking is designated by forms such as *eo*, *io*, *jo*, *yo*; ... The essential point in the reasoning is this; the agreement of Romanian *eo*, Italian *io*, Old French *jo*, Spanish *yo* cannot be fortuitous.... In a general fashion, the attempts that have been made to explain the meaning of words through properties of the nature of sounds have never met with any success...."<sup>38</sup> (op. cit.:20-21)

Meillet observes that the likelihood that chance is a factor diminishes as the number of similar forms increases, and also that:

"Correspondences that admit of general causes, true for all languages, are stripped of probative value for the comparative historian."<sup>39</sup> (op. cit.:23)

The presence of particular common sounds in two languages is not evidence of any relationship between those languages but neither is the presence of unusual sounds. Meillet says:

"By contrast, the presence in two neighboring languages of the same phoneme of a quite particular and rare sort, like the *jery* of Russian and Polish is an initial reason for believing in the relatedness of these two languages; it is however just a clue, and it is not on a phenomenon of this type, of an altogether too general sort, that one can base

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36 "Ce que l'on se propose ici, c'est de décrire le procédé de raisonnement des comparatistes et d'examiner quelle en est la valeur probante."

37 "Le raisonnement est de la forme suivante: on observe dans telles et telles langues telles et telles manières d'exprimer plus ou moins exactement concordantes; ces concordances ne s'expliqueraient pas s'il n'y avait eu à une certaine date une forme commune dont toutes les formes semblables ...sont des continuations."

38 "Il n'y a pas de raison générale pour que la personne qui parle se désigne par des formes telles que *eo*, *io*, *jo*, *yo*;...Le point essentiel du raisonnement est celui-ci: la concordance de roumain *eo*, italien *io*, vieux français *jo*, espagnol *yo* ne peut pas être fortuite....D'une manière générale, les tentatives qui ont été faites pour expliquer par des propriétés de la nature des sons le sens des mots n'ont jamais aboutis à aucun succès...."

39 "Les concordances qui reconnaissent des causes générales, valables pour l'ensemble des langues, sont dénuées de valeur probante pour le comparatiste historien."

a claim of relatedness."<sup>40</sup> (op. cit.:24)

"What establishes common origin", he continues, "is the concordant existence in two or more languages of particularities that cannot be explained by general conditions, anatomical, physiological, or psychological....What leads to the proof of relatedness is that the first person singular is characterized by a y ... and an o type vowel ..."<sup>41</sup> (op. cit.:24)

Meillet concludes:

"From the principle of the method it follows that probative facts in the domain of comparative grammar are singular facts, and they are especially probative in that, by their nature, they are less suspect of admitting a general cause. That is only natural; since it is a matter of presenting through comparative processes the historical fact of the existence of a particular language, that is to say an object that, by definition, was produced by the convergence of diverse circumstances having no necessary relation to one another, it is the singular facts of a historical nature that alone should enter into consideration."<sup>42</sup> (op. cit.:24-5)

It is at this point that Meillet introduces *est/sunt* example.

I quote at such length from Meillet here only because I think he has been so grossly misunderstood on this issue. It is not shared *anomalies* or irregularities that demonstrate genetic relatedness, but the shared (arbitrary) *symbols* that are the common currency of the standard comparative method. To be sure, paradigmatic irregularities like the gender marking of the Romance numeral 'one' or the stem alternations of the 3rd person present forms of Indo-European 'be' are more singular than the correspondences one might observe in the Romance lexeme for 'mother', but this is a matter of degree.

If there is anything singular about the 3rd person forms of the Indo-European verb 'to be' it is the persistence of the irregular stem alternation, *es-* in the singular and *s-* in the plural.

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40 "Au contraire, la présence dans deux langues voisines d'un même phonème de type tout particulier et rare parmi les langues humaines, comme le *jery* du russe et du polonais, est une première raison de croire à la parenté de ces deux langues; ce n'est d'ailleurs qu'une simple indication, et ce n'est pas sur un phénomène de ce genre, de nature encore beaucoup trop générale, qu'on peut fonder l'affirmation d'une parenté."

41 "Ce qui établit une origine commune, c'est l'existence concordante dans deux ou plusieurs langues de particularités telles qu'elles n'expliquent pas par des conditions générales, anatomiques, physiologiques, ou psychiques....Ce qui tend à prouver une parenté, c'est que la première personne du singulier soit caractérisée par un y...et par une voyelle du timbre o... "

42 "Du principe de la méthode il résulte que les faits probants en matière de grammaire comparée sont des faits particuliers, et ils sont d'autant plus probants que, par leur nature, ils sont moins suspects de pouvoir reconnaître une cause générale. Il n'y a rien là que de naturel: puisqu'il s'agit de poser par des procédés comparatifs le fait historique de l'existence d'une langue particulière, c'est à dire une chose qui, par définition, se produit en vertu d'un concours de circonstances diverses n'ayant pas de rapports nécessaires les unes avec les autres, ce sont des faits particuliers de caractère historique qui doivent seuls entrer en considération."

And that is perhaps not so much a grammatical fact as a purely lexical one.<sup>43</sup> What makes the similarities in the Latin and Sanskrit forms of 'to be' more compelling, more *singular* than, for example, those in forms of 'to bear, to carry' such as:

	3s	3p
Sanskrit	bharati	bharanti
Latin	fert	ferunt
Gothic	bairip	bairand

is precisely that they are less compositional, and more arbitrary and symbolic. They are cases where we expect the singular and plural stems to be the same; they are cases where we expect iconicity but don't find it.

That Meillet understood his *fait particulier* in this broader sense is confirmed from the examples he gives of language families in which he says they abound:

"It is through singular facts of this sort that one establishes language relatedness. Where, as often happens, the structure of the languages considered provides only a few *singular* facts that can be identified or provides none at all, the rigorous establishment of genetic relatedness faces the gravest difficulties, and historical linguistics can barely make a start. By contrast, where, as in Indo-European, Semitic, Finno-Ugric, Bantu, and Indonesian (Malay), *singular* correspondences of this type abound, historical linguistics is already established and is progressing rapidly."<sup>44</sup>  
(Meillet 1965:26)

I cannot speak for all the language families Meillet lists, but in the case of the Indonesian (by which I understand Austronesian) material that Meillet would have had at hand, shared aberrancies like the Indo-European 'to be' do *not* abound, so Meillet must have had some other sort of singular fact in mind, like the sound correspondences and concomitant lexical reconstructions proposed by such pioneering Austronesianists as Kern, van der Tuuk, and Brandstetter.

## 5. Genetic Linguistic Evidence and Nineteenth Century Comparativist Practice

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43 Unless otherwise noted, Indo-European forms are cited as in Pokorny (1958). An anonymous reader suggests that the *es-/s-* alternation in reflexives of Indo-European 'to be' is perhaps not exclusive to the paradigm of 'to be', being found also, he says, in Hittite and Vedic reflexes of \*g<sup>w</sup>hen-2(ə)- 'hit, slay': in the third singular, Hitt. kuenzi, Ved. hānti/, and in the third plural Hitt. kuananzi, Ved. ghnānti. Whether or not this is indeed the same alternation, the fact that it is found in two etyma rather than one does not make it any less lexical, unless it can be shown to be the product of regular phonological processes in the languages in question.

44 "C'est par des faits particuliers de ce genre qu'on établit les parentés de langues. Là où, comme il arrive souvent, la structure des langues considérées ne fournit pas de faits singuliers qui puissent être rapprochés, l'établissement rigoureux de parentés de langues rencontre les plus graves difficultés, et la linguistique historique arrive à peine à se constituer. Au contraire, là où, comme sur le domaine indo-européen, sur le domaine bantou, sur le domaine indonésien (malai), les concordances singulières de cet ordre abondent, la linguistique historique est déjà créée et progresse rapidement."

## 5.1 On Reasons for Invoking Intellectual History

There is no reason to be surprised that the matter of 19th century comparative-historical practice should be raised in discussions of comparative historical methodology, since most of the ideas that now govern our current practice arose in the course of roughly that hundred year period.<sup>45</sup> Though it is possible to discuss family trees without mentioning Schleicher, or the Germanic sound shifts without mentioning Rask, Grimm, and Verner, there would be no point in doing so. There are good reasons for studying intellectual history: to learn what we have inherited from our antecedents and just how far our thinking has, or has not developed over time. There are not so good reasons too, as in the search for vicarious justification that characterized much interest in the history of linguistics in the first decade of the generative movement.

Neither of these motives quite characterize the recent interest in 19th century practice that one finds in papers like Poser and Campbell (1992) and Nichols (1996). My interest in those two papers is that both assert the primacy of grammatical evidence in 19th century theory and practice. For example, Poser and Campbell declare that:

"a survey of Indo-Europeanists' claims about methods and their actual practice shows both that the recognition of languages as IE and the subgrouping of languages within the IE family have been based primarily on submerged morphology, and, especially in the case of subgrouping, secondarily on phonological isoglosses,..." (1992:218)

My main purpose in this and the following section is to argue that such claims are misleading, and do not do justice to the history of comparative historical linguistics in the 19th century. In making my case, I believe it is important to consider why these authors raise the matter of 19th century comparativist practice at all.

For Poser and Campbell the motive is clear, though I would take issue with part of their assessment of the facts. Poser and Campbell state that they want to set the record straight with respect to claims by proponents of mass comparison "that this is the methodology used to establish the Indo-European language family, and that the success of these methods in the Indo-European case shows them to be reliable" (Poser and Campbell. 1992:214). Their interest is not in critiquing the primacy of regular sound correspondences in the standard comparative method but in disputing claims by advocates of mass comparison that regular sound correspondences had little role in genetic arguments until the Neogrammarian period. As already noted in section 1 above, Poser and Campbell observe correctly that the Neogrammarian contribution was not the discovery of sound laws, which had been discussed recognized throughout the century, but the assertion that sound laws are exceptionless. The examples Poser and Campbell

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45 To argue whether *most* or *all* is the appropriate quantifier here requires a rather more detailed history than is relevant for present purposes.

cite clearly show scholars of the period using sound correspondences to reach genetic conclusions with which with still concur, and being led into error by superficial lexical resemblances. Poser and Campbell do not make a particularly strong case for the primacy of morphological evidence, since in all the positive cases they discuss, crucial reference is made to sound correspondences. Though their purpose is clearly partisan, it is no less laudable for that since Poser and Campbell are concerned with the facts of history.

It is less obvious why Nichols invokes the history of 19th century comparative historical linguistics in her 1996 paper. That paper seems to be an elaboration of the rather lengthy footnote (Nichols 1992:311-313) concerning the comparative method in her 1992 monograph. The purpose of her 1996 paper is to "argue that demonstration of relatedness through systematic correspondences in vocabulary is not the operating procedure for the classic application of the comparative method to the Indo-European languages going back to the late eighteenth century;...". (Nichols 1996:41) From that one might conclude that her purpose, like that of Poser and Campbell, is to set the record straight on comparative-historical practice in some period beginning (as she makes clear later) with Sir William Jones. Crucially, she is less clear about when the historical period she considers ends. Nichols continues: "...nor -- and this is perhaps more important -- is it the definition in the classic secondary literature on Indo-European and general comparative linguistics." (loc. cit.)

Nichols argues that the comparative method involves what she terms a heuristic component, a first step in determining genetic relatedness that "does not rely on vocabulary; ... The diagnostic evidence is grammatical, and it combines structural paradigmaticity (usually multiple paradigmaticity) and syntagmaticity with concrete morphological forms" (op. cit.:64) Her heuristic component thus corresponds roughly to the common interpretation of Meillet's *faits particuliers*. I consider her notion *diagnostic evidence* in section 6 below.

If all that is at issue in Nichols' paper are the facts of 19th century intellectual history, then it would be a straightforward enough matter for me to note where I think she has misrepresented that history, as indeed I will attempt to do in section 5.2. Yet I think there is more at stake than that. It seems to me that Nichols has succumbed to what I will term the *fallacy of operationalizing history*. As best as I can determine, for her the comparative method is not what we now, some 220 years after Sir William Jones delivered the Third Anniversary Discourse in Calcutta, say that it is. Rather, for her the comparative method is defined by what some group of scholars in the course of the 19th century did and, in fact, by the order in which they did it. For her, the comparative method *is* the history of 19th century comparative historical linguistic thinking.

Nichols' comparative method consists of four steps, which I abstract here from her exposition (1996:48-60):

- First Step: Assume relatedness of a set of languages, based on diagnostic linguistic evidence
- Second Step: Work out sound correspondences and cognate sets, thereby establishing an internal classification of the family
- Third Step: Uncover and reconstruct more diagnostic evidence
- Fourth Step: Bring more languages into the family as daughters

I have already noted what sort of evidence she regards as initially diagnostic, and consider the matter further below. Her views regarding the relation between sound correspondences and subgrouping, considered briefly in the discussion to follow, lead her to conclude not only that semantic similarity is difficult to define in determining cognacy, but is in fact totally irrelevant. A detailed critique of this rather odd perspective is not amongst the goals of the present paper. In her third step she is forced to concede some diagnostic value to sound correspondences, which presumably could have been used in step one but for their being sound correspondences and therefore a priori of no initial interest. In Nichols' view of history, or method, we first begin with a few languages (like Latin, Greek, and Sanskrit), then reconstruct, and in a final fourth step we add new members by comparing them with the reconstructions. Whether or not that is an accurate picture of the history of Indo-European studies, it does not follow, as she seems to be suggesting, that we now are compelled, by adherence to what she regards as sound methodology, to work that way.

Let me restate the issue. As already noted, if this were just her compact version of how the comparative historical linguists over a period of perhaps a century worked out the details of the Indo-European language family, then it would be just be a matter of critiquing it as history. But Nichols does not present her four steps as just history, but as a guide for how we at the beginning of the 21st century should evaluate evidence for genetic relatedness and conduct comparative historical research. It is almost as if one were instructed to write a grammar by first applying structuralist classification procedures to find the morpho-lexical categories and then to use some generative theory to describe sentence structure, because that's the way it happened!

It is perhaps not surprising that Nichols has succumbed to the fallacy of operationalizing history in her approach to the comparative method. As I have already noted, the comparative method is often presented as a catalogue of the accomplishments of 19th century scholars.<sup>46</sup> But we need not, and should not, work as they did, since we have available to us the entire scope of 19th century comparative historical thinking and more,

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<sup>46</sup> I am reminded of a course offered for a time in the early years of the linguistics program at the University of Toronto, under the tutelage of Martin Joos, entitled *Historical Linguistics and the History of Linguistics*.

from which to distill our comparative method. It indeed seems to be the case that subgrouping became a major concern to 19th century scholars only in the second half of the century, after Schleicher popularized the family tree model. In that same period, and I think coincidentally, the focus of comparativist thinking shifted away from morphology and morphological typology and toward sound change. But from that it follows neither that sound correspondences are only of value in subgrouping, as Nichols seems to be asserting, nor that we cannot think about subgrouping until we've worked out sound correspondences. As a story about the 19th century, her assertions are misleading and perhaps contentious; as a recipe for comparative historical practice, they are derisory.

## 5.2 *Comparative Linguistics in the Nineteenth Century*

This is not the place to undertake a comprehensive investigation of pre-Neogrammarian 19th century linguistic thought, nor am I particularly qualified to do so. Rather I begin here with some remarks of a very general nature concerning the received view of the history of the period, before turning briefly to the question of how lexical, grammatical, and phonological evidence were viewed and used in the comparative historical investigations of the first half of the 19th century. I do not dispute that the writings of most of the major figures of early 19th century comparative linguistics contain statements to the effect that the best evidence for genetic relatedness comes from grammatical similarities. I do however dispute any claim that this avowed preference for grammatical evidence was to the exclusion of or, in the case of many of the leading scholars of the period, even at the expense of lexical evidence supported by regular sound correspondences. I will argue that the emphasis placed on grammatical evidence in the work of early 19th century comparativists had two sources. On the one hand, like Meillet a century later, these early comparativists worried about the identifiability of borrowings and about the misuse of vague inspectional lexical resemblances in genetic arguments. On the other, many of these scholars were interesting in grammar in a sense *for its own sake*, because they believed that the essential nature of a language is to be found in its grammar. Therefore, in the first half of the 19th century, grammar was accorded a central place in both synchronic and diachronic language classification, to the extent that the two were distinguished in that period.

In her detailed study of the historiography of language classification in the 19th century, on which I rely heavily in the following discussion, Morpurgo-Davies stresses the significance of grammatical evidence in the genetic linguistic arguments of the early 19th century. She asks:

"What were the arguments used to demonstrate genetic relationships? Historians of linguistics seem to agree that by the beginning of the nineteenth century the emphasis was on structural similarity rather than on lexical equivalences. Regularity of sound

correspondences was sometimes mentioned, but certainly not be all linguists."<sup>47</sup>  
(1975:625)

In the work of such early luminaries as Schlegel, Rask, and Bopp one finds references to the possibility of foreign admixture in vocabulary. Rask's remarks, cited earlier, regarding the immunity of inflection from borrowing have a particularly modern ring, if we are to regard Meillet as modern. Nonetheless we must still beware of anachronism, and exercise caution in equating these early 19th century concerns too closely with our own. We now understand that it is possible in many circumstances to identify borrowings without textual evidence of their sources, but it is not clear that the early 19th centuries comparativists did. Lehmann reminds us too that the academic environment of the early 19th century was different from our own. He notes Schlegel's insistence that "we permit absolutely no rules of change or replacement of letters, but rather demand complete equivalence of the word as proof of descent" (Lehmann 1967:24) and concludes that Schlegel's "demand for precise agreement of vocabulary items may be understood when we compare the fanciful etymologies of his predecessors; insistence on rigor was essential to stop further such fabrications" (op. cit.:22) In fact, Schlegel's apparent strong rejection of sound correspondences is more a matter of rhetoric than methodology. In the very next sentence Schlegel gives a number of 'letter changes' that he does accept, provided that one can "demonstrate the intermediate steps or the general analogy historically" (op. cit.:24) It is cavalier appeal to 'replacement of letters' that apparently offended Schlegel, not the technique itself.

Morpurgo-Davies suggests that the preference on the part of early 19th century comparativists for grammatical over lexical evidence in genetic arguments was much more than a matter of what could and could not be borrowed. She seems to imply that we should understand comparative linguistics in the early part of the 19th century not just, or perhaps not even primarily as an inquiry into the history of languages, but as part of the broader interest in classification in the natural sciences. Just as Linnaeus and de Jussieu sought to bring order into the natural world in the eighteenth century, so at the beginning of the nineteenth that program was extended to the human world. Morpurgo-Davies notes (1975:614) that "Rask wanted to replace Linnaeus's division into *classis*, *ordo*, *genus*, *species*, and *varietas* by a (genealogical? classification of languages into *Race (Aet)*, *Klasse*, *Stamme*, *Gren (branch)*, *Sprog*, *Sprogar*".<sup>48</sup> Language was an obvious parameter on which to construct a classification of humanity. There is some evidence that from very early in the nineteenth century, genetic classification was being contrasted in this regard

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47 The occasional dissenting voice does appear. Morpurgo-Davies (op. cit: 628) notes Julius von Klapoth, who in 1823 "had argued that that *Stammverwandschaft* ['affinity, genetic relatedness' -- SPH] was mainly founded on word-similarity. The comparison of grammatical forms -- he maintained -- was not useless, since it helped to illuminate the development and progress of human *Geist*...".

48 Race (Era), Class, Stem, Branch, Language, Language Variety

with what we would now consider typological classification. In his 1813 review of Adelung's *Mithridates*, Young writes: "A perfect natural order of arrangement, in treating of the peculiarities of different languages, ought to be regulated by their descent from each other and their historical relations: a perfect artificial order ought to bring together into the same classes all those genera which any essential resemblance..."<sup>49</sup> (Young 1813-14:252, as cited in Morpurgo-Davies (1975:614))

Morpurgo-Davies wonders "whether all leading scholars in the first part of the century made a clear distinction between genetic and typological affinity" (op. cit.:615) of the sort we now recognize. She speaks of "the interplay, which we cannot help noticing, between genealogical and typological classifications" (loc. cit.) For the historical record it is important to note that some scholars, like Schlegel, clearly did not not understand genetic and typological classification as we do, while others, like Pott, had views in concert with those prevailing today. Thus Schlegel could hold that all inflectional languages were Indic and could cite in the same paragraph both substantive and schematic grammatical similarities as evidence of genetic relatedness.<sup>50</sup> By contrast, Pott wrote in the early 1830's that typological (what he termed *physiological*) properties could be shared by genetically unrelated languages, and in 1855 took Max Müller to task for claiming that all agglutinating languages were genetically related.

Yet it seems to me that Morpurgo-Davies suspects a closer connection between genetic and typological classification in the first half of the 19th century, a connection she finds difficult to describe. She writes:

"By the turn of the century (and probably long before) it had become clear that a genetic (or genealogical) relationship between languages could not be established only on the basis of lexical agreement; structural similarity seemed to be an important criterion. In the same period, however, we also find that typological classification is also based on grammatical and structural considerations. Is this coincidence entirely accidental?" (Morpurgo-Davies 1975:615)

If language was a natural parameter in terms of which to classify humanity, then it seems that for most early 19th century comparative linguists, the most natural parameter by which to classify languages was their grammar, in which the essence of the language was

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49 In the botany of the period, the Linnaean classification based on reproductive organs only was often labelled *artificial* in comparison to the more natural systems proposed by Adanson and by Jussieu, that took the entire plant morphology into consideration.

50 "In Germanic as throughout in Indic, *n* is characteristic of the accusative, *s* of the genitive." (Lehmann 1967:24) Later in the same paragraph, in comparing what he terms the imperfect in German and in Latin, Schlegel writes: "If in another type the imperfect is formed by means of an inserted *t*, this to be sure is a special characteristic, just as is the *b* in the Roman imperfect; the principle however is still the same, namely that the secondary determination of the meaning for time and other relationships does not happen through special words or particles added outside the word, but through inner modification of the root." (loc. cit.:25)

felt to reside. Thus for Rask, vocabulary might be contaminated by mixture. Even grammar is subject to change. Yet in the grammars of the languages he surveys there is evidence of their age. He writes that "[t]he language which has the most ingenious grammar is the most unmixed, the most original, oldest and nearest to the source; for the grammatical inflections and endings are constantly lost with the formation of a new language, and it requires a very long time and intercourse with other people to develop and rearrange itself anew." (Lehmann 1967:31f) The essential nature of grammar was even clearer for Schlegel, who could write:

"The decisive point however which will clarify everything here is the inner structure of the languages or comparative grammar, which will give us quite new information about the genealogy of languages in a similar way as comparative anatomy has illuminated the higher natural history." (Lehmann 1967:25)

Lehmann's assessment of Schlegel focusses on the historical dimension of Schlegel's two classes of languages. "For Schlegel", Lehmann says, "there was an ancient grammar, characterized by inflection, and a more recent grammar, characterized by analytic devices. Languages of the ancient type were more *kunstreich* (ingenious, artistic) than are those of the newer manner. Accordingly, examination of the type of a language might contribute to determining its antiquity "(Lehmann 1967:22) Yet here Lehmann overlooks the absolute nature of Schlegel's classification. Any language descended from Indic, in Schlegel's view, no matter how decayed, will betray some of its older purer inflectional character, while those languages unrelated to Indic, with no inflection, are for Schlegel of a completely different character. In Humboldt we read that "[c]ompared with the process of incorporation and loose attachment without a true word unity, the method of inflection seems to be a principle of genius, proceeding from the true intuition of the language." (Lehmann 1967:65) Not all inflected languages are equal in this regard. There is "a difference of grade between Sanskrit and the Semitic languages: in the latter, inflection in its truest and most unmistakable form and connected with the finest symbolization, yet not carried through all parts of the language and limited through more or less accidental laws--the bisyllabic word form--the vowels used exclusively for designation of inflection--the hesitation about compounding; in the first, inflection preserved against every suspicion of agglutination through the firmness of the word unity, carried through all parts of the language and prevailing in it in the highest freedom." (loc. cit.)

For all that these early nineteenth century linguists understood that vocabulary can be borrowed, and that grammatical morphemes are less likely to be, it was not only for that reason that they elevated grammatical similarity about lexical in their genetic arguments. Grammatical evidence was crucial for them because in grammar was to be found the essence of what a language is, in much the same way that it was felt that the essential nature of a plant or animal species was to be found in its morphology.

In interpreting the work of the early 19th century comparativists, we must be careful not to confuse what we now regard as their accomplishments with what they themselves might have thought they were doing, or to confuse our premises with theirs. What points of agreement there are should not lead us to conclude that their view of language, of language change, and of the goals of comparative linguistics remained homogeneous throughout the nineteenth century, let alone that they were identical to our own. Because we now tend to view the work of that period in a positive light, and its major figures as our direct academic antecedents, it is easy to be blinded both to the differences between their views on language and our own and to their errors. A case in point is Sir William Jones. If he is to be regarded rightly as the individual who ushered in the modern period in linguistics by bringing the Sanskrit language and the Indian grammatical tradition to the attention on European scholars at the beginning of the century, we must also recall, as observed in Poser and Campbell (1992:228-31) and, particularly, in Campbell (in press), that with respect to claims about genetic relatedness Jones was as often wrong as right.

We must also take care not to confuse fact with propaganda. Linguists in the 19th century stressed the contrast between their linking of historicism with empiricism and what they regarded as the speculative, unscientific rationalism of the preceding century. As Morpurgo-Davies reminds us, our histories of linguistics have taken that polemic on board to the extent that "sometimes they even give the impression of a science created ex nihilo". (1975:610) Campbell (in press:1f) observes that Sir William Jones was far from the first to observe the affinity of languages we now term Indo-European, or even to note that Sanskrit resembled Greek and Latin. Yet the existence of numerous precursors does not detract from Jones' place in history, unless it can be shown that it was their work, and not Jones', that was the impetus for the active interest in Sanskrit and Indo-European studies in Europe at the beginning of the 19th century. As I recall from history lectures, it really doesn't matter whether other Europeans preceded Columbus to America, if it was only after his voyages that Europe took an interest. With respect to pre-19th century comparative linguistic scholarship, Morpurgo-Davies, citing Robins (1967:164-9), observes that "what characterizes early work is not so much lack of insight as lack of that continuity in scholarly production which characterizes the nineteenth century". (Morpurgo-Davies 1975:616)

But it is possible, indeed likely, that the hallmark of the first few decades of 19th century comparative linguistics was not the discovery of the Indo-European language family, whose broad outlines had been intuited by previous generations of scholars, but the increasingly detailed comparison of the grammars and lexica of those languages. It was, in short, the developing methodology of comparative historical linguistics, what came to be called the comparative method, that was novel then, and is important now.

In her discussion of the evidence used in genetic arguments in the early 19th century,

Morpurgo-Davies is careful to distinguish three types: "lexical and structural similarity, and 'letter permutations' (i.e. sound correspondences)" (op. cit.:628). If for most of the major players, grammatical evidence was paramount, for the reasons discussed above, "even in the golden period of structural similarity, lexical correspondences were by no means neglected" (op. cit.:625). With reference to the work of that period, Morpurgo-Davies distinguishes lexical similarity from sound correspondences in a way that most of us would probably not because at that time it was only beginning to be clear that inspectional similarity was not sufficient to establish etymological relatedness. There must have been a considerable amount of license in the use of 'letter permutation' in some work to account for Schlegel's rejection of the practice. As noted earlier, Schlegel too accepted those correspondences that could be shown to be a 'general analogy'. Even disregarding the interest in structure that dominated the thinking of the period, we should not be surprised that so many scholars gave a subordinate role to lexical similarities, in an environment that permitted parts of words to be identified as 'roots' and that allowed 'letters' to be altered in haphazard fashion to produce an etymology. Many of the scholars of the period, Schlegel, Rask, and Grimm, for example were beginning to understand that such excesses could be controlled by insisting on regular correspondence over complete words.<sup>51</sup> Thus Rask writes:

"A language, however mixed it may be, belongs to the same class of languages as another, when it has the most essential, concrete, indispensable and primary words, the foundation of the language, in common with it...When in such words one finds agreements between two languages, and that to such an extent that one can draw up rules for the transition of letters from one to the other, then there is an original relationship between these languages; especially when the similarities in the inflection of languages and its formal organization correspond..."(Lehmann 1967:32)

In her assessment of the leading figures of early 19th century, Morpurgo-Davies (1975:630) reminds us that alongside those individuals like Schlegel, Bopp, and Humboldt, whose primary interest and source of evidence was grammar, are Rask, Grimm, von Raumer, Pott, and Schleicher whom she regards as 'phonetically minded'. Despite the deference often given to grammatical criteria in the writings of most of the major scholars of the period, few of them<sup>52</sup>, even Schlegel, showed no interest in sound correspondences. To argue that the use of sound correspondences as evidence in genetic arguments only began in mid-century is to grossly distort the history of the period.

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51 As already observed in footnote 3 above, this reference to 'regular' correspondence is not an anachronism, referring to Neogrammarian doctrine. Interest in regular patterns of correspondence antedated by a considerable period the Neogrammarian claim that sound change is exceptionless.

52 Of the major figures, Bopp is perhaps the one most focussed on morphology, because his main interest was in explaining the development of the Indo-European inflectional system. As Pedersen (1924:243) notes, when Bopp left that arena he frequently fell into error.

## 6. Nichols on Diagnostic Evidence

Nichols (1996) defines a notion she terms *individual-identifying* feature or evidence. It is in effect a statistical interpretation of the standard view of Meillet's *fait particulier*, roughly equivalent to shared aberrancy. Saying that evidence is individual-identifying "amounts to saying that all the languages having it have acquired it, ultimately, from a single source." (Nichols 1996:50) The classification does not distinguish inherited from borrowed features, Nichols admits, so fails to address the one possible account of cross-linguistic similarity that comparative linguists have been at greatest pains to eliminate.

What specifying a property as individual-identifying does tell us is that, within accepted limits of statistical tolerance, two languages are unlikely to share that feature by chance. Nichols assumes that over time the number of languages has been stable and is roughly 1000. If we set our threshold for chance at one in one hundred (a common statistical assumption), then a feature is individual-identifying if its probability of occurrence is less than  $p$ , where  $p \cdot 1000 = 0.01$ ; that is, less than 0.00001 or one in one hundred thousand.

Different factors can contribute to the ultimate evaluation of a property as individual-identifying. An individual lexical item is individual-identifying if it is sufficiently long. This is true of Indo-European *\*widhewa* 'widow', which Nichols describes as having the four consonants *\*w*, *\*y*, *\*dh*, and *\*w*, in that order.<sup>53</sup> Assuming as she does an average consonant inventory of 20, then the probability of any single consonant appearing at a given position in a word is .05, and of a sequence of four particular consonants as the product of that probability; here,  $0.05^4 = 0.00000625$ , or approximately one in one hundred thousand.

For Nichols, more typical cases of individual-identifying features involve oppositions in lexical or morphological paradigms; for example, the stem alternation in the positive and comparative of *good* in Germanic, instantiated in English as *gʊd~bet*. She represents the alternation as two independent CVC sequences in a two-member paradigm, with a probability:  $(0.05 \cdot 0.2 \cdot 0.05)^2 \cdot 0.5 = 0.000000125$ , or one in one million. Nichols stresses that it is the paradigmatic relation of these forms that is individual-identifying, since each CVC form alone has a probability of 0.0005.<sup>54</sup> Given her various assumptions, that means

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53 The example comes from Meillet (1925:51f), who does not provide a reconstruction (Pokorny (1958) reconstructs *\*widhewā*), and who gives the relevant segments as *w*, *i*, *dh*, and *u*. The difference between consonant and vowel is important, since Nichols assumes an average vowel inventory of five, given a probability of 0.2 for any particular vowel. An anonymous reader suggests that Nichols' *\*y* in this etymon is to be interpreted 'as a phonemicization', though I myself would query any phonemicized reconstruction that failed to preserve syllabicity unless there was good reason to assume that the syllabicity of the segment in question was variable. Since this is Nichols' only purely lexical example, I use it here in the form she gives it for purely illustrative purposes.

54 Most of the paradigmatic examples Nichols cites are cases of which Meillet would approve, in which particular substantive morphemes are in a paradigmatic relation. (In one case she evaluates, involving nominal gender correspondence in Afroasiatic, it is not made clear whether it is forms and meanings, or meanings alone, that are being compared.) In fact, she labels individual typological properties like ergativity

that one percent of the world's languages might have such a form by chance.

What might not be immediately obvious to the statistically-challenged (amongst which number I confess to falling) is why, in assessing the probability of the *gʊd~bɛt*- alternation, such factors as the number of morphemes in the language and the proportion of those that are CVC are irrelevant. This is because this pair is a closed, isolated set linked by membership in a two-member paradigm within which each CVC has an equal chance of filling each slot. The paradigmaticity is thus crucial, as is the uniqueness of the paradigm in being associated with the meaning 'good'. By the same reasoning, the *widow* case is in effect a one-member set defined by a particular sequence of phonological segments linked to a particular meaning. It is only rather late in her exposition, in §5.2 (pp. 61f), that Nichols makes this point clear.

On the issue of whether the number of languages attesting a property is relevant to that property's individual-identifying status, or on how many such properties are necessary to demonstrate genetic relatedness, Nichols appears contradictory. First she notes "that this procedure does not ask what the probability of finding some form (or set of resemblant forms in two languages, or three, or two out of three, or five out of ten, etc. ... Here the only point at issue is how good the form (or paradigm) is as a piece of diagnostic evidence, and not how many languages exhibit it. Nor will it be asked here how many pieces of individual-identifying evidence it takes to establish such relatedness (though it can at least be noted that well-established families have several to many in the realm of grammar, and very many in the lexicon). The only thing that matters here is how good the particular piece of evidence is." (Nichols 1996:49)

Subsequently she demurs. "Suppose a grammatical subsystem of individual-identifying value has been discovered and a set of languages has been identified as possibly related. In how many daughter languages or branches must the system be attested in order for it to confirm genetic relatedness?", she asks (op. cit.:60) Her answer does not really matter, because it is the context of the question that is revealing, as provided by the immediately preceding paragraph, where she observes, with reference to her argument thus far: "For Indo-European, it was possible to ascribe individual-identifying levels of probability to protoforms and reconstructed grammatical subsystems. Matters may be less precise and less straightforward for less precisely reconstructed protolanguages." (loc. cit.) Here the value of Nichols' statistical metric for individual-identifying properties is finally revealed. In a real sense, it works only for *reconstructions*, which are unique in the sense she requires. Given a reconstruction, we don't have to answer such nasty questions as how similar two

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or nonconfigurationality as *type-* rather than *individual-identifying*. Yet she gives them that label in isolation. One is left to wonder whether, just on the numbers, a particular constellation of schematic properties, like the existence of a paradigm with two genders, four numbers, and five cases, but with no phonological substance, might nonetheless be individual-identifying.

forms and two meanings have to be for us to consider them *the same* in the intended sense. or how much agreement there has to be in the number and function of the paradigm parameters in two languages for us to consider them the same paradigm. In essence, all that Nichols provides us is a measure of just how impressed we should be by any particular correspondence, *after* we have already used the standard comparative method, in my sense of the term, to decide that the correspondence is valid and thence to reconstruct from it.<sup>55</sup> Her procedure does not replace the standard comparative method in any way; it only serves to evaluate its results quantitatively. In effect, she provides a statistically measure of why the standard comparative method works.

## 7. Summary

In the preceding discussion I hope to have demonstrated that in genetic linguistic arguments there is no substitute for the standard comparative method, for finding regular sound correspondences in forms whose meanings, in the absence of a theory of semantic change, we are willing to accept could plausibly have developed from a single meaning. Given our current understanding of language and of language change, only the standard comparative method provides a metric for deciding just how similar two constructions have to be to count as evidence of genetic relatedness.

Some early 19th century comparativists (Schlegel coming immediately to mind) may have had an almost mystical reverence for and interest in grammatical data as indicative of the essence of a linguistic system. Yet, for most of the leading figures in comparative historical linguistics, the importance of grammatical evidence was neither mystical nor mathematical. If many of them have asserted that substantive constructions with *grammatical* meaning are better evidence of genetic relatedness than are *lexical* similarities, then it was either because the lexical similarities they had in mind were of the crude, unconstrained, inspectional sort of which we have every right to be suspicious, or because they doubted that borrowings could be distinguished from directly inherited items and believed that grammatical morphemes cannot be, or at least are less likely to be borrowed.

Meillet was been particularly misunderstood in this regard. His singular facts are not shared aberrancies. A linguistic fact does not have to irregular to be singular; it is

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<sup>55</sup> It might be worth noting in this context that truly accidental similarities can appear individual-identifying by Nichols' metric. My favorite example is English *paddle* and the word *padil* /patil/ 'paddle' in the Ponapeic languages of Micronesia. That there is no relationship whatsoever between these forms is clear from cognates for *padil* in other Micronesian languages, e.g. Trukese *fétún* /fət+n/ 'paddle'. (Voicing is not contrastive for consonants in Ponapeic; intervocalic consonants are often phonetically voiced.) Counting only the three consonants (voicing being non-contrastive for Ponapeic consonants) and the low vowel, and using Nichols' average inventories, we obtain  $0.05^3 \cdot 0.2 = 0.00025$ . This probability just misses being individual-identifying at the 0.01 level and is individual-identifying at 0.05. Of course no one, not even Nichols I venture, would suggest a historical connection between the English and the Ponapeic words, precisely because the sound correspondences belie any such connection.

sufficient that it be specific and arbitrary, and not vague, general, or iconic. If we are particularly impressed by cross-linguistic correspondences in alternations *est~sunt* or *good~better* it is, as Nichols observes, because we are surprised by suppletion in paradigms.<sup>56</sup> But our degree of surprise does not thereby absolve us from the requirement that we establish the cognacy of the individual paradigm members first, through regular sound correspondences. As a good Neogrammarian, Meillet understood that. He understood the need to identify similarities in both form and meaning, the former through regular sound correspondences and the latter, rather more problematically, from our experience with language and our knowledge of the world. It is then only fitting that Meillet should be given the last word on the subject:

"Everywhere where the phonetic system and the grammatical system show precise correspondences, where regular correspondences permit the recognition of the single origin of the words and of the phonetic system and where the system of grammatical forms is explained from a point of common origin, [genetic] relatedness is obvious."<sup>57</sup>  
(Meillet 1965:88)

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56 In a discussion of teleological arguments in historical linguistics, McMahon (1994:331), citing Lass, notes that individual sound changes (as opposed to more general analogical levelling) in the history of English seldom seem to have operated so as to reduce allomorphy. If that observation is more generally true, perhaps we have less right to be surprised by suppletion than we would generally believe!

57 "Partout où le système phonétique et le système grammatical présentent des concordances précises, où des correspondences régulières permettent de reconnaître l'unité d'origine des mots et du système phonétique et où le système des formes grammaticales s'explique en partant d'un original commune, la parenté est évidente."

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