IN SEARCH OF THE REGIONAL DIVERSIFICATION OF LATIN: SOME METHODOLOGICAL CONSIDERATIONS IN EMPLOYING THE INSCRIPTIONAL EVIDENCE[1]

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ABSTRACT

The aim of the project entitled "Computerized Historical Linguistic Database of Latin Inscriptions of the Imperial Age" (http://lldb.elte.hu) is to develop and digitally publish a fundamental computerized historical linguistic database that incorporates and manages the Vulgar Latin material of the Latin inscriptions from the European provinces of the Roman Empire. In my paper, however, I do not present the Database (as this has already been done, in Adamik 2009), but instead I consider only the methodology of extracting regional variations from inscriptions, with reference to Adams 2007. In connection with this I will show that the methodology recommended by Adams 2007 does not really work whereas that established by Herman (meaning the last version, in Herman 2000a) is the most efficient, and yields a solid basis for building up the new Database.

Introduction

In the year 2007, the most extensive study on the regional diversification of Latin language ever published appeared. As one might expect, J.N. Adams' new book of more than 800 pages discusses, among other things, the methodology of extracting regional variations from inscriptions. However, it was hardly expected to contain in

1. The present paper has been prepared within the framework of the project OTKA (Hungarian Scientific Research Fund) No. K 81864 entitled "Computerized Historical Linguistic Data Base of Latin Inscriptions of the Imperial Age". I am very grateful to Zsuzsanna Sarkadi for correcting my English.

the very introduction such decisive statements on the topic indicated in the title of my paper as the following: "of the evidence that might be called on in investigating the regional diversity of Latin, inscriptions, with their uniformity right across the Empire, are the weakest."[2] Or, somewhat prior to that: "the best evidence for variation is found not in the inscriptions that have traditionally been investigated for this purpose, but in literary *testimonia*, non-epigraphic documentary corpora and even some literary texts."[3] These statements are totally in contradiction with what I know about the topic from the studies of József Herman, who declared several times[4] that inscriptions yield the best evidence for the regional variation of Latin instead of literary *testimonia* or literary texts. Right at the beginning, however, I have to make clear that such negative statements of Adams should rather be regarded as hyperboles which serve to direct attention to the sources *other* than epigraphic as the main topic of his book.[5] Therefore they are not to be regarded as too shocking, and, as Adams formulates later in his book, if one asks whether inscriptions can produce evidence for the regional diversification of Latin, the answer is a "guarded yes."[6]

In order to explicate his "guarded yes," Adams dedicated a separate chapter[7] to the methodology of using inscriptions in the investigation into the regional diversity of Latin during the Empire. In that chapter Adams reconsiders nearly all the literature on the matter, and mainly criticises and condemns those investigating the inscriptional evidence for their many mistakes, especially their mistakes concerning methodology. His criticism usually seems valid, though sometimes it is not quite well-founded.

Of the many issues discussed by Adams in that chapter I intend to deal only with that of the methodology of extracting regional variations from inscriptions. I do this on the one hand because methodology is a crucial point also for our Database of inscriptions which aims at just the same as the book of Adams, namely, at discovering the territorial differences of Latin. Therefore, in the light of Adams' negative statements, one may ask whether there is any point at all in building such a Database of inscriptions, or whether this venture is entirely pointless and futile. On the other hand, I partly agree and partly disagree with Adams concerning the next statement:[8]

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2. Adams 2007, p. 7. See also *ibid.*, p. 35: "It will become clear that I do not find inscriptions, particularly of the Empire, satisfactory as evidence for the regional diversity of the language."
3. *Ibid.*, p. 3.
4. E.g. in Herman [1985] 1990, p. 166-167.
5. The main part of his book (p. 114-623) deals with the non-epigraphic sources as possible evidence for regional diversification.
6. Adams 2007, p. 676: "Can inscriptions turn up evidence for the regional diversification of Latin? The answer is a guarded yes, particularly if the inscriptions from a region can be supplemented by non-literary documents. Inscriptions are less satisfactory if they have to be used on their own, because they are scattered about, often undated, and probably composed in many cases by outsiders to the areas in which they were found."
7. Chapter X, p. 624-683.
8. Adams 2007, p. 625.

"The methodology of those who have compiled statistics has not always been satisfactory, and doubtful claims have been based on weak evidence. I consider it more important here to discuss methodology than to range widely over many types of misspellings." With this second sentence of Adams I agree completely, moreover, this is the reason why I changed my original plan to give a simple presentation on our new Database, which is now under construction. My decision was also facilitated by the fact that recently a new study containing a general presentation of this Database was published sooner than expected.[9]

Henceforth contrasting the methodological views of J.N. Adams with those of J. Herman on inscriptional evidence I try to show that the almost negative and depressing conclusion drawn by Adams and cited above is not to be accepted at all. This way I hope the epithet "guarded" could also be removed from before his "yes".

Methodological revival in the sixties resp. seventies of the 20th century and Adams' criticism of it

As both Adams[10] and Herman[11] rightly emphasise, there are good reasons to believe that Latin had geographical variations already during the time of the Empire. The only question is whether inscriptions can provide evidence for the regional diversification of Latin. Until the sixties of the 20th century the answer was an unhesitant no, because the early descriptions of the inscriptional material of the provinces (carried out by Carnoy, Pirson and Skok) could not reveal any territorial differences in the language used in the inscriptions. These first studies found that the same banal-seeming misspellings, however significant linguistically, turn up all over the Empire and they are so widespread that cannot serve as means of differentiation of one region from another.[12]

In the sixties and seventies of the 20th century, scholars including Herman (in various papers), Gaeng,[13] Barbarino,[14] and others sought to refine the unpromising data by establishing that certain errors, though found all over the Empire, are of varying frequencies in various regions. As Adams[15] characterises this methodological revival: detailed statistical tables were compiled showing the incidence of particular misspellings in different parts of the Empire. If misspelling X is common in one place

9. Adamik 2009.
10. Adams 2007, p. 1-2.
11. Herman 2000b, p. 115.
12. Cf. Adams 2007, p. 6 and Herman 1996, p. 57.
13. Gaeng 1968.
14. Barbarino 1978.
15. Adams 2007, p. 6 and 624.

but rare in another, the assumption can be made that the underlying linguistic change was more advanced in the first region than in the second. There was some optimism that by comparing the incidence of misspellings from area to area it might be possible to find signs of the dialectalisation of Latin.

In the criticism of the above assumption and the methods of Herman on the one hand and of Gaeng,[16] Omeltchenko,[17] and Barbarino[18] on the other, Adams[19] mainly just lumped together these scholars and their methodologies, because he found all their methods unsatisfactory in some ways. However, there are significant differences between the methodology of Herman and of the others, as Adams[20] himself admits it, therefore it is more convenient here to separate the discussion on the methods of Herman and of the other scholars. After all, Herman, dealing with the same topic, also distinguished his own methodology from that of Gaeng and Omeltchenko.[21]

Let us first see the weak points of the methods of Gaeng,[22] Omeltchenko,[23] and Barbarino[24] according to Adams' criticism. Here it has to be emphasised that there is a broad overlap between the views of Adams and those of Herman in their distancing from the methods of scholars just named, so we can discuss the methodology of this group as separate from Herman's œuvre briefly and quickly.

Eliminating the use of absolute figures

As Adams[25] points out: "It has been assumed that if a misspelling is more frequent in one place than another, there must have been some sort of difference in the speech of the two places. That is the assumption underlying the works of Gaeng 1968, Omeltchenko 1977 and Barbarino 1978. However, there is an obvious question raised by the above assumption: how to determine 'frequency'?"

Adams[26] rightly states: "The use of absolute figures, that is the mere counting of instances of an error without reference to the frequency of its correct alternative or

16. Gaeng 1968.
17. Omeltchenko 1977.
18. Barbarino 1978.
19. Adams 2007, p. 629-635.
20. *Ibid.*, p. 626 and 629.
21. See Herman [1985] 1990, p. 69-70, on distinguishing the methodology of Gaeng and Omeltchenko ("pour certains auteurs") from his own ("Pour d'autres"). Cf. n. 28.
22. Gaeng 1968.
23. Omeltchenko 1977.
24. Barbarino 1978.
25. Adams 2007, p. 629.
26. *Loc. cit*.

some other comparandum, must be ruled out, as Herman[27] noted.[28] The size of corpora varies, and a low figure for one region may be statistically more significant than a high figure for another."[29] This means that it is rather pointless to evaluate absolute figures in isolation because this way we would measure extra-linguistic factors only.[30] Using absolute figures would be revealing only if a misspelling of a certain type was attested in only one area and nowhere else. However, evidence of this type is uncommon: much the same types of misspellings turn up all over the Empire.[31] In short, an absolute figure of errors gets significance only if interpreted as in proportion to another figure, either to the number of the correct alternative (as for Gaeng and his followers) or to the number of errors of a selection of different types, resp. to the number of errors of all other types in a corpus (as for Herman).[32]

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27. Herman [1985] 1990, p. 69-70.

28. The formulation of Adams is somewhat misleading, because Herman rejected not only the use of absolute figures of errors but the reference to the frequency of their correct alternatives as well, and he only voted for the method of calculating the relative frequency of 'faults'. Cf. the diplomatic formulation of Herman [1985] 1990, p. 69-70: "pour certains auteurs (i.e. for Gaeng and Omeltchenko, B. A.), il s'agit de tenir compte de la proportion entre 'fautes' et graphies correctes, c'est-à-dire mesurer la fréquence des fautes contre le nombre d'occasions où elles auraient pu être commises. Pour d'autres (i.e. for Herman himeself, B. A.), cette méthode recèle encore un danger : celui de mesurer le degré de culture et les connaissances orthographiques des rédacteurs et non pas les propriétés de leur langue, il vaut donc mieux calculer la proportion entre le nombre des fautes d'un type donné et le nombre total des fautes, ou bien la proportion entre plusieurs types de fautes confrontés l'un à l'autre."

29. However, the significance of the different size of corpora must not to be overestimated, cf. Herman 2000a, p. 126: "Analogamente il numero delle grafie devianti messo in relazione con il numero delle iscrizioni di una certa provincia presenta un interesse puramente epigrafico, culturale, e non permette nessuna considerazione linguistica." If we disregard corpora of extremely small sizes, like, for example, that of Raetia with its ca. 850 inscriptions, theoretically we may find the same distributions of faults in regions with different sizes of corpora (say, in a province A with 6000 and in a B with 9000 inscriptions) and, conversely, we may register totally differring distributions of faults in regions with same sizes of corpora. Accordingly, the size of a corpus does not really matter.

30. See Adams 2007, p. 632: "I conclude that a high percentage of errors of a particular type in one area compared with a moderate percentage in another or a low percentage in a third can tell us nothing about dialectalisation if considered in isolation."

31. Adams 2007, p. 624: "It is, for instance, conceivable that a misspelling of peculiar type indicative of a feature of pronunciation might be attested in just one area (and the pronunciation reflected in the same area in Romance). But evidence of this type is lacking. Much the same types of misspellings turn up right across the Empire, and the changes of pronunciation that they reveal are reflected generally in Romance rather than in particular languages." Cf. p. 624, note 2 as well: "The same point is made several times by Herman ([1965] 1990: 11, 14, [1978] 1990: 36). See also Herman ([1985] 1990: 86)."

32. If, theoretically, we had for the merger of *ǫ́* and *ú* as an *ǫ* 30 items in region A and 60 items in region B in a given period, at first sight one would conclude from the differring absolute figures that the merger of *ǫ́* and *ú* was more advanced in the region B than in the A. But if we consider also the figures for the merger of *ę́* and *í* as an *ę* in both regions in the same period, say, 60 items

The comparative method based on proportions of correct and incorrect spellings

Let us now see the method of calculating frequencies based on the comparison of correct and incorrect spellings. According to Adams' description, Gaeng,[33] Omeltchenko,[34] and Barbarino[35] all counted certain errors province by province and set the numbers of correct spellings against them, calculating the percentage of errors. They took the percentages at face value as indicating dialectal variations, without truly acknowledging the fact that the variations in the number of errors might only reflect variations in literacy.[36] Criticising their methodology, Adams[37] rightly underlines: "It is certainly not enough to calculate the incidence of an error province by province in the manner of Gaeng and his followers. The supposition that the differing frequencies of an error in different areas, measured against the frequencies of the corresponding correct spelling, may reveal differences in the Latin of those areas is highly questionable for various reasons. The degree of error (expressed as a percentage) is at least as likely to reflect the educational level of writers as it is the state of the spoken language in a region."[38] Adams[39] adds that the same point is made several times by Herman. It is true: Herman distanced himself from this method on every possible occasion.[40]

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of it in A resp. 180 items in B, and we interpreted the figures of the former two phenomena (A: 30, B: 60) as proportions of the latter two (A: 60, B: 180), we would get a proportion of 50% in the former case (60/30 = 1/2) and 33% in the latter (60/180 = 1/3). Consequently, the merger of *ó* and *ú* would seem more advanced not in the region B (30%) but, on the contrary, in region A (50%). As for the methodology of comparing these mergers see Herman [1971] 1990, p. 139.

33. Gaeng 1968.
34. Omeltchenko 1977.
35. Barbarino 1978.
36. See Adams 2007, p. 629 and 626.
37. *Ibid.*, p. 630-631.
38. See also Adams 2007, p. 6-7: "The degree of spelling correctness or, conversely, the degree of error in a corpus of inscriptions may reflect the educational level of those who composed the inscriptions that happen to survive."
39. Adams 2007, p. 630, n. 26.
40. Herman [1971] 1990, p. 125-126, and Herman [1985] 1990, p. 70. Cf. text quoted *supra* n. 28. We add also Herman 2000a, p. 126, not cited by Adams at all: "Parlando di statistica, è chiaro che vanno evitati tutti i calcoli, tutte le comparazioni che rischino di riflettere, anziché tratti linguistici, differenze culturali ovvero diversità nel grado di scolarizzazione o nel livello professionale delle officine di lapicidi le varie fra provincie. Per esempio, le variazioni da un territorio all'altro della proporzione di tale 'sbaglio ortografico' rispetto alla grafia corretta e tradizionale, di per sé mostrano solamente la differenza culturale fra i territori considerati, e non corrispondono a differenze fonetiche e tanto meno dialettali."

As compared to his negative attitude, Adams later,[41] surprisingly, formulates his own ideas on the matter as follows: "A statistical survey of spelling errors ought to adhere to the following principles. The frequency of an error should be calculated as a proportion of the number of corresponding correct spellings. The frequency of one particular error should not be calculated in isolation. The degree of correctness of other spellings in the corpus needs to be established to provide a comparison with the spelling under investigation. The more numerous the other types of spellings used in the comparison the better."[42]

Here is an obvious discrepancy between Adams' attitudes to the methodology of his own and that of the other scholars which automatically invalidates his own recommended method. If the differing frequencies of an error in different areas, measured against the frequencies of the corresponding correct spellings, may only reflect variations in literacy *alias* the degree of spelling correctness, or if, conversely, the degree of error in a corpus of inscriptions may reflect the educational level of writers, as admitted by Adams himself, how could the comparison of frequencies of errors calculated as proportions of the numbers of corresponding correct spellings reflect any regional diversification of Latin? That is, how could the comparison of *such* relative frequencies reveal any dialectal differences in the Latin of different areas? It is rather pointless and somewhat misleading to discredit the relevance of *such proportions* while at the same time to suggest *their comparison* for investigating the regional diversification of the language. Here Adams simply combines the method of Gaeng, which is based on the degree of spelling correctness and that of Herman, which is working with the relative frequency of 'faults'.

If the comparison of percentages calculated as the proportions of correct and incorrect spellings still reveals some kind of dialectal variation, this is just because it reflects the rates of incorrect spellings in proportion to each other, that is, the relative frequency of 'faults' implicitly embedded in the percentages calculated as proportions of correct and incorrect spellings. Nevertheless, the relative frequency of 'faults' will thus be reflected in a somehow distorted and misrepresented manner,[43] since the numbers of correct spellings, as Herman pointed out, depend on extra-linguistic cultural factors such as the mass-production of normative texts of the administrative centres of the Empire etc.[44]

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41. Adams 2007, p. 662-663.

42. See also Adams 2007, p. 630: "The only satisfactory way of calculating the frequency of an error is to begin by identifying every single lexeme in the corpus that might have contained the error and to count the errors against correct spellings."

43. See the diagrams below.

44. See Herman [1971] 1990, p. 125-126: "Il n'est pas admissible non plus d'opérer – comme on le fait parfois – avec la proportion des graphies fautives et des graphies correctes, c'est-à-dire avec la fréquence d'une faute comparée au nombre des occasions où il aurait été possible de la commettre ; la mesure que l'on obtient ainsi ne reflète directement que l'état culturel de la région, la tenue orthographique des ateliers, le degré d'instruction de la moyenne des habitants ;

Diagram 1[45]

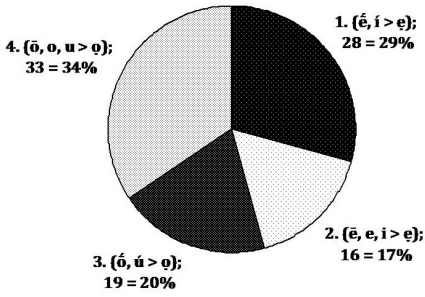
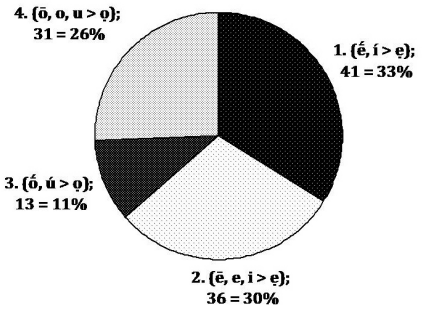


Diagram 2[46]



As it can well be seen, the analysis by Adams' method shows the proportion of *o*/*u* and *e*/*i* mergers to be pretty much the same (*e*/*i* = 46%; *o*/*u* = 54%), which (as the number of the *e*/*i* sounds is respectively the double of the *o*/*u* sounds within the Latin language) would mean that the merger *o*/*u* was more advanced than that of *e*/*i*. However, the analysis by Herman's method shows *the opposite*: the proportion of

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il est évident qu'une seule et même déviation du système parlé par rapport aux normes classiques se reflétera relativement moins souvent dans les inscriptions d'un centre scolaire profondément romanisé comme Mediolanum que dans celles d'une petite ville de la côte dalmate."

45. In the first Diagram I charted the data (for the vocalic mergers, see the legend of the diagram) from the 6-7th centuries collected by Gaeng 1968 from the Christian inscriptions of Narbonnensis, according to the methodology recommended by Adams 2007, p. 662-663 (cited above). In section 1 (representing the merger of *ḗ* and *í* into an *ę́*) the figure 28 means originally 28% (108 correct spellings vs. 41 deviations, 108 + 41 = 149, 41 / 1,49 = 28), in section 2 (representing the merger of *ē*, *e* and *i* into an *ę*) the figure 16 means originally 16% (191 correct spellings vs. 36 deviations, 191 + 36 = 227, 36 / 2,27 = 16); in section 3 (representing the merger of *ṓ* and *ú* into an *ǫ́*) the figure 19 means originally 19% (57 correct spellings vs. 13 deviations, 57 + 13 = 70, 13 / 0,7 = 19), in section 4 (representing the merger of *ō*, *o* and *u* into an *ǫ*) the figure 33 means originally 33 % (63 correct spellings vs. 31 deviations, 63 + 31 = 94, 31 / 0,94 = 33). Then, taking Adams' advice, 2007, p. 636 and p. 662-663 seriously, I compared the relative frequencies of this for types of errors. This curious comparison was possible only by taking these percents as absolute figures (N = 96 = 28 + 16 + 19 + 33) and this way I could chart their relative frequency (100% = 29% + 17% + 20% + 34%).

46. In the Diagram 2 I charted the same data but I considered only the deviations (leaving out the corresponding correct forms) and their relative frequency, according to Herman's methodology (see below). Here section 1 displays the figure of the deviations for the merger of *ḗ* and *í* into an *ę́* (41 instances with the relative frequency of 33%), section 2 displays the figure of the deviations for the merger of *ē*, *e* and *i* into an *ę* (36 instances with the relative frequency of 30%), section 3 displays the figure of the deviations for the merger of *ṓ* and *ú* into an *ǫ́* (13 instances with the relative frequency of 11%), and section 4 displays the figure of the deviations for the merger of *ō*, *o* and *u* into an *ǫ* (31 instances with the relative frequency of 26%). Thus the absolute figures (41 + 13 + 36 + 31 = 121) are displayed as proportions of their own total number, i.e. 121 (= 100% = 33% + 11% + 30% + 26%).

the *e*/*i* faults is the double of the *o*/*u* faults (*e*/*i* = 63%; *o*/*u* = 37%), which means that the two changes took place more or less at the same time and with the same intensity – as it is reasonable to expect.[47]

Therefore proportions of correct and incorrect spellings may only be useful for sociolinguistic and cultural, but not for linguistic and dialectical investigation.[48] This means that correct spellings as negative evidence are to be banished from research and only the misspellings as positive evidence are to be taken into consideration in the investigation into the regional diversity of Latin.[49]

Herman's comparative method of calculating the relative frequency of 'faults' as understood by Adams and as it really is

Adams' own methodology and his criticism of that of Herman

Now we can turn to Herman's methodology, which was criticised by Adams[50] as follows: "Herman adopted his own method of calculating the relative frequency of 'faults' in different provinces. He did not bother with correct spellings, but instead calculated the frequency of the error in which he was interested (call it A) as a proportion of the total numbers of errors (of types, say, B to F) in a corpus." As Adams describes, the important thing for Herman was to set the figure for one particular error against the number of errors of all other types, or at least against the number of errors of a selection of different types. Adams agrees with Herman in saying that the significance of one error can only be assessed in the light of other errors in the same corpus, though he criticises the way in which Herman applied the principle.[51]

47. As for the methodology of comparing these mergers see Herman [1971] 1990, p. 139.

48. Herman [1971] 1990, p. 126, n. 12: "Naturellement, la proportion entre graphies fautives et graphies correctes peut être très suggestive pour caractériser l'état de langue d'une seule région si on compare diverses proportions de ce genre entre elles. Le danger inhérent à l'emploi de ces données pour déterminer des caractéristiques dialectales a déjà été mis en évidence avant nous par E. Pulgram, *The Tongues of Italy.* Cambridge, Mass., 1958, 281."

49. Getting rid of the ballast of collecting correct spellings, the research concerned may gather speed and can concentrate on collecting as much data (i.e. deviations) as possible. For example for *ó*, *ú* > *ọ* in Rome in the 3-4th cent. Gaeng 1968, p. 95, had to gather 242 (!) correct items in order to register only 3 (!) deviations etc.

50. Adams 2007, p. 629.

51. *Ibid.*, p. 635: "Herman was well aware that the cultural level of the inscriptions surviving in one province might be higher than that of those surviving in another. He speaks of the danger that the investigator might be measuring degrees of culture and orthographic knowledge, not linguistic variation ([1985] 1990: 70). It is important, therefore, he states, to set the figure for one particular error against the number of errors of all other types, or at least against the number of errors of a selection of different types (see also above, 3). I agree in principle that the

Later Adams,[52] criticising a study of Herman,[53] tells what he thinks Herman should have done: "Herman's idea of assessing the frequency of the *B*/*V* confusion around the Adriatic against the frequency of the change *ns* > *s* […] was a good one, but I have suggested that his statistical method is open to criticism. *He might instead have calculated the degree of the two errors. The frequency of B for V in the various Adriatic regions should have been established by comparing errors with correct spellings and presenting the errors as a percentage of the whole. The same should then have been done for* s *(<* ns*) versus* ns *correctly written. Finally a comparison of the relative frequencies of the two types of errors might have been made*" (my emphasis). One may of course criticise the practice of Herman's methodology, but it is certain that the suggestion of Adams cannot be accepted because, as we have seen, the use of proportions of correct and incorrect spellings is inadequate for dialectal investigations.[54]

At the same time, the method accepted by Adams as the solely satisfactory one allows only for a restricted use of inscriptions in research.[55] As Adams[56] formulates: "The absence of an error from a corpus (or its marked infrequency) will be far more significant if the corpus is otherwise full of errors than if it shows a correctness of orthography across the board."[57] Then he continues as follows:[58] "If one of the two errors is frequent in the corpus but the other all but non-existent, it may be justifiable to

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significance of one error can only be assessed in the light of other errors in the same corpus, though I have criticised above (3) the way in which Herman applied the principle."

52. *Ibid.*, p. 636.

53. Herman [1971] 1990, p. 121-126.

54. Thus the judgement of Adams 2007, p. 676, is not acceptable at all: "The study of misspellings in inscriptions can easily degenerate into the study of variations in literacy. Herman was aware of the problem, but his own methods are open to criticism. His method of undertaking small-scale comparisons (his 'microtechnique') of one restricted area with another, counting misspellings of one type or another but rarely if ever setting them alongside the corresponding correct spellings, has not succeeded in establishing convincing regional variations."

55. Adams 2007, p. 650: "Those who have studied Latin inscriptions have tended to treat as significant percentage variations of the type just used in my illustration, or even variations that are less marked, and they have usually not employed the comparative method described here at all: the frequency of single errors in isolation has been calculated for different regions of the Empire […]"

56 *Ibid.*, p. 636.

57. *Loc cit.*: "… This comparative method of judging the significance of a correct spelling, that is by looking at the correctness of other types of spelling in the same corpus, can be nicely illustrated (though not in the context of regional variation) from the case of final *-s*. In many non-literary corpora *-s* is usually correctly written, but that in itself is unrevealing. But in the same corpora final *-m* is frequently omitted. The treatment of the two final consonants in some corpora is so sharply contrasting that one cannot but conclude that in the speech of the writers final *-m* was lost but final *-s* retained."

58. *Ibid.*, p. 637.

conclude that one linguistic change had occurred in the province but not the other."[59] Thus, according to Adams,[60] "by examining a second error we may be able to interpret the significance of the first, if only under restricted circumstances […]"[61] As Adams emphasises, this comparative method cannot always work out. He maintains that the method can only be revealing if there is a stark contrast between the frequencies of the two errors in the corpus.[62] He also admits[63] that "a comparison of the incidence of two errors in a corpus can only throw up evidence of possible significance for our purposes *if one of the errors is virtually non-existent but the other frequent*" (my emphasis).[64] What Adams states here in a restricted way[65] is practically exactly what is established by Herman in a much more complex manner. Nevertheless, Adams here compares the frequencies of only two errors and he, just like Herman, does not bother with corresponding correct spellings. However, Herman worked in a much more sophisticated way which allowed for more elaborate and detailed evaluation of the frequencies of errors, and which was capable of reflecting also the nuances in the distribution of the frequencies instead of showing only sharp contrasts. Henceforth, I try to present Herman's methodology in its last version.

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59. And he goes further: "If scribes or stonemasons committed one error often, their level of literacy was not high. If they avoided the other error entirely that avoidance could not be put down to spelling ability, but might be taken to suggest that the linguistic change underlying the error had not taken place where they were writing. It need hardly be pointed out that the significance of the absence of one particular error from such a corpus would be enhanced if the corpus contained not just one type of contrasting error but errors of many types."

60. Adams 2007, p. 649-650.

61. […] = "If error 1 was frequent across the whole Empire with the exception of one region (A), where it did not occur, its absence from A might only mean that the texts were written by highly educated writers. But if another error, 2, were frequent not only elsewhere but also in A, it would be reasonable to say that the corpus of A was not the work of highly educated writers at all. The absence of error 1 would suggest that the linguistic change underlying it had not occurred in A. Comparison of the two errors would allow us to go beyond spelling to the identification of a linguistic feature of A."

62. "If error 1 is non-existent but error 2 (and ideally other errors as well) numerous, we can say that the avoidance of error 1 was not due to scribal competence. […] If on the other hand error 1 was merely less frequent in percentage terms than error 2, that would not be sound evidence on which to base a claim that the linguistic change underlying error 1 had not taken place in area A."

63. Adams 2007, p. 650.

64. With the help of this method and calling also non-epigraphic documentary corpora on in the investigation Adams could reveal a marked contrast between Africa and Gaul in their phonological system, see Adams 2007, p. 637-638, 647 and 676.

65. Apart from calling non-epigraphic documentary corpora on what is welcome otherwise, see Adams 2007, p. 651: "Fortunately there are also some non-epigraphic documentary corpora, and the significance of these, particularly in Africa, is out of all proportion to their size. In Africa they give support to the evidence of the local inscriptions, and thus offer some justification for our not abandoning all hope of extracting something worthwhile from inscriptions."

Herman's method as it really is

Herman published his methodologically most elaborated study in 2000. It is striking that this article is *not* cited by Adams, moreover, not even a hint at it is to be found in the entire book,[66] whereas Herman wrote it essentially in order to repel Adams' former criticism, and in it he explicated his method much more accurately than he formerly had done.[67] Therefore let us see briefly the main points of Herman's last study, which contains and describes not only the methodology, but its employment as well.

In his "Premessa metodologica", before explaining the point of his own statistical method, Herman makes clear what he means by 'faults'.[68] 'Faults' are defined as any linguistic phenomena which can be isolated at text level and which deviate from what is called the 'classical' norm. These 'faults' are to be divided into two groups: 'faults' which are due to linguistic factors and which are due to extra-linguistic (that is, graphic or technical etc.) factors. After eliminating the 'faults' of technical origin, the interesting but isolated and sporadic phenomena, which are not appropriate for a statistical treatment, should be excluded. This way only those 'faults' of linguistic relevance remain, which are thus suitable for a statistical survey – provided they are of sufficient frequency. As a peculiarity of inscriptional texts, the greatest number of these data is of phonological nature, so first of all phonology can serve as a subject for statistical treatment.

After distancing himself from every method which measured degrees of culture and orthographic knowledge rather than linguistic variation, Herman[69] declared that the only way of statistical treatment is to consider the total numbers of 'faults' for every region and then to calculate the relative frequency of the different types of 'faults' in relation to each other;[70] thus every region will have its own profile based on the proportion of deviations considered independently of the totality of correct

66. Nevertheless, Herman published it in that Acts of the Round Table held and organised by himself at the University of Venice in 1998 (Herman 2000a) from which Adams did use some other studies (of Calboli, Coleman, Marinetti and Prosdocimi).

67. Cf. Herman 2000a, p. 126, n. 8: "Il principio metodologico riassunto nel punto 4. è stato definito, spiegato e applicato alcuni decenni fa (già in Herman 1965), e ha suscitato reazioni positive. Di recente è accaduto tuttavia che un autore illustre (Adams 1995, 88 n. 19), probabilmente per via di una lettura troppo rapida, mi rimproverasse di misurare differenze culturali anziché linguistiche, facendomi dire così l'esatto contrario di quanto in realtà ho sempre detto e fatto."

68. Herman 2000a, p. 124, avoids using the term 'sbagli', and instead he uses the term 'grafia' as abbreviation for "grafia, forma grafica erronea, deviante, sbagliata".

69. *Ibid.*, p. 126.

70. The same is found in Herman, [1985] 1990, p. 70: "il vaut donc mieux calculer la proportion entre le nombre des fautes d'un type donné et le nombre total des fautes, ou bien la proportion entre plusieurs types de fautes confrontés l'un à l'autre. Il est d'ailleurs possible d'utiliser ces modes de calcul conjointement."

inscriptions and thus of cultural factors.[71] The linguistic differences between the regions may be represented as differences between these profiles, that is, in the diverse distribution of the 'faults'. If a 'fault' is committed at a special point of the structure of a language, it cannot have any other reason than a linguistic one for not being committed at other points. As a result, the diverse distribution of 'faults' must display linguistic differences. Furthermore, in order to guarantee that these profiles indeed represent linguistic differences, the figures of the fluctuation between AE and E have to be highlighted for every region considered, since the frequency of this fluctuation depends on the cultural level only. The linguistic change underlying this fluctuation had already taken place at the beginning of the Empire, thus the figure of the fluctuation between AE and E can serve as a kind of cultural index for the region concerned. If these cultural indices (which can be measured also as proportions of the total number of errors including this fluctuation as well) do not differ from each other significantly while the profiles with their particular structure of deviations do, it must mean that the diverse distributions displayed in the profiles represent linguistic differences.[72]

The results of the research carried out on a corpus of about 1300 Christian inscriptions from five regions of Italy by Herman are charted in a very illustrative way. Based on their linguistic profiles, the five regions can be divided into two groups. Therefore, to make the charts easier to understand, I compiled two tables which summarise the explanation provided by Herman.[73]

71. Herman 2000a, p. 126: "L'unica soluzione consiste nel considerare, per ogni unità territoriale, 1'insieme, 1'universo degli sbagli, delle grafie, e di calcolare la frequenza relativa dei tipi di sbagli in relazione fra loro; così, ogni territorio avrà un suo 'profilo', una sua struttura delle deviazioni, considerata indipendentemente dalla totalità delle iscrizioni corrette, dunque indipendentemente da fattori culturali."

72. *Ibid.*, p. 126, n. 7: "Segnaliamo un punto di particolare importanza: dato che alla fine dell'impero la monottongazione *ae* > *e* era un processo compiuto da secoli, le frcquentissime fluttuazioni e incertezze fra *AE* e *E* sono semplicemente ortografiche, senza incidenza fonetica. La loro frequenza dipende dal livello culturale, di competenza ortografica, e non dalla pronuncia. Per completezza, noi teniamo conto del numero degli sbagli di questo tipo, che costituiscono una specie di indice culturale, ma non li prendiamo in considerazione nel determinare il profilo fonetico caratteristico della regione."

73. Some of the figures displayed in my commentary tables are not given by Herman (as being of less interest), who gives only the percentages they represent, so I had to recalculate these figures from the percentages with the help of their distribution for the sake of a more transparent display.

TABELLA 1
Regio Augustea IX (Liguria)

grafie con incidenza fonetica (N = 100 %): 146
grafie senza incidenza fonetica: 23
grafie *AE* ~ *E* : 27

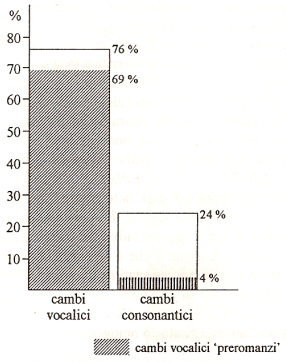
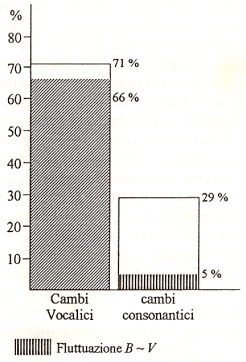


TABELLA 2
Regio Augustea XI

grafie con incidenza fonetica (N = 100 %): 212
grafie senza incidenza fonetica: 37
grafie *AE* ~ *E* : 77



cambi vocalici 'preromanzi' Fluttuazione *B* ~ *V*

Table A	
Profile of *Regio Augustea IX* (Liguria) [Herman 2000a: 129, Tabella 1]	Profile of *Regio Augustea XI* [Herman 2000a: 129, Tabella 2]
N = 100% = 146 items ('faults' of phonological relevance)	N = 100% = 212 items ('faults' of phonological relevance)
[23 items of 'faults' of not phonological but morphosyntactical relevance, not included in N]	[37 items of 'faults' of not phonological but morphosyntactical relevance, not included in N]
[27 items of AE ~ E, not included in N; $N^{+AE\sim E}$: 146 + 27 = 173: cultural index 15,6%]	[77 items of AE ~ E, not included in N; $N^{+AE\sim E}$: 212 + 77 = 289: cultural index 26,5%]
76% *= 111 items of vocalic 'faults' that include:*	*71%* *= 150 items of vocalic 'faults' that include:*
69% = 101 items of vocalic 'faults' reflected in Romance (*i ē* > *ę*, *u ō* > *ǫ*, *e*V~*i*V etc.) and 7% (= 76%-69%) = 10 items of vocalic 'faults' not reflected in Romance or of insufficient frequency	66% = 140 items of vocalic 'faults' reflected in Romance (*i ē* > *ę*, *u ō* > *ǫ* etc.) and 5% (= 71%-66%) = 10 items of vocalic 'faults' not reflected in Romance or of insufficient frequency
24% = 35 items of consonantal 'faults' (CC~C, palatalisations, H > Ø, *ns* > *s*) including:	29% = 62 items of consonantal 'faults' (CC~C, *ns* > *s*) including:
4% = 6 items of B ~ V	5% = 11 items of B ~ V

TABELLA 3
Provincia Sardinia

grafie con incidenza fonetica (N = 100 %): 115
grafie senza incidenza fonetica: 15
grafie *AE* ~ *E* : 22

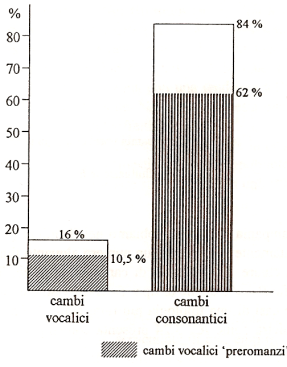


TABELLA 4
Regio Augustea VIII (Etruria)

grafie con incidenza fonetica (N = 100 %): 147
grafie senza incidenza fonetica: 35
grafie *AE* ~ *E* : 41

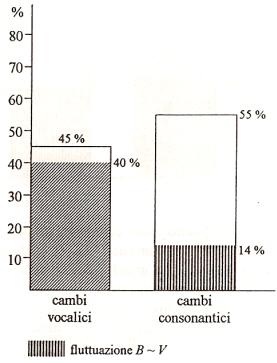


TABELLA 5
Regio Augustea III (Bruttium)

grafie con incidenza fonetica (N = 100 %): 53
grafie senza incidenza fonetica: 13
grafie *AE* ~ *E* : 29

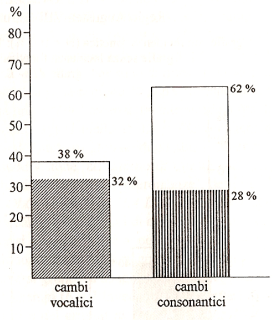


Table B		
Profile of *Provincia Sardinia* [Herman 2000a: 131, Tabella 3]	Profile of *Regio Augustea VIII* (Etruria) [Herman 2000a: 131, Tabella 4]	Profile of *Regio Augustea III* (Bruttium) [Herman 2000a: 132, Tabella 5]
N = 100% = 115 items (‘faults’ of phonological relevance)	N = 100% = 147 items (‘faults’ of phonological relevance)	N = 100% = 53 items (‘faults’ of phonological relevance)
[15 items of ‘faults’ of not phonological but morpho-syntactical relevance, not included in N]	[35 items of ‘faults’ of not phonological but morpho-syntactical relevance, not included in N]	[13 items of ‘faults’ of not phonological but morpho-syntactical relevance, not included in N]
[22 items of AE ~ E, not included in N; $N^{+AE\sim E}$: 115 + 22 = 137: cultural index 16%]	[41 items of AE ~ E, not included in N; $N^{+AE\sim E}$: 147 + 41 = 188: cultural index 22%]	[29 items of AE ~ E, not included in N; $N^{+AE\sim E}$: 53 + 29 = 82: cultural index 35%]
16% = 18 items of vocalic ‘faults’ that include:	*45% = 66 items of vocalic ‘faults’ that include:*	*38% = 20 items of vocalic ‘faults’ that include:*
10,5% = 12 items of vocalic ‘faults’ reflected in Romance (*i ē* > *ę*, *u ō* > *ǫ*, *e*V~*i*V etc.) and 5,5% (= 16%-10,5%) = 6 items of vocalic ‘faults’ not reflected in Romance or of insufficient frequency	40% = 59 items of vocalic ‘faults’ reflected in Romance (*i ē* > *ę*, *u ō* > *ǫ*, *e*V~*i*V etc.) and 5% (= 45%-40%) = 7 items of vocalic ‘faults’ not reflected in Romance or of insufficient frequency	32% = 17 items of vocalic ‘faults’ reflected in Romance (*i ē* > *ę*, *u ō* > *ǫ*, *e*V~*i*V etc.) and 6% (= 38%-32%) = 3 items of vocalic ‘faults’ not reflected in Romance or of insufficient frequency
84% = 97 items of consonantal ‘faults’ (X [ks] > SS/S [s] etc.) including:	55% = 81 items of consonantal ‘faults’ (X [ks] > SS/S [s] etc.) including:	62% = 33 items of consonantal ‘faults’ (X [ks] > SS/S [s] etc.) including:
62% = 71 items of B ~ V	14% = 21 items of B ~ V	28% = 15 items of B ~ V

While the cultural indices of these regions do not differ from each other significantly, the phonological profiles of the same regions do. Moreover, they are to be sorted in two contrastive groups according to their characteristic distribution of types of 'faults': in region A with a clear innovative vocalism but with an explicit conservative consonant system, and, conversely, in region B with an explicit conservative or less innovative vocalism and with a largely or significantly innovative consonant system. What is more, the cultural indices of the two most contrastive regions, that is, of Liguria with its advanced vocalism of 69% (Tabella 1) and of Sardinia with its far gone consonant system of 84% (Tabella 3) are in essence identical (15,6% and 16% resp.), consequently the differences in their phonological profiles must be of dialectological nature.

These results yielded by Herman on a not too extensive but coherent corpus of 1300 Christian inscriptions from Italy prove that his own method, the gist of which was devised decades ago, is quite efficient, therefore it is not to be replaced by any other method. This is the reason why the new Computerized Historical Linguistic Database of Latin Inscriptions of the Imperial Age conceived by my Master about 20 years ago has adopted this very methodology. The new Database, together with its computer program, has already been running in the last few years (see http://lldb.elte.hu/). The only remaining task is to collect, hopefully in an international cooperation, all the data which are waiting to be extracted from the widest possible inscriptional corpora of the Empire in order to create an effective tool for the research of the regional diversification of Latin.

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