

## Meillet's Magic Bullet: On the Logic of Genetic Arguments Using Morphological Evidence

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### 1. Introduction

It is commonplace in the field of comparative-historical linguistics to find assertions, in textbooks, in areal surveys, and in reports of new comparative research, to the effect that certain sorts of morphological evidence have pride of place in determining genetic relatedness. The nature of these claims varies. The most frequently encountered are those like the following:

"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)

Others claims are more general:

"It is generally agreed that shared morphology is the surest proof of genetic relatedness. Phonemes have no meaning in themselves and their organisation into systems and their phonetic realisations 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)

Such differences in generality are perhaps hardly surprising, since what it is that makes one morphological fact more anomalous than another is seldom made explicit.

The value of morphological similarity in determining genetic relatedness has been asserted since the very outset of the modern comparative-historical enterprise. Thus, in his *Investigation on the Origin of the Old Norse or Icelandic Language* (1818), Rasmus Rask wrote:

"When agreement is found in such words in two languages, and so frequently that rules may be drawn up for the shift in letters from one to the other, then there is a fundamental relationship between the two languages; especially when similarities in the inflectional system and in the general make-up of the languages correspond with them."<sup>1</sup>

Such early references are of antiquarian interest only. Claims regarding the significance of morphological evidence take on their modern significance only with the development of the *comparative method* (fully articulated in the 1870's) and the recognition of the method's pre-eminence as a means of determining genetic relatedness. Antoine Meillet (1866-1936) is the individual most often credited with stressing the importance of morphological

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<sup>1</sup>Cited in Pedersen 1931 (1924):251.

evidence in genetic arguments in this modern context,, in work published in the early decades of the 20th century.

In this paper, we investigate what Meillet actually said on the subject, and attempt to put his work in its proper historical context. We explore the logic of arguments for the evidential primacy of morphology, over other sorts of evidence for genetic relatedness, and conclude that such arguments are circular.

The position we adopt regarding morphological evidence is similar to that put forward 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. ..."

If Hock is correct that only those aberrancies reflecting what the comparative method determines are regular sound correspondences count as evidence of genetic relatedness, then one must conclude that such aberrancies are no *magic bullet* for determining genetic relatedness. Logically, they are of no particular evidential value at all.

In section 2, we review the logic of comparative method, with particular emphasis on its *semiotic* preconditions and *regularity* assumption. Section 3 deals with the importance of morphological evidence for determining genetic relatedness, as presented in the writings of Antoine Meillet. In section 4, we evaluate a particular instance of the use of morphological evidence in a genetic argument -- Blake's (1988) argument that the Tangkic languages are not Pama-Nyungan. In section 5, we evaluate the status of morphological arguments for genetic relatedness in general.

## 2. The Comparative Method

The *comparative method* for determining genetic relatedness amongst languages is the most important product of 19th century comparative-historical linguistics. It continues to have pride of place amongst comparativists, but many of its practitioners don't seem to fully appreciate how and why it works, and as a result do not fully appreciate its centrality or understand its limitations.

The comparative method is just that, a *method* or *procedure*, It is a method for determining genetic relatedness (and only incidentally and secondarily for making reconstructions on the basis of such determination). Crucially, the comparative method must not be confused with the problem it is intended to solve. It is but one means of determining of genetic relatedness. It need not be the only means; there may be other ways of ascertaining whether or not two or more languages are genetically related, and whose methods might be different in important respects from the comparative method itself.

One alternative to the comparative method is the *mass comparison* technique associated with the work of Joseph Greenberg. By that technique, one simply counts lookalikes in relatively large data sets across a substantial sample of languages over a large geographical area. If there are enough lookalikes across that range of languages, then

that fact is unlikely to be the result of either chance or borrowing. Greenberg's logic was not immediately dismissed because the first case to which it was applied, Afro-asiatic (Greenberg 1950), was a family that many scholars familiar with the languages in question were prepared to accept<sup>2</sup>. Greenberg's attempt to apply mass comparison to Amerindian languages (Greenberg 1987) was met with almost universal outrage amongst Amerindianists.

At the outset, though, all genetic arguments are the same. They must all begin with *observations of similarity* amongst the set of languages whose genetic relatedness is at issue. If those languages are indeed genetically related, then the argument will proceed, via some defensible set of steps, from those observations of similarity to the conclusion that those similarities are (most likely to be) the consequence of inheritance from a common ancestor.

The logic of a proof of genetic relatedness, via the comparative method or any other means, can be viewed as a classical *disjunctive syllogism*. In a genetic linguistic argument, one enumerates all the logically possible accounts of some instance(s) of similarity, and eliminates each in turn until only one remains, that of inheritance from a common ancestor.

Those logically possible accounts of similarity between languages are the following:

- i. similarity due to nature (in this case, the nature of human language)
- ii. similarity due to chance (that is, accidental separate development)
- iii. similarity due to borrowing or diffusion
- iv. similarity due to inheritance

If one can rule out (or, at the very least, render unlikely) accounts of similarity based on nature, on chance, or on borrowing, then one has licence to conclude that the similarity must be due to inheritance, and that the languages sharing that similarity are genetically related.

### *2.1 Natural Similarity and the Semiotic Restriction on Comparison*

Natural languages are sign systems. The expressions that make up a language have both a form (the *signans*) and a function/interpretation (the *signatum*). It is this essential semiotic aspect of language that permits the elimination of *nature* as an account of similarity. If one admits that for some linguistic signs, the relation between form and interpretation is *natural*, but for others it is non-natural, one need simply restrict attention to similarities between *non-natural* signs.

The late 19th century philosopher Charles Sanders Peirce developed a rich typology of signs, applicable to both linguistic and non-linguistic systems. His best known trichotomy classifies signs according to their 'fitness to signify'. An **indexical** sign is fit to signify by virtue of a real material relationship between *signans*, and *signatum*. In an indexical relationship, the *signans* is *part of* (or follows from) the *signatum*. An **iconic** sign is fit to signify by virtue of a resemblance between *signans* and *signatum*; the *signans* *looks like* the

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<sup>2</sup>For some of us, though, the jury is still out on Afro-asiatic (in its fullest proposed extent at least). Its strongest support comes from recent work by Ehret (1995), work that uses the comparative method.

signatum. Indexical and iconic signs are both natural, in that some aspect of the nature of the signans makes it fit to stand for its signatum. A **symbolic** sign, by contrast, is fit to signify only because some (community of) sign user(s) deem(s) it fit. The relationship between signans and signatum in a symbolic sign is non-natural; it is arbitrary and conventional. Therefore, as long as we restrict attention to symbols, we have eliminated nature as an account of similarity.

There is general consensus that ordinary lexical items, like *skrika* 'cry' and *häst* 'horse' in Swedish and *shriek* and *horse* in English, are symbolic signs, and so are admissible objects for comparison. There is less agreement regarding what linguistic objects are inadmissible by virtue of being natural signs.

Most linguists would readily (though perhaps not entirely accurately) identify onomatopoeic forms like *woof* and *meow* as iconic. Such cases are far from the best examples of iconism in natural language. A much better example is the use of reduplication to express such concepts as plurality or ongoingness. The icon here is diagrammatic: repetition in form stands for repetition in interpretation.

Ind	<i>anak</i> 'child'	<i>anakanak</i> 'children'
Mok	<i>kijou</i> 'run'	<i>kijkijou</i> 'keep on running'

Such diagrammatic **iconicity** is the essence of grammar, of *paradigmatic* and *syntagmatic* relations amongst (sets of) linguistic expressions. **Syntagmatic** relations are iconic to the extent that they are *compositional*; to the extent that the interpretation of the whole is a function of the interpretations of its parts, and their mode of composition. For any phrase token, the fact that its components are contiguous and/or overtly marked in some analogous fashion is diagrammatic of the fact that those components are understood together. The diagrammatic nature of syntax is perhaps clearer for (phrasal) category types, for the generalisations/rules 'describing' sets of expressions exhibiting the same syntagmatic relations. The (abstract) form<sup>3</sup> of a syntagm is a diagram of its compositional interpretation.

If these rules and categories follow from universal constraints on language, one might even argue that they approach **indexicality**; the rules stand for sets of linguistic expressions because they *are* what the human linguistic capacity stipulates (what is common to) those sets of expressions to be.

**Paradigmatic** relations are similarly iconic, both at the token and the type level:

1. To the extent that members of a set of complex (lexical) signs, like an inflectional or derivational paradigm, are similar in meaning/function, it is natural that they be similar in form. The similarity in form is a diagram of the similarity in function.
2. The parameters that define a paradigmatic class are a diagram of, or a metaphor for how the corresponding class of referents function in the world. To the extent that paradigm structure is constrained universally it too might be viewed as **indexical**.

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<sup>3</sup>Syntactic rules are linguistic signs in the same sense as are particular phrase tokens, differing only in level of abstraction. This insight is made particularly clear in Head-driven Phrase Structure Grammar.

By restricting comparison to symbols, to arbitrary associations of form and meaning, one can obviate the possibility of natural similarity. The comparative method, as a method for determining genetic relatedness, accepts as data for comparison only lexical tokens, which have been recognised as arbitrary and conventional at least since Saussure and perhaps as far back as the time of the Alexandrian anomalists.

Note, though, that this lexical restriction does not mean that one cannot compare complex signs, so long as one treats them as wholes. After all, many words are morphologically complex. But as data for the comparative method, one either erases the morpheme boundaries, or compares the morphemes individually, treating them (rather than the words they comprise) as the objects of comparison. Note also that this semiotic restriction does not mean that one cannot compare abstractions on (diagrams of) complex linguistic expressions, and reconstruct abstractions thereon. But it does mean that one can draw no genetic inferences from such comparison, and that any reconstruction based thereon is meaningless unless the languages in question have first been shown to be related on the basis of symbolic, lexical comparison.

## *2.2 Chance, Borrowing, and the Regularity Assumption*

The possibility that observed similarities are due to chance is at the very least reduced by the comparative method's assumption that sound change is *regular*. In this context, *regular* is understood as meaning that a sound change affects all tokens of the appropriate phonetic type, for all speakers in the speech community, simultaneously. When comparing forms with similar meanings in different languages, it follows from the regularity assumption that one expects to find *patterns* of phonological similarity (or, perhaps more accurately, of difference), *if* the languages compared are indeed genetically related and if the forms descended from a single ancestral form. The regularity assumption tells us that we cannot draw a genetic inference from just any random observation of similarity in the form of linguistic signs; the similarities must be patterned and regular. If observed similarities are due to chance, then one does not expect to find those similarities to be a function of regular patterns of correspondence.

Our choice of the term *assumption* to describe *regularity* is deliberate, because the regularity hypothesis is demonstrably false. Sound change is not regular in the intended sense. Rather, it proceeds through the lexicon<sup>4</sup> and through a speech community over time. But it really doesn't matter in most cases in which one appeals to it that the assumption is false, because given enough time, sound change gives the appearance of regularity. Most sound changes will, over time, encompass most of their token domain and most of the speech community using that domain. The consequences of *not* applying the regularity assumption are, however, catastrophic, since without it we cannot hope to distinguish similarities due to inheritance from those due to chance<sup>5</sup>.

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<sup>4</sup> We intend this to mean that a sound change that begins in a highly restricted phonological environment may move toward categoriality over time. We do not intend our claim as a ringing endorsement of lexical diffusion.

<sup>5</sup>Harrison (in press) argues that the regularity assumption serves another important function in the comparative-historical enterprise. It serves as a stand-in for a theory of phonetic similarity. Armed with the regularity assumption, one need not have to answer the question: "How similar must two forms be to be considered similar?" The regularity assumption says that two forms are similar if they can be reconciled through regular sound correspondences.

The regularity assumption also serves to eliminate cases of similarity through borrowing, insofar as we do not expect the patterns of similarity found in borrowings to be regular. Problems arise in recognising borrowing only in those cases in which the borrowing has been on so massive a scale that there is no great token disparity between those correspondences due to regular sound change and those due to borrowing. But one should be cautious about overestimating the comparative problems arising through large scale borrowing. Two Oceanic cases, that of New Caledonian languages and that of Rotuman, point out the difference between those cases of massive borrowing that pose principled problems for the comparative method and those that do not.

In the case of Rotuman (as reported in Biggs 1965), 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 the existence of two distinct sets of sound correspondences for Rotuman and other Oceanic languages, sets that Biggs noted were overwhelmingly mutually exclusive by lexical item. The problem was to explain away one of those sets, which Biggs did 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 be the inherited set.

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) 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 important for our purposes is Grace's (1990) account of *how* the New Caledonian situation arose. One account rests on the fact that sound change is not in fact regular, so that what we observe here 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.

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The analogous question might be asked about meanings as well: "How similar must two meanings be to be considered similar?" Here, the comparative method has nothing to offer, and nor do any developments in semantic theory since the 19th century. In practice, if one can construct a plausible metaphoric association between two meanings, they are similar enough. Semantic similarity remains the soft underbelly of the comparative method.

It is in cases such as these, and not the perhaps more common Rotuman-type cases, that the 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 comparative method simply has nothing to say on the matter.

It is often claimed, however, that particular sorts of forms, though perhaps not immune from borrowing, are so resistant borrowing that their similarity can be used as evidence of genetic relatedness even when the comparative method would otherwise fail. They provide a sort of magic bullet, *Meillet's magic bullet*, for demonstrating genetic relatedness. It is to the nature and utility of this putative magic bullet that we now turn.

### 3. Meillet's Magic Bullet

We must first clarify precisely what Meillet had in mind with regard to:

- i. the scope of morphology
- ii. the kind of morphological evidence of use in genetic arguments
- iii. the reason that evidence is necessary
- iv. the justification for believing the evidence serves the need

#### 3.1 *The Scope of Morphology*

Meillet may not have understood the term *morphology* to mean what it is typically taken to mean today; that is, the structure of, and relationships among words, with respect to their meaningful/functional components. Meillet appears to have understood *morphology* rather more broadly, in a sense familiar to those who remember the orthodox American structuralist view that morphology and syntax were not particularly distinguishable:

"Morphology, that is, the set of processes by which words are modified and grouped to form sentences is the most stable thing in the language." (1967:36)

It is clear, however, that Meillet did not believe just any grammatical (non-lexical, non-phonological) similarity between languages is potential evidence of genetic relatedness. He was critical of appeals to broad typological similarities, such as strict suffixation and vowel harmony, in arguments for a genetic relationship between of Finno-Ugric and Turkic (1965:90-91), or to demonstrate the kinship of English and French by noting similarities in word order or in the use of prepositions to mark grammatical relations.

Meillet rejected such arguments on the basis of what Harrison (in press) refers to as the *poverty of choice* argument. In the domain of word order, that argument points out that there are precisely  $n!$  possible orders in which any  $n$  linguistic objects can occur. For  $n \leq 3$ , that number is too small to rule out chance as an account of similarity. But for  $n > 3$ , the number of permutations increases sharply, and one might well begin to question the possibility of chance. Hymes (1955, 1956) suggests that similar relative category order might be evidence of genetic relatedness for this reason<sup>6</sup>. The argument fails in general, but that failure is not due to the temporal instability of such ordering constraints, as

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<sup>6</sup>We are grateful to Lyle Campbell for pointing out these references to us.

Campbell and Poser (in prep) suggest, but because of their *iconicity*. 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<sup>7</sup>.

### 3.2 Singular Facts

The morphological evidence of interest to Meillet is what he terms *des faits particuliers*, usually rendered in English as 'singular facts' or 'particular processes'. Nowhere to our knowledge does he explicitly define what he means by this term, except by exemplification. As best as we are able to determine, for Meillet a *singular fact* is some symbolic feature of a particular language that is relatively isolated (perhaps anomalous or irregular) within the grammatical system of that language. On the basis of the examples he gives, singular facts may be either affixal or non-affixal, and have a grammatical/functional rather than a lexical/contentive interpretation. He specifically includes what he terms "non-meaningful grammatical forms" (Meillet 1967:42) such as exponents of grammatical gender.

The following quote is representative of Meillet's views on the nature of 'singular facts of morphology':

"What conclusively establish the continuity between one *common language* 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." (1967:39-40)

(Meillet uses 'common language' where we would now be use 'proto-language'.) That he intends these 'singular facts' to be *symbolic* (non-natural) is clear from the following:

"On the basis of the method it follows that probative facts in the domain of comparative grammar are singular facts, and they are all the more probative in that, by their very nature, they are less suspect of admitting of a general cause. There is nothing natural in them; since it is a matter of demonstrating by comparative means the historical fact of the existence of a particular language, that is to say something that, by definition, is the outcome of a set of diverse circumstances having nothing necessarily to do with one another, these alone are the facts, singular in character, that should be considered."<sup>8</sup> (1965:24-25)

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<sup>7</sup>If choice within the range provided for a parameter by the nature of language is arbitrary, then the *poverty of choice* argument can be brought into play.

<sup>8</sup>Where the translations are our own, we include the French original:

"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

There is some evidence that Meillet considered morphology, in the sense of affixal morphology, to have any particular pride of place as genetic evidence. He states:

"... 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 surpasses 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.** (emphasis ours)"<sup>9</sup> (1965:93)

What Meillet does seem to stress as evidence, though as we have noted not exclusively, are what have come to be termed **shared aberrancies**. The particular case most often cited is 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 e-grade stem allomorph in the singular and the Ø-grade in the plural. 'Singularity' is apparently not an absolute, but a gradient, as Meillet observes:

"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.**" (emphasis ours)

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 1967:41).

We take up the question of what makes an aberrancy aberrant in section 3.4.

If two languages share a singularity, then Meillet appears to regard *that* fact as particularly good evidence of genetic relatedness. Assuming that there is reasonable agreement regarding what facts are singular (see section 3.4), the utility of these facts as genetic

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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 qui doivent seuls entrer en considération."

<sup>9</sup>"...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."

evidence turns on how one goes about recognising a shared singularity. It is there, we argue in section 5, that the whole concept fails.

### 3.3 Borrowing

As we demonstrate in section 4, Meillet's magic bullet is often now used as a substitute for the comparative method, as a way of constructing arguments for genetic relatedness when the comparative method appears to have failed. It is not at all obvious, from the sorts of cases Meillet discusses and from the general context in which he introduces the question of 'singular facts' and 'anomalous forms', that he had such cases in mind.

It goes without saying that Meillet understood and adhered to the comparative method in its strictest sense, as evidenced by such statements as:

"It is not on the external similarity of words that linguists rely in order to reconcile and to establish the etymology of those words, but on patterns of regular correspondence. ... Those patterns, once established, permit recognition of the broad outlines of the history of pronunciation and to work out how phonetic systems have been replaced by others in related languages."<sup>10</sup> (1965:93)

Meillet's concern was the possibility of **borrowing**, the possibility that some perhaps substantial portion of the lexicon on the basis of which regular sound correspondences are established, and through which genetic relatedness is determined might, in fact, have been borrowed from one language into another, rather than inherited. Meillet appears not to have trusted that the comparative method itself will filter out borrowings, in the manner we have outlined in section 2 above:

"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.** (emphasis ours)"<sup>11</sup> (1965:91)

Or perhaps he simply wanted to obviate any attack by anti-comparative nay-sayers, in essence asserting that 'even if some of the cognate sets we have used are spurious, and the result of borrowing, we have the same correspondences in items most unlikely to have been borrowed, so our correspondences, and our genetic conclusions, remain valid'. We are putting words in Meillet's mouth, to be sure, but those words do not appear to be counter to the spirit of his work, since nowhere does he suggest that the comparative method is dispensable.

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<sup>10</sup>"C'est pas sur la ressemblance extérieure des mots que se fondent les linguistes pour les rapprocher et en faire l'étymologie, mais sur des formules de correspondences régulières. ... Ces formules une fois établies permettent de reconnaître dans ses grandes lignes l'histoire de la prononciation et d'établir comment les systèmes phonétiques se sont succédé les uns par les autres des langues apparentées."

<sup>11</sup>"...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 prouve jamais d'une manière absolue, parce qu'on ne peut jamais affirmer qu'elles n'expliquent pas par des emprunts. ..."

### 3.4 *The Singularity of Singular Facts*

In the following remarks concerning the 3rd singular forms of the Indo-European verb 'to be' noted above, Meillet comes as close as he ever does, to our knowledge, to explaining what makes a singular fact singular.

"For so singular a state of affairs is the result of the confluence of a whole series of circumstances: the expression of the verb 'to be' by *es-*, of the 3rd singular by *-ti* and the 3rd plural by *-enti* or *-onti*, the use of the stem alternate with *e* in the singular and without *e* in the plural; it is improbable that any of these circumstances viewed separately arose identically in two languages independently, more improbable still that these four circumstances are found simultaneously at a given moment independently in two different languages." <sup>12</sup> (1965:25)

He does not seem to us to be particularly clear or convincing. Does the singularity of the four observations he presents rest on the fact that they involve two pairs of bound morphemes, or would any four words/morphemes do? Is it important that one pair are person/number agreement markers and/or that the other involves the copula?

Taken apart from their status as grammatical suffixes, there is in fact nothing particularly singular about these reflexes of PIE *\*-ti* and *\*(e,o)nti* so far as we are aware<sup>13</sup>; any more than are the reflexes of PIE *\*pa4te4%r* 'father' in Classical Greek or in Latin. The following remarks, from *Les Parentés des Langues*, on the difference between *vocabulaire* 'lexicon' and *morphologie* 'grammar' serve only to muddy the waters further.

"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 *tu* 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 *tu*, 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>14</sup> (Meillet 1965:107-8)

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<sup>12</sup>"Car un état de choses aussi singulier résulte du concours de toute une série de circonstances: l'expression du verb *être* par *es-*, de la troisième personne du singulier par *-ti*, et de la troisième du pluriel par *-enti* ou *-onti*, l'emploi d'une alternance de forme radicale avec un *e* au singulier et sans *e* au pluriel; il est improbable qu'aucune de ces circonstances considéré à part vienne à se reproduire exactement dans deux langues d'une manière indépendante, plus improbable encore que ces quatre circonstances se retrouvent simultanément à un moment donné d'une manière indépendante dans deux langues différentes."

<sup>13</sup>Neither of this paper's authors claims to be an Indo-Europeanist; if there is something truly odd about these two forms, we are not aware of it.

<sup>14</sup>"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 2<sup>e</sup> personne est en latin *tu* est un fait de vocabulaire; en français actuel, *tu* n'est plus un mot autonome; ce n'est que la caractéristique de la 2<sup>e</sup> personne singulier des verbes. Du latin *tu*, qui était un mot autonome, au français *tu*, qui est un pur élément 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. ..."

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. And that is perhaps not so much a grammatical fact as a purely lexical one. What makes the similarities in the Latin and Sanskrit forms of *to be* more compelling than those in cognate forms of 'to carry' such as:

	3s	3p
Sanskrit	bharati	bharanti
Latin	ferit	ferunt

is not that the former are any more 'grammatical' or 'morphological', but that they are less compositional and more *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.

In his remarks on IE *be*, Meillet has presented an argument, convincing or otherwise, that similarities between what he regards as singular facts are unlikely to be due to *chance*.<sup>15</sup> He is rather less forthcoming about why he considers them resistant to *borrowing*, despite the significance he accords that problem. What we find in Meillet's writings are *assertions* regarding the stability of singular (morphological/grammatical) facts:

"Singular facts of this sort are often quite stable. ...a language with an involved and complex morphology, containing a large number of specific facts, lends itself well to the proof of relationships, whereas a language with a simple morphology, operating principally with general processes such as word order, makes the discovery of valid proofs difficult." (1967:39-41)

Over the past two decades, a large body of evidence (for example, Thomason and Kaufman 1988 and Campbell 1993) has been compiled to refute the strong claim that there are aspects of language immune from borrowing. But the fact that nothing is *immune* from borrowing/diffusion does not belie the probability that certain aspects of language are *resistant* to it.

We borrow, if that's the term, what we think we need. Let's consider a couple of pompous examples. Neither of the authors of this paper speak German, but one of them at least makes opportune use of the German word *Schadenfreude*. As soon as he encountered that word, he decided that it fit a very nice lexico-semantic hole in English. That same author is also fond of Latin plurals in words like *corpora*, *morae*, and *foci*, probably to show off a tarnished classical education. And in this way, foreign morphology enters English, with the roots to which that morphology is attached. But alongside these classical plurals are the nativised *corpuses*, *moras*, and *focuses*. There is

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<sup>15</sup>Meillet (1900) does address the possibility of the parallel independent development of some quite singular facts. He notes a grammatical example in Slavic, in which all modern Slavic languages have replaced the Proto-Slavic simple adjective declension by one originally involving an enclitic demonstrative. This chance appears to have occurred independently throughout the Slavic family. Meillet concludes "dans le cas où l'évolution a été sensiblement unique, le resultat est le même que s'il y avait eu unité dès le début ... Il y a là, pour la grammaire comparée, une difficulté fondamentale qui tient à l'essence même de sa méthode et qu'on n'a pas le droit de jamais perdre de vue." (1965:43) ("in the case where the development has apparently been indentical, the result is the same as if there had been agreement from the outset ... One finds there, for comparative grammar, a fundamental difficulty that affects the very essence of its method and that one never has the right to lose sight of.")

always pressure to adapt imported roots to native paradigms. Foreign affixes (particularly inflectional affixes) get lost for a variety of reasons, all well known:

- i. they may violate prosodic constraints in the target language
- ii. they may involve inflectional contrasts not made in the target language
- iii. few users of the borrowed forms know enough about the source language to know what category the affix is actually marking

and so forth. But none of these social or linguistic constraints is strong enough to rule out the possibility of borrowing. So we can never be sure.

We are thus not unwilling to accept the claim that some linguistic objects are more borrowable than others. We would argue, though, that the borrowability of singular facts is an irrelevancy, a red herring. In section 5, we conclude that there is no reason whatsoever to appeal to singular facts, morphological aberrancies, or whatever, in making a genetic argument. We will argue that the existence of such similarities alone, in the absence of evidence of regular sound correspondences, is meaningless, and, together with regular sound correspondences, irrelevant.

#### **4. A Smoking Gun**

We want to make it clear that we are not advocating the execution of a straw man. There are indeed published arguments of genetic relatedness that use the putative stability of grammatical/morphological facts as a sort of magic bullet, as a way of determining genetic relatedness in the absence of the regular sound correspondences demanded by the comparative method. We present an extended example from Blake (1988) involving the use of pronoun forms as evidence in deciding whether to assign particular languages to the Pama-Nyungan branch of a putative Australian family.

##### ***4.1 Pronouns as Subgrouping Evidence in Australia***

###### ***4.1.1 Background***

In Blake (1988), pronoun similarity is the principle evidence in arguments for reclassifying three groups of languages as  $\pm$ Pama-Nyungan. Here we consider only his argument that the Tangkic languages should be considered non-Pama-Nyungan rather than Pama-Nyungan. He takes for granted that each of Pama-Nyungan and non-Pama-Nyungan (Northern Australian) is a genetic unit, and are immediate daughters of Proto-Australian. By implication, his remarks about the position of Tangkic, based on pronoun comparison, uses those same pronoun comparisons as crucial evidence for the two competing subgroups themselves. Blake demurs on the issue of whether his two subgroups have a common (Proto-Australian) ancestor.

Why does Blake give such importance to pronouns? He begins by noting the problems involved in using lexical comparison in the Australian context:

"Lexical comparison can establish genetic distance providing we can distinguish borrowings from cognates. This is not always easy. In Australia it is rather more difficult to establish sound correspondences than it has been with Indo-European languages. The vocabularies that have been assembled tend to be small ..., the lexical

diversity between languages is considerable, and the phonological differentiation between languages is not very great. This makes it hard to find sufficient examples to establish a correspondence with a proper account of the conditioning involved ..." (1988:4)

The lack of a clear means for distinguishing borrowing leads him to decide that:

"In light of the problems of distinguishing cognates from borrowings it is natural to turn to other areas of language for evidence of genetic distance or subgrouping." (op. cit.)

He is at first cautious about the use of grammatical information in genetic arguments, noting that grammar can be calqued, and citing Australian evidence of indirect diffusion of switch-reference patterns in Australia (after Austin 1981). Nevertheless he takes the plunge:

"However, there is a generally accepted belief that function roots are relatively unborrowable. Where function roots are borrowed ... this implies large scale lexical borrowing. This suggests that a comparison of function roots would be the best guide for establishing genetic distance and strict subgrouping." (op. cit.)

Blake is careful to make reference to Heath's (1978) study of diffusion in the Australian context, noting that Heath had demonstrated that 'even function roots' have been borrowed. Nevertheless, Blake concludes:

"I do not think that Heath's findings render a comparison of function roots useless as a guide to establishing genetic relations, but they do suggest caution. Interestingly, Heath did not discover any significant borrowing of pronouns." (ibid.:5)

It is clear here that Blake accepts Heath's findings regarding the lack of pronoun diffusion in Arnhem Land as a demonstration that pronouns are especially impervious to borrowing and that as a result, and despite the risks, can serve in the Australian context at least, as the magic bullet.

#### 4.1.2 *The Pronoun Evidence*

The bulk of Blake's paper is a comparison of pronouns explicitly aimed at establishing genetic distance and strict subgrouping. Blake uses the pronominal evidence to make three revisions to the O'Grady et al. (1966) lexicostatistical classification of Australian languages, and derives a new classification of languages which Dixon (2001, 2002) terms *Pama-Nyungan mark II*.

In this context, it is worth considering what Heath actually has to say about the apparent lack of pronoun borrowing in Arnhem Land. Heath (1978:105) provides a table of the categories investigated in his study. These categories fall into two columns headed "Diffusible" and "nondiffusable" [sic]. Independent pronouns (along with bound pronominals, verbal inflectional affixes, demonstrative stems and demonstrative adverbs) are listed in the "nondiffusable" column. However, Heath then seeks to provide some general explanations for why certain categories fall into one column or the other. It is

precisely this discussion which is widely cited in the literature on the borrowability of morphosyntax. On independent pronouns, Heath writes:

"In general, the factors hindering diffusibility seem only weakly applicable to independent pronouns. ... these considerations do not suffice to rule out the possibility of diffusion of independent pronouns. Although no clear examples of borrowing of independent pronouns occur in the area investigated here, we should certainly keep our eyes open for possible instances as we obtain data from other languages in the same general region." (1978:113-114).

Notwithstanding Heath's caution, Blake is content to use shared pronoun forms as indicative of (close) genetic relationship. His survey of non-Pama-Nyungan (Northern) pronoun systems produces a set of possible reconstructions. These are compared with his assumed reconstructions for Pama-Nyungan languages in Table 1.

	<b>Northern</b>	<b>Pama-Nyungan<sup>16</sup></b>
<b>1</b>	*ngay	*nga-y
<b>1e du</b>	*nyi-rrV	*(ngali)
<b>1e pl</b>	*nyi-rrV	*nga-na
<b>1i</b>	*nya	*ngali
<b>2</b>	*nginy	*ngin
<b>2du</b>	*nu-rrV, *ku-rrV	*nyu-NpalV
<b>2pl</b>	*nu-rrV, *ku-rrV	*nyu-rra
<b>3m</b>	*nu	*nyu
<b>3f</b>	*ngaya	*nyan
<b>3du</b>	*pu-rrV	*pu-la
<b>3pl</b>	*pu-rrV	*tya-na

**Table 1**  
**Australian Independent Pronoun Reconstructions**  
**(after Blake 1988:19)**

Against the background of these reconstructions, Blake concludes that the Tangkic languages (including Lardil, Kayardild, Yukulta and Yangkaal), previously classified as Pama-Nyungan, should be considered non-Pama-Nyungan (Northern). Relevant pronoun forms for Lardil, Kayardild, and Yukulta are presented in Table 2. Forms in parentheses are the oblique stems:

	<b>Lardil</b>	<b>Kayardild</b>	<b>Yukulta</b>
<b>1</b>	ngata (ngitha-)	ngata (nhityin-)	ngata (ngityi-)
<b>1e du</b>	nya(rr)-	ngarra	ngarra
<b>1e pl</b>	nyal-	ngalta	ngala
<b>1i du</b>	ngaku-	ngakurra	ngakurra
<b>1i pl</b>	ngakul-	ngakulta	ngakulta
<b>2</b>	nyingki (ngimpe-)	nyingka (ngumpan-)	nyingka (ngumpa-)
<b>2du</b>	kirr-/nyi-	kirra	kirra

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<sup>16</sup>This reconstructions are from Dixon (1980), who attributes them to Proto-Australian. Blake suggests that Dixon's Pama-Nyungan data bias makes them more appropriate to Proto-Pama-Nyungan. In these tables and the related discussion, N represents a homorganic nasal and V a vowel of indeterminate quality.

2pl	kil-	kilta	kilta
3	niya	niya	niya
3du	pirr-/ni-	pirra	pirra
3pl	pil-	pilta	pilta

**Table 2**  
**Tangkic Independent Pronouns**  
**(after Blake 1988:66)**

Blake argues that these paradigms show four characteristics distinctive of the pronoun forms reconstructed for non-Pama-Nyungan (Northern) in Table 1:

- i. the 2nd nonsingular \*ku reflected as *ki*
- ii. the nonsingular marker \*rrV reflected as a dual *rr(a)*
- iii. the Lardil 1e nonsingular base *nya*, which may reflect the Northern \**nya* '1i' or \**nyi* '1e plural'
- iv. Tangkic *nyiN*- '2s nominative' (possibly Pama-Nyungan or Northern) alternating with *nguN/ngiN* '2s oblique', where the suppletive alternation is a distinctly northern phenomenon
- v. the absence of the distinctly Pama-Nyungan forms for 1i, 2du, 3du, 1pe, and 3p in Tangkic

Note first that these observations are Blake's, and not ours. Furthermore, to understand Blake's reasoning with respect to (iv), one must be aware of some of the data underlying the 2s reconstructions. Pama-Nyungan \**ngin* '2s' is reconstructed on the basis of a range of forms: *ngin, ngun, nyin, nyun, ...*. Northern \**nginy* '2s' masks the fact that there is an alternation between nominative forms in *nyiN*- and oblique in *ngVN*- in many non-Pama-Nyungan languages. This same alternation occurs in a subset of Northern languages, but is not general.

Blake concludes:

"All in all the Tangkic pronouns align with the northern ones and not at all with the Pama-Nyungan ones. There is not a single formative that suggests alignment with Pama-Nyungan and no dearth of forms that are distinctively northern." (ibid.:23)

He fails to consider the third possibility — that the Tangkic pronoun material is unrelated to either reconstructed set.

#### 4.1.3 Assessing the Evidence

In assessing Blake's observations, we can first discount those regarding the *absence* of forms – negative evidence proves nothing one way or the other. What is similar in the Tangkic and reconstructed Northern pronoun sets is the following:

- i. a second nonsingular form whose vowel has changed: *kV*
- ii. a nonsingular formative whose reconstructed vowel is indeterminate: \**-rrV*
- iii. a first nonsingular exclusive *nya* in Lardil, which may reflect either \**nyi* or \**nya*. It has either changed vowel (*nyV*) or changed function (1 non-singular).
- iv. a suppletive alternation in the 2nd singular

How compelling is this evidence? Two of these cases involve pronominal roots of the form CV, in which the quality of the vowel is indeterminate. So we are left with two single consonant identities, and two indeterminate vowels. A third involves a (non-root) formative *rrV*. The best evidence is perhaps the suppletive alternation, though even that involves very little phonetic substance.

But even if we found these similarities compelling, or if the resemblances were closer than they actually are, very serious doubts would remain. If two languages have been separated for so long that the amount of lexical replacement militates against the comparative method, then is it not also likely that those few cognates that remain have been obscured by layer upon layer of distinct sound changes? If that is the case, then why do any putative shared grammatical or morphological retentions look similar? Are such forms not only resistant to borrowing, but to sound change as well? The most recalcitrant cases are those like that found in New Caledonia and in Australia, where phonemic inventories are shared, but not much else. The most obvious account of that sort of situation is that the phonemic inventories have converged as a consequence of just the long term contact giving rise to the differential lexical borrowing and replacement that has obscured any shared lexical retentions, and has obviated the comparative method.

Others are less sceptical than we are. Evans (1995:32) stresses these same observations:

"Lexical items and typological features are readily diffusible, particularly in Australia, and to get a more reliable idea of the genetic affiliations of Tangkic we need to turn to subgroupings established by regular sound changes, as well as aspects of morphology that are resistant to borrowing, such as pronouns. The comparative data that is decisive in grouping the Tangkic languages as non-Pama-Nyungan are the pronoun forms, and the retention of initial apical stops and nasals that laminalize in Pama-Nyungan.<sup>17</sup>"

Here Evans makes it clear that he considers pronouns to be resistant to borrowing. In a footnote to the passage we quote, he cites Heath's (1978) study as follows:

"(Heath] mentions diffusion of nouns (including kin terms) and verbs, but not pronouns, although he makes no specific reference to this." (Evans 1995: 32).

Evans appears to have overlooked Heath's remarks concerning the relative diffusibility of pronouns cited above.

## **5. Is There a Magic Bullet?**

Meillet was less than clear on at least two issues. As we noted in section 3.3, he leaves us somewhat confused regarding how one goes about recognising a singular fact. Though he does seem to suggest that some facts (morphological irregularities in particular) are more singular than others, he does not say tell us whether we are free to use any

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<sup>17</sup>This latter phonological innovation, the merger of initial apicals and nasals in Pama-Nyungan, is only as persuasive as the evidence Evans (1988) presents for it. He cites 19 forms distributed across the four correspondence sets, and selected from a range of languages across the Pama-Nyungan and non-Pama-Nyungan regions. We do not find the case to be convincing.

morphological similarity (in the modern sense of that term), or any similarity between grammatical items, in a genetic argument. But that in clarity is perhaps not particularly important, if we infer that to extend the range of singular facts from morphological anomalies to any 'grammatical' morpheme simply weakens our genetic argument.

A second issue is more significant. Meillet does not tell us *explicitly* how to go about identifying *similarity* between singular facts in different languages. Are we just to observe that two singular morphological forms in two different languages look similar? Surely not. Nowhere does Meillet suggest that any random phonological likeness is similarity in the required sense. But if that is the case, then how do we know when two singular facts are formally similar. The answer, as suggested earlier, is obvious; *we use the comparative method*. If the forms are reconcilable through regular sound correspondence, then they are similar. But if we must use the comparative method to determine what is or is not similar, then at best we have a dilemma, and at worst, a circularity: we can use similar grammatical oddities to determine whether or not two languages are likely to be genetically related, so long as we have first used the comparative method to determine that the forms are indeed similar.

In only one sort of case does this dilemma appear to be resolved. If we have already determined that some set of languages is genetically related, using the comparative method, and we want to sort that set of languages into subgroups, there the conditions for an appeal to Meillet's singular facts are met. But then we must demonstrate that the grammatical oddity to which we want to appeal is a shared innovation, and not a retention. That is an even more challenging task.<sup>18</sup>

We must conclude then that there is no magic in Meillet's magic bullet; in fact, there is very little in it of practical value to the comparativist at all.

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<sup>18</sup>For some discussion, see Harrison (1986, in press).

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