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## A Taxonomic Study of the Manuscript Tradition of Juvenal

*By John G. Griffith, Oxford*

1.1. Years of lecturing on Juvenal have led me to some unorthodox lines of thought about problems posed by the history of his text, which for their detailed exposition and, so far as may be, justification, would need extended treatment in book form. Before investing time and energy in such an undertaking, I offer this study in the hope of eliciting opinion, adverse or otherwise, that may help me to decide whether to proceed further or to leave matters where they are and turn – not, I suspect, before it is time – to think about something else. If this essay is to be kept within bounds, I can only indicate in barest outline some of the arguments on which I rely, especially in the preliminary sections (2.11 to 3.23). The price of brevity is an impression of over-confident assertion, and I have not always given specific references to the writings of those scholars who have supplied much of the information (imprimis U. Knoche<sup>1</sup>) or have otherwise activated my thinking, principally E. A. Lowe<sup>2</sup>, G. Pasquali<sup>3</sup>, J. Mallon<sup>4</sup>, C. H. Roberts<sup>5</sup>, M. Bévenot<sup>6</sup>, P. Maas<sup>7</sup> and A. Dain<sup>8</sup>. Where so much has been written on this kind of problem, I am diffident about adding more. As however the substrate of what follows here seems to have passed beyond the tentative stage, *τρεῖν μ' οὐκ ἔα Παλλὰς Ἀθήνη*.

1.2. Juvenal, most self-effacing of Roman satirists, has left us in ignorance of much that we would like to know concerning his life and times; as if to compensate posterity for this disappointment, there is more evidence available for the study of the early history of his text before 800 A.D. than for that of many ancient

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\* A short abstract stating the results of this study was published in *Nature*, vol. 215 (15 July 1967) 326. I much appreciate the interest of its editor in a topic somewhat removed from the mainly scientific fields which that journal covers. I am also most grateful to Mr. C. H. Roberts, Mr. N. G. Wilson and Mr. J. D. P. Bolton for kindly reading a draft of this article and for the encouragement of their criticisms. The technical sections owe much to Dr. P. H. A. Sneath and Dr. J. Rollett, as will be evident from note 24 below.

Much of the preliminary work was done during a visit to the Fondation Hardt at Geneva, and I take the opportunity of expressing my appreciation of the generous hospitality I enjoyed there then and again on a second visit in 1967.

For some remarks on the “mathematical” analysis of the mss. of Theodoretus *Περὶ τῆς θείας ἀγάπης*, by P. Canivet and P. Malraux (Byzantion 34 [1964] 385–413), see note 26 at end.

<sup>1</sup> U. Knoche, *Handschriftliche Grundlagen des Juvenaltexes*, *Philologus Suppl.* Bd. 33, 1 (1940).

<sup>2</sup> E. A. Lowe, *C(odices) L(atini) A(ntiquiores)* vols. I–XI, and other writings.

<sup>3</sup> G. Pasquali, *Storia della tradizione e critica del testo*<sup>2</sup> (Florence 1952).

<sup>4</sup> J. Mallon, *Paléographie romaine* (Madrid 1952).

<sup>5</sup> C. H. Roberts, *The Codex*, in *Proc. Brit. Acad.* 40 (1954) 167f.

<sup>6</sup> M. Bévenot, *The Tradition of Manuscripts* (Oxford 1961).

<sup>7</sup> P. Maas, *Textkritik*<sup>3</sup> (Leipzig 1957): English translation by Barbara Flower (Oxford 1958).

<sup>8</sup> A. Dain, *Les manuscrits*<sup>2</sup> (Paris 1964).

authors, while material after that date is too vast and too heterogeneous for comfort. Over-frequent copying can be a doubtful blessing. I believe however that in the light of what can be stated or inferred with reasonable confidence about the earlier phase of the transmission, rather more can usefully be said about the later one than is commonly admitted. This enquiry sets out to see how far an unsophisticated but, I hope, judicious use of taxonomical methods (4.22f., below) may lead towards a rational sorting of those mss. which, being fully collated, represent for this purpose the 'open' or 'horizontally interpolated' element in the tradition. From this it may be possible to arrive at criteria for rejecting from a future apparatus criticus the more erratic among them while admitting others, codices raro adhibitos, which may have stronger claims. The clearest way of presenting the chosen documents in their relative order as determined by the particular context will also call for consideration, and some unusual but interesting readings which crop up in unexpected places may come in for revaluation.

*The first phase, before 800 A.D.*

2.1. There seem to be no grounds for questioning the communis opinio on the following:

2.11. The virtual lack of references to Juvenal's writings suggests that he was an author almost if not entirely unknown to readers of the second, third and fourth centuries of our era<sup>9</sup>. The ancient grammarians had such a strong predilection for archaic literature that their silence in regard to Juvenal may be less significant than is sometimes implied: that however Charisius quotes Persius four times but never mentions Juvenal may be indicative.

2.12. When interest in Juvenal, as probably in Lucan and Statius too, revived in or about 390 A.D., as the well-known statement of Ammianus Marcellinus (xxviii 4, 14) certifies, there was, it seems, only one copy available for the dissemination of his text, and that a mutilated one, ending abruptly at xvi 60 and lacking both the longer and the shorter of the two 'Oxford' fragments of satire vi<sup>10</sup>.

At this point it may be remarked:

2.13. The division of the 16 satires into 5 books, respected by our medieval mss., may be ancient, in as much as the number of lineae in each book corresponds to quantities that would conveniently fill 5 papyrus rolls<sup>11</sup>. Notwithstanding some experiments with codex-format for literary purposes as early as 100 A.D., there is no doubt that the volumen continued to be the standard vehicle for literature until around 300 A.D.

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<sup>9</sup> See further H. J. Thompson in Cl. Q. 22 (1928) 24-27 and P. Wessner in Berl. Phil. Woch. 49 (1929) 302.

<sup>10</sup> On these lines, see my note in Hermes 91 (1963) 104f.

<sup>11</sup> The lengths are: Book I, 990 lines; Book II, 692 lines (including the two 'Oxford' fragments); Book III, 668 lines; Book IV, 704 lines; Book V (in its present state) 814 lines. These figures need to be reduced by the number of spurious verses (2. 51) not present at this early stage.

2.14. It is however likely that the sole text, mutilated at the end, which was available in or around 390 A.D. was a codex, not a set of rolls. The end of a roll, being wound inside, was less vulnerable to terminal mutilation<sup>12</sup>, whereas the last leaves of a codex could easily have become detached.

2.15. These last considerations point to a transcription out of volumina into a codex at some time in the fourth century, or perhaps a little earlier. To this transcription may be attributed the removal of the two 'Oxford' fragments from satire VI, whatever the editorial motive may have been.

2.21. Systematic annotation is alien to the roll, and comments at this early date would have been written in a separate hypomnema. It can be shown independently that the core of the extant body of the scholia (the P-scholia, 3.22) is based on a compilation that does not go back beyond the middle of the fourth century<sup>13</sup>. The volumen-text may however have carried some alternative readings, perhaps marked by diacritical signs (2.64), which, in view of Juvenal's eclipse in the early centuries (2.11 above) may well have been author-variants. The possible instances are too few to lend any support to the now discredited hypothesis of a second recension of the satires by the author or a literary executor. It seems not to have been noticed that these putative variants are densest in Book III (satires VII-IX) and this may be connected with the fact that this book presents more textual difficulties than the others. This may be due to mere chance: the volumen-text might have been less carefully written in this section, or the writing material have been less durable. Equally, the possibility that this portion of the satires had not been as thoroughly revised as the rest needs to be kept in mind.

2.22. Of these suggested author-variants, I do not regard VII 100 (*nullo quippe modo* as against *namque oblita modi*) or VII 139 (*fidimus eloquio?* as against *ut redeant veteres*) as examples, for in each place a reasoned choice is possible. The stronger cases would seem to be VII 51f, VIII 5f., VIII 122f. and some passages in IX. Outside Book III there may be an instance at x 312-313. Possibly too the consistent alignment of the mss. in three places in satire VIII where there is a cover-name alternative (Damasippus) to the muleteer-consul's real name (Lateranus) at VIII 147, 151 and 167 points to an author-variant here too. A few more marginal cases cannot be treated within the scope of this paper<sup>14</sup>.

2.31. The volumen-text whence all subsequent copies of Juvenal ultimately derived would not have been fault-free. Some of its minor lapses may well have been put right in the first transcription into a codex, for copying need not always

<sup>12</sup> In this connection one thinks of numerous instances where papyri give the latter part of a work whose beginning is lost, as e.g. P. Oxy. 841 (last half of Plato's *Symposium*), 1232 (end of Sappho's First Book), and other texts, of which St. Mark's Gospel is perhaps the most striking example.

<sup>13</sup> For proof, see P. Wessner, *Scholia in Iuvenalem Vetustiora* (Leipzig 1931) xxxvif.

<sup>14</sup> On these passages see my observations in *Festschrift Bruno Snell* (Munich 1956) 101f. I am not convinced by an attempt in this direction by F. Jacoby in *Hermes* 87 (1959) 449 in regard to III 10-21 and VII 36f.



have been a one-way process of degeneration (see 3.22 below). The more serious errors which occur in all our extant mss. are however best attributed to the hazards of transcription: of these 'primitive errors' (to use a handy label) I regard the following as the hard core<sup>15</sup>:

- III 109 ... †*aut ab inguine tutum*† ... (see Cl. Rev. 75 [1961] 55 n. 3)
- IV 13 †*serioque*] *Seiioque* Housman (anticipated in two late mss., Goth. 52 and Laur. 34, 34, but probably by conjecture)
- VI 158 *incestae dedit hunc*] *incestae gestare* Housman
- 172 *depone*] *dea, pone* Graevius
- VIII 27 *alio*] *alto* H. Richards (Cl. Rev. 13 [1899] 19)
- 38 *sis*] *sic* H. Junius (also, whether by a fluke or by conscious emendation, in Brit. Mus. 11997, written in 1441)
- 68 *primum*] *privum* Salmasius
- 105 †*hinc Antonius*] *Antonius ebrius* J. Ashton (see Cl. Rev. 75 [1961] 56 and n. 1)
- 223 *quid*] *quod* Peyrard (as also Urb. 342, but cf. VIII 38)
- 240 †*in*] *igni* D. S. Robertson (Cl. Rev. 42 [1928] 60–61)
- IX 106 *taceant* or *clament*] *fac eant* Haupt
- 110 *librarius*] *libarius* anon.
- XIII 48 *aliquis*] *imi* Housman
- XVI 24 †*tot caligatos*] *tot caligas, tot* (emendation, but in two mss. known to Ruperti and in one known to Dempster: see Housman, p. xvii)
- 25 *absit*] *adsit* S. T. Collins (Cl. Q. 3 [1909] 279)
- 56 *labor*] *favor* Ruperti

2.32. Although not all the corruptions listed in the previous paragraph are simple confusions of one or two letters, most of them are. It would seem that the more spectacular of the multi-stage corruptions, such as that at VIII 148, originated at a later phase in the transmission, probably around 800 A.D. This is to some extent confirmed by the survival of the truth at that place in four witnesses, CGU and Val. 410.

2.33. There is however nothing wrong with the text at v 104<sup>16</sup>, as was shown

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<sup>15</sup> Among other passages which seem to contain primitive errors not yet certainly resolved are:

- VIII 111–112 (error not easily localized)
- XI 56f. ... †*vita et moribus et re*†
- XIII 70 *miris*] *miniis* Porson, alii alia

To these should probably be added:

- VI 57 *cedo*] *credo* A. Thierfelder (Hermes 76 [1941] 317–318)
- 107 *sicut*] *sulcus* Nisbet (JRS 52 [1962] 235)
- 589 *aurum*] *atrum* Killeen (Hermes 94 [1966] 119)
- XIII 44 *siccato*] *saccato* Schurtzfleisch
- XIV 269 *perditus* ac †*vilis*† ?*perditus* ac *fatuus* (to be discussed elsewhere)
- XV 85–86 *Prometheus* / *donavit* ?*Prometheu*, / *donasti* (see Cl. Rev. 75 [1961] 57) .

<sup>16</sup> On this passage see also G. Giangrande in Hermes 95 (1967) 118f.

by A. T. von S. Bradshaw (in *Cl. Q.* 59 [1965] 121f.), or again at xii 61 (to be discussed elsewhere, see *Cl. Rev.* 75 [1961] 57).

2.4. A number of these primitive errors are due to haplography or other reason which gives no clue to the handwriting of the volumen-text which was misread in the course of transcription. The few that may be indicative appear to be letter-confusions such as *b* with *d* (xvi 25), *s* with *t* (xvi 24) or *t* with *f* (ix 106). These suggest a semi-calligraphic hand "blending literary and documentary styles" in C. H. Roberts' words (*Proc. Brit. Acad.* 40, 197–198). One might think of a hand such as that of P. Oxy. 2088 (vol. xvii, plate III) where these three confusions are optically easy. If the curl of the *s* was made with a dry pen or one of the strokes of the *f* imperfectly done, the remaining traces could be mistaken for a *t*. *b* and *d* are also similar in this, as in other semi-formal hands: see further Mallon, *Paléogr. romaine* (1952) 42f. 49f. There may be other documents which I have overlooked that fit the conditions better, but P. Oxy. 2088 serves well enough to indicate the kind of hand to envisage; the contents of that papyrus, an unidentified fragment of an antiquarian work, show that it was thought suitable for a work with literary pretensions.

2.51. Though disfigured by corruption, the codex-transcript will not have carried, for reasons which will appear, any of the spurious lines present in all (or almost all) our extant mss. but rightly excluded from modern editions. No two critics will agree exactly even on a short-list of them; they are, I believe, less numerous than some extremist eradicators would claim. Isolated instances may still await exposure: the most recent to be shown up in its true colours is vi 568 (see Nisbet in *JRS* 52 [1962] 235). The hard core of supposititious verses would seem to consist of:

- iii 113 (see Garrod in *Cl. Rev.* 25 [1911] 240)
- v 66. 91 (om. PRW). 140
- vi 188. 140. 568 (see above). 614abc
- vii 135
- viii 258
- x 225–226 (a decency-interpolation, as substitute for 221–224, concocted out of the idea of xiv 86f. and the phrasing of i 25, which is structurally necessary in the context of satire 1)
- xi 99
- 110 (the probability against two successive lines containing both this caesural pattern and the monosyllabic ending is, it seems, of the order of 350–1; mathematics apart, the banality of 110 should condemn it)
- 161. 165–166
- xii 50–51
- xiii 166. 236
- xiv 1a (om. P, FU). 117
- xv 97–98

There are certainly others, but it would be hard to obtain unanimity on their identification.

2.52. As with few exceptions all of these spuria are found in every ms. that has come down to us, it would seem that they were injected into the text over a comparatively short period of time; otherwise their incidence should be much more sporadic than it is. Can this time be pin-pointed? It was probably distant by a considerable interval from the date of composition of the satires: this seems a necessary inference from the triviality or banality not only of these lines but also of some other suspected ones. We are thus virtually committed to thinking in terms of a date around 400 A.D., when Juvenal was, we know, being widely read. Indeed it is only when an author is in general circulation that his text is likely to attract such accretions. The point could be put more strongly: is it really conceivable that anyone should think up additions if they were destined for oblivion in the text of a virtually unknown writer who, for all that could be divined at that time, was likely to remain so?<sup>17</sup>

2.53. We must thus suppose that at about this time some person (or persons) unknown compiled, with that love of completeness which characterized Latin scholarship throughout antiquity, as full a text of what was taken to be Juvenal's works as was then available. This became the standard version. Nothing warrants the identification of this man with the Nicaeus of the subscriptio common to K (Laur. 34. 42) and L (Leid. 82), even if nothing forbids it either. This man has a slight but inalienable interest for us in that he confirms Ammianus' statement (2.12 above) about Juvenal's popularity at this time, and testifies to studious intentions in regard to him by a member of the so-called Servian Circle. Nicaeus' scholarly attainments are quite unknown: the odds are that he was little more than a dilettante, such as Sabinus and others who are hardly more than names to us<sup>18</sup>.

2.54. It is usual to postulate another text, put out rather later, which carried an interpolated version, with frequent facilitations of syntax (not that Juvenal, truth to tell, offers much difficulty on that score), emasculated readings and other aids to an ignorant or bashful reader. Thus the mutable allegiances of our extant mss. would be accounted for in terms of contamination in varying degrees between the divergent readings of the two hypothetical standard texts. A possible but hardly a necessary view, with at least one disability, in that it is not easy to suggest a date for the compilation of the interpolated version that squares with the evidence for its supposed effect. For if there had existed a ready-made source of interpolation in the sixth century, one would expect to find more interpolation

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<sup>17</sup> Furthermore, at least one metrical oddity (*érgō*, III 281) is alien to Juvenal's practise but consistent with the reversion to Augustan prosody characteristic of the age of Ausonius (cf. 185, 7; 215, 7 Peiper) and Claudian (xv 260, al.).

<sup>18</sup> On these gentlemen-revisers, see a judicious note by W. V. Clausen in *Hermes* 91 (1963) 252f.

than one does in the Juvenal-quotations preserved in Priscian, while a date much after 550 is unlikely, in view of the falling off of interest (and copying activity) in regard to literary texts from then on until the Carolingian Revival in 800. An alternative hypothesis will cover the facts: we need only think of the one definitive edition we are bound on other grounds to postulate as having acquired variants, marginal or suprascript, which multiplied in the course of time in its descendants. If this is what happened, the problem confronting us will be one of determining the mechanics of variant-exchange. I have assumed a single variant-carrying edition in what follows here, but the argument would not be vitiated if evidence were one day forthcoming to guarantee the existence of the putative 'second edition' which I have provisionally discounted.

2.611. The bulk of Servius' 73 quotations come from passages where no doubt attaches to the reading. Allowance must be made for unverified memory of details irrelevant to the expository purpose and for accommodation of words quoted to Servius' own train of thought: thus his quoting of x 143 with *rupit* (not *rumpit*) may be due to his having made (in his note on Aen. x 13) a statement in past tense about Hannibal. Servius' mss. may also have become infected with interpolated variants put in by scribes whose knowledge of Juvenal went back to the debased texts they had used at school: this may account for the alternatives *Cordi* and *Codri* which occur in two different citations of i 2. Where interpolation might have been expected, Servius often knows nothing of it, as e.g. in quoting vii 100 (see 2.22 above), x 2 (*dinoscere*, no trace of *discernere*), ix 84 (*actorum*, not *auctorum*) or xii 1 (*dulcior*, not *carior*). Cases such as xv 168 are rare<sup>19</sup>, and it seems safe to say that Servius' text was basically a good one, whose few aberrations can be explained simply and without special pleading.

2.612. A number of Priscian's 116 quotations are obviously garbled (e. g. xi 203) and he has a curious trick of joining the beginning of one line to the tail of the preceding one, as at ii 130-131 or ix 59-60. Such quirks of unverified memory do not deceive. He twice quotes xi 55, each time with the only possible reading *et fugientem*, which has vanished from all but two of the medieval mss., and displays scholarly virtue elsewhere, notably at vii 77 (*leviori*), 139 (*fidimus eloquio?*, see 2.22 above), viii 225 (*cantu*), x 150 (*aliosque*) and 281 (caesural hiatus preserved). But whereas Servius only shows isolated cases of interpolation, there are about half-a-dozen clear instances in Priscian and perhaps as many more doubtful ones. Among the more obvious are i 168 (vocative *anime* as against *animo*), ii 93 (*tinctum* correctly quoted at one point in Priscian, but ousted by *tactum* in 6 out of his 8 mss. on its second appearance), iii 232 (*illum* as against *ipsum*), xiv 30 (plural *moechos* as against *moechum* in four citations consistently), 293 (*coemptor*

<sup>19</sup> Here Servius offers what one would take to be the more obvious and as such possibly interpolated variant *nescirent ... extundere* as against the more recherché *nescierint ... extendere* peculiar to P and A. Conceivably there was textual instability at this point early on; one may even have to do with an author-variant (2. 22).



as against *coempti*). Priscian's grammatical training and interests may have saved him from some pitfalls, and so his text may not be an entirely fair pointer to the state of things in his day elsewhere. None the less the provisional inference would seem to be that while interpolation has gained ground since Servius' day, it is not yet serious.

The three surviving fragments of mss. giving portions of Juvenal's text prior to 550 A.D. point to the same conclusion.

2.62 Vat. Lat. 5750 (illustration in CLA I 30). This, the so-called 'Bobbio fragment', carries 52 lines of Persius (I 53–104) and 51 of Juvenal (xiv 324 to xv 43; the title of xv occupies one line). Lapses are fairly numerous in both parts, though some are quite minor. Others are more disconcerting: at this early date (probably around 500 A.D.)<sup>20</sup> the unfamiliar *aeluros* had disappeared at xv 7 (to survive only in U<sup>ac</sup>) and the simple verb *duxerat* at xv 25 had given place to the unmetrical compound *deduxerat*. The first is plain corruption, but the second verges on interpolation. To offset these misadventures, the proper name *Iunco* survives intact at xv 27; this is only found in a few of our medieval mss., though indicated as a correction in some others. In company with the interpolati, the Bobbio-fragment preserves the spondeiazon-ending *et Cyclopas* at xv 18 which has been converted to *atque Cyclopas* in the normally more reliable mss., P(S), A and T. On the whole it seems that corruption has done more damage than interpolation in this witness.

2.63. The Ambrosian fragment (Milan, Cimel. 2; illustrated in CLA III 305) of the sixth century covers parts of xiv 250–256, 261–264, 268–291 and 303–319. In about half-a-dozen places careless copying is to be detected; in only one of the five places where interpolation might be expected to occur does it actually show itself, at 289 (*uda*). Its main contribution is that it carries back to the sixth century the corruption at xiv 269 (see note 15 on 2.31 above) and thus disqualifies Housman's over-ingenious restoration (*ad loc.*, and Manilius vol. I, p. xxxvi) which depends on a form of abbreviation undreamed of at this early time.

2.64. Five agreeably controversial passages in satire VII occur in the 50 lines preserved in the parchment fragment from Antinoë, written about 500 A.D.<sup>21</sup> It is surprising to find a relic of what must once have been a good-quality copy of Juvenal turning up in Egypt, and one may well wonder whether it represents a path of textual descent, distinct from the 'Western' tradition we have been following. Assessment of its more striking readings is delicate, for one must be careful to discount any prejudice in their favour which arises from the age and

<sup>20</sup> Specimens of Rustic Capital go back to the earliest centuries, but the dating of the 20-odd surviving mss. of literary works in this hand is a controversial matter. It would seem that there are grounds for favouring a later date (i.e. post 400 A.D.) for the oldest specimens than was at one time believed.

<sup>21</sup> Not in *CLA*, but presumably to appear in the forthcoming supplementary volume. Facsimile in *Journ. Egypt. Arch.* 21 (1935): transcription also accessible in R. Cavenaile, *Corpus Papyrorum Latinarum* (Wiesbaden 1958) 114–117.



interest of the document which carries them. I believe however that any future editor must consider very seriously all of those noted below:

vii 149 *imponere* (as against *ponere* of P, Vat 3286 or Bücheler's emendation *poscere*)

153 *isdem* (as against *idem* of GU, but not A which has been misreported)

156 *fronte* (as against *parte* or *forte*: cf. too C. E. Stuart, Cl. Q. 3 [1909] 7)

177 *scindens* (as against *scindes*: the participle is supported by the interlinear gloss *dividens* in Antinoë as by the P-scholion p. 131 Wessner and the desired sense can be deduced from Quint. prooem. 13 and Sen. Epp. 89, 16. See further Cl. Rev. 75 [1961] 53 n. 3)

185 *componit ... condit* (as against *componat ... condit/-at*)

Antinoë also exhibits a diacritical sign of uncertain meaning opposite line 192: this verse is obscure, but not spurious and the sign may have been intended to draw attention to a note elsewhere. The presence of such a sign is of special interest and confirms an acute guess of Knoche's as far back as 1927, who thought they may have occurred in the earliest copies of the text. Even when allowance is made for the unusual richness of satire vii in textual difficulties, the survival in this document of so many significant lections, whether right or wrong, within the small compass of 50 lines is noteworthy.

2.7. The three fragments briefly described above together with the 180 lines quoted by Servius and Priscian embrace barely 10% of the total of Juvenal's surviving work (3870 lines). Inference from so limited a sample can be at best tentative, but the relative preponderance of corruption over interpolation seems quite clearly marked. Not that signs of interpolation are lacking: it is present, but only in bud, and for its full flowering we probably have to wait for the renewed burst of copying activity early in the ninth century. Now that all but the supplementary volume of CLA has appeared, it is plain as it hardly was before that the seventh and eighth centuries yield next to nothing in the way of mss. of classical authors. Theological and liturgical works abound and seem, when all possible allowance has been made for accident of survival, to have absorbed practically all scribal effort in the West during this period. Thus the evidence we have been surveying may well give us a fair picture not only of the state of Juvenal's text as it was about 550 A.D., but as it was to remain until the turn of the eighth and ninth centuries, when the next phase begins.

### *The second phase, after 800 A.D.*

#### *A. The closed element in the tradition*

3.1. The line of demarcation between the mass of interpolated mss. and the small cluster comprising P (Montpellier 125, written c. 900, almost certainly at Lorsch in South-West Germany) and its congeners is obvious enough. Of these only P contains the whole text of the satires (less of course the 'Oxford' fragments, of which however a hint survives in the P-scholia to vi 345, p. 96 Wessner). Within

this charmed circle direct copying seems to have been the rule, uncomplicated by interpolations or, what amounts to the same thing, intrusive variants. Relationships can therefore be established by rule-of-thumb methods. The known congener documents are:

3.11. W, Vindob. 111/107, containing I 1 to II 59 and II 107 to V 96, in two columns of 55 lines each. Occasionally W shows to advantage over P, as at III 67 (spelling of the difficult Greek word *trechedipna*) or possibly at III 18, where however the reading of P before correction cannot be determined. W was written towards the later part of the ninth century, and so is a little older than P. It was accurately collated by Goebel in Sitzber. Wien. Akad. 1859, 37 f.

3.12. R, Parisinus 8072, tenth century, containing I 1 to II 66 and III 32 to VI 437. It has a single column, 34-line format: the omission thus corresponds to the loss of a double leaf. Where interpolation has entered P, the process has sometimes gone further in R, as at IV 144. There the correct *exire* has become *exhibere* in P, but R has a disastrous attempt to mend matters with *adhibere*, whence the truth could never have been recovered. Something similar has happened five lines later, at IV 149. It seems to be a 'nephew' of P, rather than a gemellus, as rightly divined by C. E. Stuart in his collation of it in Cl. Q. 3 (1909) 1 f. Distinctive readings peculiar to W, R and P are plentiful, as at I 67, III 105, IV 43 and elsewhere; as tell-tale as any is the trivial linking-error at IV 51, the ghost-word *despastum*. Neither W nor R have scholia, but it is likely that R's exemplar had. Otherwise its curious reading *Egeum* for *Ionium* at VI 93 is puzzling, though readily explicable as an import from the P-scholia to this line (Wessner p. 79 line 4).

3.13. C, Florilegium Sangallense 870. Written at about the same time as P, this contains a collection of 459 lines of Latin poetry, 282 from Juvenal. This anthology is followed some pages later by a complete transcript of the scholia with lemmata, identical, except for quite trivial details, with those in P, but lacking an accompanying text. Two of the Persius-citations<sup>22</sup> suggest that the anthology text had some value independent of P, but XIII 107 does not prove the same for Juvenal, as the reading *confirmat* has been misreported: it is the lemma of the C-scholia and the anthology does not contain that verse. C has however usefulness in telling us on occasion what the reading of P once was, where that is now no longer legible, as at VIII 148.

3.14. Somewhat earlier, another anthology had been compiled by one Mico, a monk from St. Riquier (Centula, near Abbéville), in or about 825 A.D. at Reichenau on Lake Constance. Though he only cites 31 lines of Juvenal, his readings are so close to those of P that there is no doubt, even on the strength of this tiny sample, that he took them from a P-stream document, which would almost certainly have been a direct ancestor of P itself. Thus at XIV 122 he not only

<sup>22</sup> See Clausen's edition of Persius (Oxford 1956) XI.

shares with P and C the idiomatic ablative *via* (*viam* *rell.*) but also the transparent corruption *peragant* for *pergant*<sup>23</sup>.

Brief mention suffices for two documents which can safely be left out of count for the rest of this enquiry:

3.15. Q, Schedae Arovienses, now at Aarau in Switzerland. These much-mutilated fragments contain portions of satires II, III, VI and VII. When complete this ms. must have been a high-quality representative of the P-stream. It has exact identity of format and *mis-en-ligne* with P, as regards both text and scholia. Unfortunately a careful study of its few divagations from P (which cannot be undertaken here) raises a strong suspicion that it is a descriptus of P; just possibly these sheets may have come from another copy of the same exemplar, ruled and transcribed at the same time and place as P. The question cannot be decided on the basis of the published reports of readings, which autopsy has shown to need correction in material details, as e.g. at VI 297. In any event Q contributes nothing of value towards the establishment of the text: in doubtful places where P is indecipherable, so is Q. In so far as it carries less over-writing than P now does, it could have value for enquiry on this rather specialist line, but it need not cumber any future apparatus.

3.16. I also leave out of reckoning the curious palaeographical freak, the Orleans fragment of 'Spiegelschrift' found on the inside covers of a Fleury anthology. This when deciphered proved to contain a text of Juvenal in mirror-image form but in great part legible which runs from II 32–89 and III 35–77 and 79–93. This last section exactly corresponds, save for the omission of III 78 and the consequential inclusion of III 93, to the contents of folios 17<sup>r</sup>, 17<sup>v</sup>, 20<sup>r</sup> and 20<sup>v</sup> in P. Identity of format does not seem however to go hand in hand with textual affinity: the Orleans-fragment is illegible at some crucial places, but only twice does it show P-stream readings (III 37 and 58) as against 4 where it does not (II 34. 80; III 61. 80: add too II 82 and III 60), while at two points it lapses into downright idiosyncrasy. Disappointing, for there is reason to believe that the original text thus fortuitously preserved may have been penned about 850 A.D., earlier than either W or P. The whole question is discussed in an interesting article by the first researchers to publish the fragment, McKinlay and Rand, in *Harv. Stud. Class. Phil.* 39 (1938) 229, with an important addendum by B. M. Peebles (*ibid.* 261–263).

3.21. There is no need here for a full statement of P's virtues and defects. A good formal description is given in Wessner's edition of the Scholia (1931 [reprinted 1967] viii ff.), although Beer's account in his *Spicilegium Iuvenalianum* (1885) 9 ff. is fuller and still valuable. Essentially P represents a tradition that had largely escaped interpolation of the bowdlerizing or facilitating kind; this general sincerity appears in its congeners too, which can thus be treated as a 'closed' element in an otherwise wide-open transmission. It carries a good deal

<sup>23</sup> On Mico, see the interesting account in Knoche, *Grundlagen* (1940) 234f.

of corruption, much of this minor and mostly transparent. A number of careless errors used to be laid to the charge of its copyist, but some of these have since turned up in the congeners too, so that the negligence lies further back in time. An example is the ghost-word *stetis* appearing in both P and R at I 149 (cf. I 152 and XI 36, where the unmetrical *emitur* at line-end is found in both P and C).

3.22. Neither is there need to list here the places where P, accompanied on rare occasion by the lemmata of the scholia, exhibits manifest interpolation, or other passages where the truth has to be recovered from the generally less sincere mss. Essentially the P-tradition can be treated as a closed one, but there are tell-tale places which show how precariously it survived as such. Consider XII 128, where there is no doubt that Juvenal wrote:

*vivat Pacuvius quaesio vel Nestora totum,  
possideat quantum rapuit Nero, montibus aurum*  
130 *exaequet, nec amet quemquam nec ametur ab ullo.*

The variants in 128 are:

*pacuvius*] *pacubius* Pac C, Mico (also A, but no matter)  
*totum* P, *rell.*: *tantum* C (two citations), Mico

The first entry, trivial as it is, emphasizes the close affinity of P, C and Mico; what of the second? Some 70 years before P was written, the nonsensical *tantum*, misbegotten from *quantum* in the next line, had become lodged in the P-stream. Yet in P the truth has reasserted itself, it can only have been from a variant or an outside source. In an 'open' tradition such reversals of the usual process of degeneration are understandable: here we have an instance where we might least have expected to find one: see 4.2524 below.

3.23. Although there are sometimes clear indications, there is frequently a difficulty in distinguishing the several later hands that have at various times defaced the original reading of P. This has been, in my experience, underestimated: a good instance is at VI 527, where inspection of P has satisfied me that Clausen's apparatus here alone points to the truth (*calidaque* Pac: *calidasque* *rell.*). Thus in default of decisive considerations, such as letter-spacing or other chance aid, I am reluctant to go further in reporting readings than Pac or P<sup>pc</sup>, suspending judgment on the precise relation between first writing and correction. P appears to be a ms. that will only yield up its more intimate secrets to the sustained scrutiny of an expert blessed with the leisure and patience to live in its company over a long period, as Rostagno did with the Mediceus of Tacitus or Studemund with the Ambrosian palimpsest of Plautus. Such undisturbed diligence belongs to an ampler age than ours: we, *viles pulli nati infelicibus ovis*, can only say, with the Elder Pliny, *oportet nos aliqua nescire*.

### *B. The open tradition*

4.11. I now turn to those other mss. which have been collated in sufficient detail to make quantitative comparison possible. There is an undoubted element



of randomness inherent in the data, in as much as the choice of mss. for collation was made by different individuals long ago, and was, so far as can be seen, largely a haphazard one. Indeed much of the work of collation was undertaken before many of the possible candidates for scrutiny were known or even listed. It may well be that the choice was on the whole not a bad one. Whatever its merits or failings, it is only practical to make the most of the information at present available. Any improvement to the text of the author will thus be consequential, neither is this an effort to establish an order of merit of the witnesses being examined. This would be tantamount to reviving the meaningless concept of a 'best ms.'. What I set out to do is to explore a promising method of achieving a meaningful calibration of the superficially disparate array of manuscript-characteristics in terms of 'near-neighbour' affinity, which is quite another matter. If an arrangement of clusters of mss. can be detected underneath the confusion of data before us, the path to the next stage of enquiry may be clearer than it would otherwise be. If the process is properly conducted, it should result in a scale of such near-neighbour relationships, with the most heavily interpolated documents coming together at one end of the scale and as far apart from the sincerer witnesses as possible. Clusters with less pronounced characteristics should appear in the middle section of the line, which can be thought of as similar to a spectrum-line in the field of Physics or Chemistry. It is essential to remember that the operation is conducted in terms of similarities and differences between the several mss. Thus there is no question of forming provisional judgments about the rightness or wrongness of a reading which is open to doubt. In the conventional methods of evaluating mss. there is an inherent danger of circular argument, from which the taxonomic process is free.

4.12. From what has just been said, it will be clear that one must cut oneself loose from the preconceptions and limitations of the conventional family-tree presentation, whose inadequacy has become increasingly evident as the intricacies of such open traditions come to be better understood. A further advantage of the process is that the investigator is entirely ignorant of the way the data he is analysing will work out until his counting is completed and he comes to the final stage of the resolution of the figures he has arrived at for each section of the satires. This will become clear when the *modus operandi* is explained (5.21 ff. below). Before doing so, some remarks are necessary on the presuppositions of the method to be employed and on its suitability to the present problem.

#### 4.2. *The mechanics of variant exchange*

4.21. Scientists have long been aware of the limitations of the traditional methods of classifying specimens; biologists in particular have laboured under this handicap. Within the last 10–15 years considerable advances have been made, largely because techniques developed for computer use have enabled specialists in this activity, who style themselves numerical taxonomists, to sift with speed



and precision large masses of unpromisingly heterogeneous material, and thereby to isolate groups or 'taxa' of related specimens, on the basis of which further enquiry may be conducted. Not surprisingly the underlying assumptions of this approach were at first called in question. It has now proved its worth and is regarded in scientific circles as being intellectually respectable, provided, of course that it is properly applied. There is still room for debate on technical points, such as the need for weighting factors (which do not arise in all cases and have, after some deliberation, been excluded from this essay), but these do not affect the usefulness of the method. For a textual critic operating with only a few thousand lines of text it is simply not worth the trouble of programming the data for machine-processing, anyway for a first assault on the problem; this might however arise later. Where the number of points of divergence is relatively modest (about 800–1000 in the case of Juvenal), the best computer for the job is located between the ears of the investigator. But he is not debarred from turning to advantage the methods of taxonomy, even if he finds it convenient to adhere to time-honoured methods of handling the material.

4.22. So long as one is dealing with compact arrays of data which show characteristics common to the whole group of specimens being studied, the accepted methods are generally adequate, in biological study as elsewhere. Such systems are now called 'monothetic' and their treatment and interpretation are mostly straightforward. They are obviously analogous to a 'closed' textual tradition, in which features such as lacunae of identical extent common to a whole group of mss. tell their own story. 'Polythetic' systems are another matter. Here characteristics are not universal to the group under investigation: thus they exist in nature birds without wings, mammals lacking red corpuscles and countless other 'untidy' phenomena to bedevil classification. Concentration on single characteristics leads nowhere in such situations, and attention has to be directed to an aggregate of properties. Certainly, acute observation, aided by luck, may detect some striking features of resemblance, but discovery of this kind is at best fortuitous and at worst misleading, in that what has been observed may have come in only at a late stage in the evolutionary process, and so mean less than it might seem. Obviously, for the taxonomical approach to be valid, a substantial number of points of similarity between specimens or groups of specimens is essential. There seems general agreement that a minimum of 40 mutually unrelated elements should be taken for any single operation, but a figure of the order of 60–70 is safer<sup>24</sup>. If therefore in a literary text a significant divergence occurs about once in every 4 or 5 lines, reliable results may be expected where stretches of not less than 300 lines have been treated; in most of what follows considerably longer

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<sup>24</sup> For these figures I rely on R. R. Sokal, *Numerical Taxonomy* (1963) and on conversation with his colleague Dr. P. H. A. Sneath of the Microbial Systematics Research Unit at the University of Leicester. At an earlier stage Dr. J. Rollett of the Oxford University Computing Laboratory gave me invaluable help. This seems the place to record my great appreciation of

blocks have been used. Caution is however needed here: if too long a section is taken, any changes of relationship that may occur in the course of it will be obscured or lost sight of altogether, and these changes could be just what one ought to be particularly alert to sensing.

4.23. Choice of break-points is inevitably a somewhat arbitrary business. I list the mss. concerned, in addition to those already discussed, P (3.21f.), R (3.12) and W (3.11). I use the sigla commonly adopted elsewhere, except in two cases, denoted by an asterisk, where I have had recourse to capital letters of the alphabet, hitherto unemployed, for obvious reasons of convenience.

- A Monacensis 408; eleventh century
- B Leidensis Voss. F 64; tenth century
- F Parisinus 8071 (Thuaneus); early tenth century. Commences at III 316 and lacks IX 40–150
- G Parisinus 7900a (Puteanus); tenth century
- H Parisinus 9345; eleventh century
- \*J Vaticanus 3286; eleventh century, in Beneventan hand but only available for I 1 to X 366 as the rest of the text is in a fifteenth century hand and of no value (= Knoche's I)
- K Laurentianus 34, 42; eleventh century
- L Leidensis Bibl. Publ. 82; eleventh century
- \*N Vaticanus Reginensis 2029; early eleventh century (= Knoche's r)
- O Oxoniensis Canonici 41; early twelfth century, in Beneventan hand
- T Cantabrigiensis O IV 10; tenth century, in Anglo-Saxon hand
- U Vaticanus Urbinas 661; early eleventh century
- V Leidensis Voss. Q 18; tenth century
- Z Londiniensis Mus. Brit. Add. 15600; ninth to tenth century

Further investigation may be justified in the case of (see 6.61):

Val Valentianensis 410; eleventh century

Not all the mss. under scrutiny contain the complete works of the satirist, yet to omit any on this ground would be to sacrifice valuable information. It would be possible to take the longer satires individually and to group some of the shorter ones such as IV and V, or XI and XII in pairs: this would give the desired lengths. It is however more practicable to set the break-points at places where a significant witness begins or ends. This makes each operation self-contained and simplifies comparison later. One can after all compare two ranking-lists, one containing 15 and the other only 14 items by simply omitting the 'odd man out' from the comparison and renumbering the remaining items in the longer list. I have therefore broken down the work into 8 phases, as follows:

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their enthusiastic yet critical interest in this project. Such ready encouragement to an 'amateur' temerarily mounting an invasion of territory lying right outside the confines of his own specialist interest surely goes some way towards dispelling the illusions begotten of that pseudo-myth of the Two Cultures, at least in so far as the scientists are concerned.

1. (a) I 1 to III 31. Unfortunately W is defective for II 60–106 and R lacking for II 67 to III 31. As these two mss. behave in a disciplined manner, their relative positions vis-à-vis the remainder can be ‘interpolated’ (in the scientific sense) without difficulty.
- (b) III 32–316. For this stretch we have W, R and the rest, but not F.
2. III 317 to v 96. For this we have WR, F (which begins at III 317) and the others. W ends at v 96.
3. v 97 to VI 437. This takes us to the point where R ends. From this point on only P remains to represent continuously the sincerer tradition.
4. VI 438–661. Shortly after the start of this section G shifts its allegiance, as shown in detail at 4.27 below and in 9.2 (with documentation).
5. VII–IX. Corresponds to Book III (see 2.21).
6. x 1–366. At the end of this satire J drops out: the rest of its text is written in a fifteenth-century hand and is so heavily interpolated as to have no value for our purposes.
7. XI–XIII. Towards the end of XIII G (see above) reverts to its earlier state (i.e. to its behaviour between I 1 and circa VI 475).
8. XIV 1 to XVI 60.

4.24. The problem is posed, and the data-sources enumerated. Before embarking on the arithmetic, something must be said on the fundamental question: is it legitimate to apply the methods of another discipline in the manner proposed? When a biologist studying, let us say, 27 specimens of nematode worm, has tabulated their several points of similarity one with another and plotted the resulting number-count onto a similarity-matrix chart, he has available a procedure whereby he can rearrange his 27 specimens into near-neighbour groupings or taxa, which in this case will correspond to species or sub-species. An illustration of just such a procedure is shown at Appendix ‘A’ (9.1, below). What the biologist has observed as a basis for his classification is in essence a reflection of the several genetic histories of his 27 specimens, whose similarities and differences result from the varying degrees of ‘penetrancy’ (i.e. ability to ‘breed out’ in a subsequent generation) possessed by the ‘mutant-genes’ in their inherited make-up. These can be further studied under laboratory conditions and the significance of any anomalies tested by specially designed experiments, repeated as often as may be necessary, until evidence is forthcoming which is sufficient as a basis for conclusions or further hypotheses. At this point an obvious objection must be met: surely, it will be said, all this stands on a quite different conceptual level from the activity of the copyist and the textual critic: are not laboratory and scriptorium worlds apart? Certainly, if attention is focussed on community of subject-matter or experimental procedure, there are obviously few, if any, relevant points of contact. It is however another matter once the analogy is seen to be one of observational method, a matter of recording and analysing behaviour-patterns which may be unpredictable in any single instance, yet are not random happenings.

Here there is common ground between the disciplines; the analogy between the behaviour-patterns of mutant genes in biology with those of interlinear or marginal variants in mss. is a viable one, as a little reflection will show. Once these patterns are established, the two enquiries go their separate ways, but not till then.

4.25. Variant readings, whatever their origin, may affect a ms. in which they lodge in a finite number of ways; just as mutant genes 'play by the rules' (complex and obscure as these may often be), so do variants, whose possible effects can be readily enumerated, on the lines indicated by Bévenot in his book on the tradition of Cyprian (1961) 126-129:

4.251. The intrusive variant may be incorporated as a correction, effacing beyond recovery the reading previously there. In this case a copy taken from that altered ms. will inevitably show the alien lection: thus a copy made of P in its present state would at 12 certainly have shown *Codri* as the last word in the line, not the correct *Cordi*, unless by good fortune the clumsiness with which the alteration was done had roused suspicions in this particular instance.

4.252. The variant may have been written in, but the original left legible. In this case there are four possibilities open when the next generation mss. come to be made:

4.2521. The intruder is simply recopied alongside the reading already there, no effort being made at discrimination. Both readings are thus kept alive; for a good instance of this practice see Bévenot (1961) 127 n. 1: "A fourteenth-century ms. [of Cyprian] from the Benedictine Abbey at Abingdon (now Camb., Corpus Christi Coll. 25) is a copy of the twelfth-century ms. from the Cistercian Abbey at Buildwas (now Camb., Pembroke Coll. 154). It enters alternative readings in the margin in precisely the same way; verification would probably show that they were simply transferred from the one to the other."

4.2522. The intruder is ignored, so that in the next copy there would have been nothing to mark its temporary presence.

4.2523. The intruder is adopted into the text, and the original reading ignored. This has brought about the situation in 4.251, but at an interval of one ms. generation.

4.2524. The intruder is adopted into the text, but the original reading survives as either an interlinear or a marginal variant. In that event there is a possibility of the original reading finding its way back into the text in a subsequent copying: this is what may be the explanation of the reinstatement of *totum* at XII 128 (see 3.22 above).

4.253. It should also be kept in mind that any variant (original or intruder) which survived 'dormant', as in 4.2521 or 4.2524, may have had the good fortune to continue so (its situation being that of 4.2521) for two or more generations, to 'breed out' later, in much the same way as mutant genes may 'skip' one or more generations. This is, I conceive, the likely explanation of the survival, often in



late and highly improbable mss., of readings whose nature is such as to preclude their being a scholar's guess; these must in consequence somehow stem from ancient tradition. Such a one is, in my view, *fronte* at VII 156, which outcrops in the Antinoë-fragment (2.64), in J (eleventh century) and sporadically thereafter in mss. of varying date, the latest being of the fifteenth century. Such precarious survival is economically explained in terms of persistence as a 'dormant' variant.

4.254. The behaviour of variants is instructive in another way. Two mss. may exhibit similarities so striking as to preclude chance and testify to indissoluble intimacy. Yet these features could have been acquired very late in the evolution of either or both mss., and so be meaningless as pointers to the underlying 'genetic history' we are trying to uncover. A better clue to affinity may be an aggregate of similarities, individually uninteresting but just because they are uninteresting less likely to be effaced by alteration, whether random or systematic.

4.26. But is not this to make too much of the part variants can play? Many surviving mss. contain none at all. True, but we must not forget that many we possess were destined to be library copies, and, while they might still have been used as exemplars, were likely to escape tampering which might befall a much-copied 'Stammkodex'. The fate of such a one is eloquently described by Dain (1949/1964<sup>2</sup>) 139f.: "... On gratte et on ajoute sans arrêt, pour le plus grand dam des yeux des éditeurs modernes. Comme d'un eczéma, nos livres souffrent de ces grattages continuels ... Tous les textes, à mesure que le goût du temps s'attachait à leur étude, subissaient une semblable transformation ... dans les marges ou dans l'interligne de l'exemplaire officiel, ou dans celles du prototype dont on disposait sur place, on ajoutait les nouvelles leçons, en les faisant précéder du signe *γρ* ... Si l'on corrigeait à l'aide de ces documents le texte de l'exemplaire de la bibliothèque ou du scriptorium, on reportait en marge la leçon primitive éliminée." Dain is thinking here primarily of Greek texts, but his remarks have a wider application. One thinks of the frequent overwritings in P, which illustrate this state of affairs, tersely summed up in Dain's phrase "l'éclectisme des mss.". For coexistence of variants in a Greek author Pasquali (1952<sup>2</sup>) 141-142 quotes the pleasing case of the mutually exclusive doublets in a ninth-century ms. of Epiphanius (Vat. 503) which in juxtaposition wreck the syntax. Here it is clear that one of each pair is an officious reader's attempt to improve the Christian Father's undistinguished Greek. These are easily eliminated, even if we had not an eleventh-century ms. (Marcianus 125) for control by comparison. For Latin one need look no further than the first verse of Juvenal's first satire as it stands in P. There above *numquamne* stands suprascript the ghost-word *numquine* in an ink not manifestly different in colour from that of the primary text. A copy of P as we now have it could perfectly well have offered this worthless nonsense-word in place of the truth. By a fluke this vox nihili actually stands in the text of H, a ms. which we shall see later to be of scant value but which seems to have had among its ancestors a ms. related to the source of some of the interlinear glosses in P.



4.27. The analogy with biological research which we are using, imperfect though it is, is probably no worse than that provided by many logical 'models', and, if applied with discretion, certainly less misleading than the defective one of the family-tree or stemma which has been made to serve till now. In really complicated cases an admirable ingenuity is often displayed in adorning the stemma with an elegant arabesque of dotted lines to show the presumed channels of contamination: a recent example is to be seen on p. xxv of K. Müller's 1961 edition of the text of Petronius' *Satyricon*. Granted however that the spectrum-line may be more informative than the stemma in such cases, one obvious objection remains. It might be urged that while the penetrancy of mutant genes is affected to some extent at least by environmental conditions, these are not random effects, whereas the loss or survival of a variant in a ms. could be due to nothing more than human caprice. Does this vitiate the analogy? Certainly the element of chance must have been present, but we may doubt if it was the only or even the main determining factor. It is not, I think, known whether any instructions, general or specific, were given to scribes in regard to variants in the exemplars. In this connection it may be useful to look briefly at the protean behaviour of G (Par. 7900); a documented statement of its shifts of allegiance and the evidence for them is reserved to Appendix 'B' (9.2). If we are prepared to believe that G was copied from 3 or 4 different exemplars, it ceases to have relevance in this matter, but this is on the face of it a rather unlikely supposition and on this view its reversion in satires XIV–XVI to the behaviour it had shown in the greater part of the first six would be puzzling. It is more economical to suppose that its abrupt changes of affiliation reflect a changing disposition of variants in its exemplar. Thus from I 1 to a point very close to VI 475, G offers typical 'interpolated' readings, with no obvious affinity to any well-marked ms.-cluster: for this section its claim to regular citation might be thought to be thin. At about VI 475 there is a sudden concentration of distinctively P-stream readings, not all of which are recorded in the collation on which every modern apparatus is based. Whether this happened because G stemmed from an exemplar in which a sincere 'primary' text had been over-written with debased variants only as far as VI 475, or whether it was the other way round, that is to say, a debased text received an injection of P-variants from VI 475 on, cannot now be determined, and while it would be nice to know, the argument would not be affected either way. However that may have been, a further change occurred in the early part of VII, where unmistakable U-characteristics make their presence felt, and these persist until the end of XIII. After that point G relapses into its 'interpolated' condition, much as it had been up till VI 475. Again it is open question 'which way round' the change was; what is interesting is the regular way in which the several types of variants make their appearance. It is of course possible to stand this argument on its head and say that the very regularity is consistent with changes of exemplar rather than with changes in the distribution of variants. It is not perhaps necessary to die in the

last ditch in defence of the hypothesis of variant-exchange here: in so far as the taxonomic method is concerned, it should be equally efficacious in detecting these changes, whatever their causes, and in fact it is. In this instance G's behaviour had been observed by direct collation before the taxonomic approach had been so much as considered; some other instances occur in which changes of affinity were brought to light shifts in the near-neighbour relationships.

5.1. Preliminaries disposed of, I turn to the arithmetic. For illustrative sample I have chosen in the first instance the third section (4. 23 above), covering v 97 to vi 437. These 500 lines run from the point where W stops to the end of the surviving portion of R; they are also in F, which begins at iii 317. Including J (Vat. 3286) and N (Vat. Reg. 2029), we have to deal with 16 mss., 2 of which represent the P-stream and show the close coherence of this, the 'monothetic' element in the tradition, contrasting sharply with the 'polythetic' behaviour of the remainder. For convenience of tabulation the count was done in two parts, 50 entries being made between v 97 and vi 222 and 26 more from vi 223-437. This is because a dot-count becomes hard to maintain accurately on normal-sized stationery after more than about 40 entries have been made. In addition to these 66 entries, 12 more were incorporated to represent readings that are the peculiar property of P and R. This figure could have been swollen by the inclusion of a number of trivialities which they share, but as it is sufficiently obvious that their sincerity will bring P and R out at one end of the spectrum-line anyway, there is no need to emphasize their individuality further. A higher figure would not, as will appear presently, have affected their relative placing vis-à-vis the others, so that it seemed appropriate to feed in to the calculation a reasonable if somewhat 'notional' allowance, rather after the manner of a 'constant' in mathematics. It is possible (though not verifiable without extensive re-collation) that if some of the other mss. had received the same intensive scrutiny as P and R, a number of other more or less trivial points of resemblance between them might have come to light. In any event the balance is best kept by concentrating on what is plainly significant as far as possible.

5.21. The procedure is simple, if somewhat laborious. The sigla of the mss. involved, 16 in this case, are written down the left hand margin and along the top border of a piece of foolscap graph-paper, 1" square rulings proving more convenient than centimetre-squares. A dot is then placed in each square that corresponds to a coincidence of reading in each pair of mss. The agreement of an isolated pair is recorded by a single dot in the appropriate square; three dots are needed for a trio of mss. in agreement, 6 for a quartet, and so on. Obviously an aberration or singularity on the part of an individual ms. has to be neglected, if only because there is nowhere to register it. (One might in such a case record the agreement of the other 15, but it is doubtful whether the extra labour would affect the result and isolated eccentricities are more likely to falsify the picture

than to clarify it.) The only critical activity demanded of the recorder is that he should be alert to exclude from the count any non-significant similarities, such as lines lost by homoeoteleuton (such as I 87–88) or homoearchon (as at II 24–25). Spelling trivialities are also inadmissible, as in these details scribes may have unconsciously conformed to ingrained habit and so not reproduced faithfully what was before them. The recording process is not however completed by merely filling in the squares wherever there is agreement between pairs of mss. and leaving it at that. Thus at v 116, PRA agree in *spumat*, as is shown by the three entries in distinctive hachuring (white bands running from 'North-West' to 'South-East') in diagram I below:

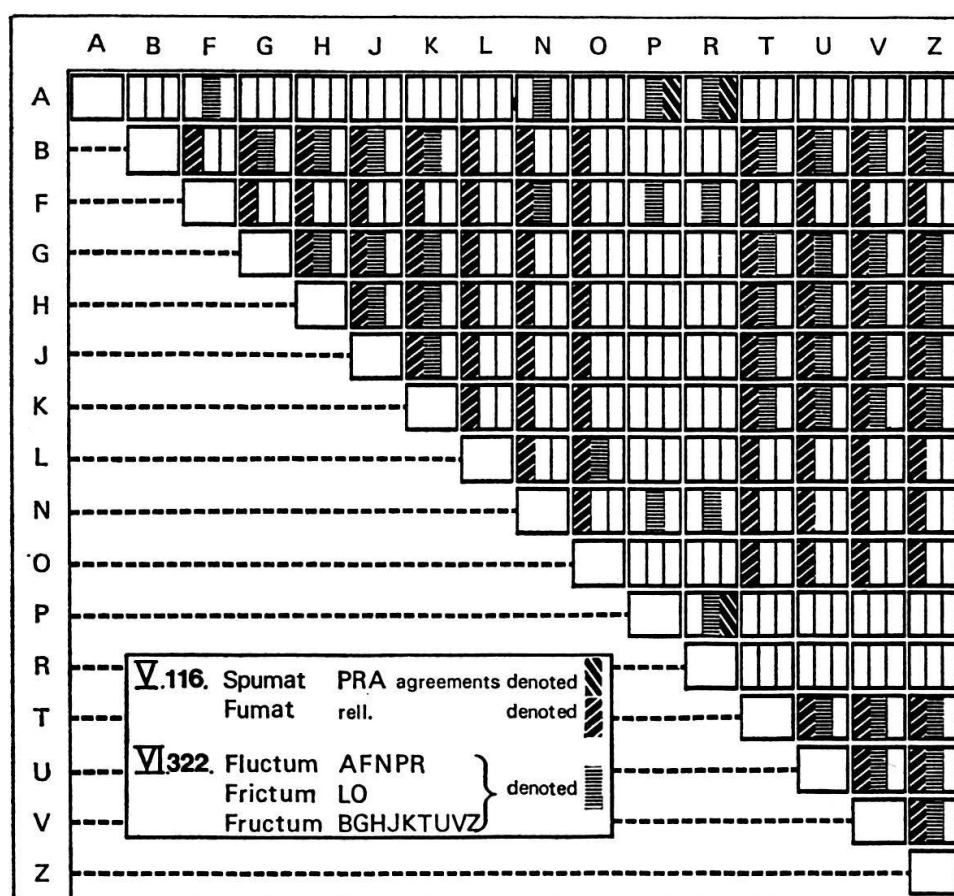


Diagram I.

But the agreement of the other 13 in the alternative *fumat* must be plotted in too, as has been done in the diagram by a rather different hachuring, with the white bands running from 'South-West' to 'North-East'. This second operation entails 78 recording acts, in addition to the three already made: in practise no distinction need be made between the agreement within a small and the agreement within a large group, which in diagram I has only been done for clarity of exposition. Obviously the least tedious distribution to record is a split of 8 mss. against 8, which requires only  $28 + 28$  or 56 recording acts in all. The maximum number occurs when 14 mss. agree against 2: this works out at  $91 + 1$  or 92 in all.

Where mss. split into 3 groups, each group is treated separately, as at vi 322. There AFNPR agree in *fluctum*, LO in *frictum* and the rest (BGHJKTUVZ) in *fructum*. This calls for 10+1+36 recording acts, or 47 in all, as is shown in diagram I. Reference to the diagram and its attendant key should dispel obscurity.

5.22. When the dot-count is completed, the number of dots in each square are added up and tabulated as below:

(A)	B	F	G	H	J	K	L	N	O	P	R	T	U	V	Z
A	50	44	61	49	52	48	62	62	48	42	44	53	61	47	43
B		65	68	88	58	66	68	58	59	14	15	76	50	88	73
F			52	61	52	58	59	49	50	27	23	61	43	67	68
G				69	56	63	77	62	62	20	22	69	56	67	59
H					55	61	65	66	54	18	20	71	54	83	70
J						51	60	52	56	30	33	59	49	57	58
K							68	62	60	28	26	71	59	65	66
L								63	63	22	23	68	60	66	64
N									55	26	24	60	59	55	57
O										22	25	64	54	59	55
P											82	25	33	15	24
R												25	33	21	22
T													58	77	72
U														50	52
V															72
Z															—

Diagram II. Numerical conversion of the completed dot-count for v 97 to vi 437 (see diagram I).

5.23. The next stage is to reduce these figures for each of the 16 mss. (which are the Operational Taxonomic Units or, in the technical idiom of this work, the OTUs) to a symbolic classification. This is usually done by using shading of varying degrees of intensity, as in the biologists' example reproduced in Appendix 'A' (9.1) below, but in practice I have found it simpler and no less neat in free-hand work to substitute Greek letters. Where numbers run up to a maximum of the order of 80, the following convention is suitable; it may of course be modified at will to suit the numbers involved in any particular instance.

$\alpha$ = 0-20	$\varepsilon$ = 51-60
$\beta$ = 21-30	$\zeta$ = 61-70
$\gamma$ = 31-40	$\eta$ = 71-80
$\delta$ = 41-50	$\vartheta$ = 81 or more

Thus for the section v 97 to vi 437 the conversion takes the form:

A	$\delta\delta\delta\delta\delta\delta\delta\delta$	$\varepsilon\varepsilon$	$\zeta\zeta\zeta$			
B	$\alpha\alpha$	$\delta\delta$	$\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta$	$\eta\eta$	$\vartheta\vartheta$
F	$\beta\beta$	$\delta\delta\delta\delta$	$\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta$		

G	$\alpha$	$\beta$	$\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta\zeta\zeta\zeta$	$\eta$		
H	$\alpha\alpha$	$\delta$	$\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta\zeta\zeta$	$\eta$	$\vartheta\vartheta$	
J	$\beta$	$\gamma$	$\delta$	$\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon$			
K	$\beta\beta$	$\delta$	$\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta\zeta\zeta\zeta$	$\eta$		
L	$\beta\beta$	$\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta\zeta\zeta\zeta\zeta$	$\eta$			
N	$\beta\beta$	$\delta$	$\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta$			
O	$\beta\beta$	$\delta\delta$	$\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta$			
P	$\alpha\alpha\alpha\alpha$	$\beta\beta\beta\beta\beta\beta\beta\beta\beta\beta$	$\gamma$	$\delta$	$\vartheta$		
R	$\alpha\alpha$	$\beta\beta\beta\beta\beta\beta\beta\beta\beta\beta$	$\gamma\gamma$	$\delta$	$\vartheta$		
T	$\beta\beta$	$\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta$	$\eta\eta\eta\eta\eta\eta$			
U	$\gamma\gamma$	$\delta\delta\delta\delta\delta\delta$	$\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta$			
V	$\alpha$	$\beta$	$\delta\delta$	$\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta$	$\eta\eta$	$\vartheta\vartheta$
Z	$\beta\beta$	$\delta$	$\varepsilon\varepsilon\varepsilon\varepsilon\varepsilon$	$\zeta\zeta\zeta\zeta$	$\eta\eta\eta$		

5.24. It is now a comparatively straightforward procedure to arrange these documents in a grid lay-out which will have the effect of bringing those most alike into adjacent positions. Where, as here, only 16 OTUs are involved, trial and error methods are satisfactory and refined logical processes are not necessary. Some adjustments may have to be made towards the end; thus some zetas and etas have been transposed in the grid below (diagram III) and there is an element of approximation at some points. This inexactitude is however no more than that accepted in other statistical processes, such as that of the 'best-fit line' in graphical analysis. Inspection will show (and a little trial-and-error will confirm) that once

V	B	H	T	Z	L	G	K	F	N	O	J	U	A	R	P
V	$\vartheta$	$\vartheta$	$\eta$	$\eta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\delta$	$\delta$	$\beta$	$\alpha$
B		$\vartheta$	$\eta$	$\eta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\zeta$	$\varepsilon$	$\delta$	$\delta$	$\alpha$	$\alpha$
H			$\eta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\delta$	$\varepsilon$	$\alpha$	$\alpha$
T				$\eta$	$\eta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\beta$	$\beta$
Z					$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\delta$	$\beta$	$\beta$
L						$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\beta$	$\beta$
G							$\eta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\beta$	$\alpha$
K								$\varepsilon$	$\varepsilon$	$\zeta$	$\varepsilon$	$\delta$	$\varepsilon$	$\beta$	$\beta$
F									$\varepsilon$	$\delta$	$\delta$	$\delta$	$\delta$	$\beta$	$\beta$
N											$\varepsilon$	$\varepsilon$	$\varepsilon$	$\delta$	$\beta$
O												$\varepsilon$	$\varepsilon$	$\delta$	$\beta$
J													$\varepsilon$	$\gamma$	$\beta$
U														$\zeta$	$\gamma$
A															$\delta$
R															$\vartheta$
P															—

Diagram III. Resolution of data in diagram II by rearrangement of OTUs into taxonomic sequence.



the 'immovable' OTUs have been located on the left or right hand side, there is no scope for an alternative sequence elsewhere which will not distort the rest of the grid beyond repair. Of course OTUs with identical or very nearly similar characteristics may be written in either of the two possible orders, as could B and V below. This would not affect the taxonomic relationship which we are seeking, as these two go hand in hand anyway.

5.25. One next looks at the resolved matrix (diagram III) to see if any clear-cut divisions appear which may mark off the taxa or species within the system. P and R, with their predominance of  $\alpha$  and  $\beta$  obviously form such a taxon, but one knew that already. At the other end of the scale B, H and V can be associated, with their prevalence of  $\eta$  and  $\vartheta$ . J and U come fairly close together, with A standing somewhere between them and PR. FN and O are similar in make-up, and stand some way apart from G and K. It would thus be reasonable to set up the following clusters or taxa:

VBH, TZLGK, FNO, JU, A, PR

The main line of cleavage is between GK and those to the 'left' of them and FNO and those to the 'right'. G's location seems manifest: whatever may hold good elsewhere, for this portion at least it has little or no affinity with U and U's nearest congeners, J and N.

5.31. A mathematical check is called for at this point. As the count of the section v 97 to vi 437 was for convenience conducted in two stages, it is easy to compare the near-neighbour sequence for the first (v 97 to vi 222) with that for the second (vi 223-437), and so test the consistency of the behaviour of the OTUs involved. The outcome is given below:

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
v 97 to vi 222	B	V	H	L	T	Z	K	G	F	O	N	J	U	A	P	R
vi 223-437	B	V	T	H	Z	L	G	N	K	O	J	U	F	A	R	P
Differences in placing	-	-	2	1	1	2	1	3	2	-	1	1	4	-	1	1

Inspection shows a convincing degree of correlation, notwithstanding the shortness of the second of the two test lengths (214 lines). But a numerically exact method demands a numerically exact check, disdaining subjective certification. We turn to a layman's life-line in such matters, M. J. Moroney's *Facts from Figures* (Pelican Books, 1951, reprint of 1967), chapter 18, headed 'Ranking Methods'. Though not identical, our problem is comparable with that of measuring the relative reliability of two judges judging dogs at Cruft's Show or shapely young ladies on the beaches of Blackpool or Cannes, and can be solved in the same way. We first ascertain Spearman's Rank Correlation Coefficient, which is explained for the benefit of those who admit to a degree of vagueness in such things, on p. 335 of that singularly lucid vademecum. We add up the squares of the numbers representing the differences in placing for those 12 mss. which differ at all: this

is  $4 + 1 + 1 + 4 + 1 + 9 + 4 + 1 + 1 + 16 + 1 + 1$  or 44 ( $= \sum d^2$ ). For ( $n = 16$ ) OTUs, we find by simple substitution in the formula

$$R = 1 - \frac{6 \cdot \sum d^2}{n^3 - n}$$

that  $R = +0.93$ .

Since this formula is designed so that a value for the coefficient  $R$  of  $+1$  indicates complete agreement and a value of  $-1$  the maximum of disagreement, the value arrived at here should satisfy the most sceptical. As however the number of OTUs being ranked exceeds 10, we can go a stage further and calculate for this case the value of 'Student's  $t$ '<sup>25</sup>. This tests the significance in this particular instance of the rank correlation coefficient itself. We have recourse to the formula

$$t = R \sqrt{\frac{n-2}{1-R^2}}$$

and, given  $R = +0.93$ , we find by substitution that  $t = 9.58$ . To attach a meaning to this magic number we turn to the graph on p. 230 of Moroney's book, reproduced as diagram iv below:

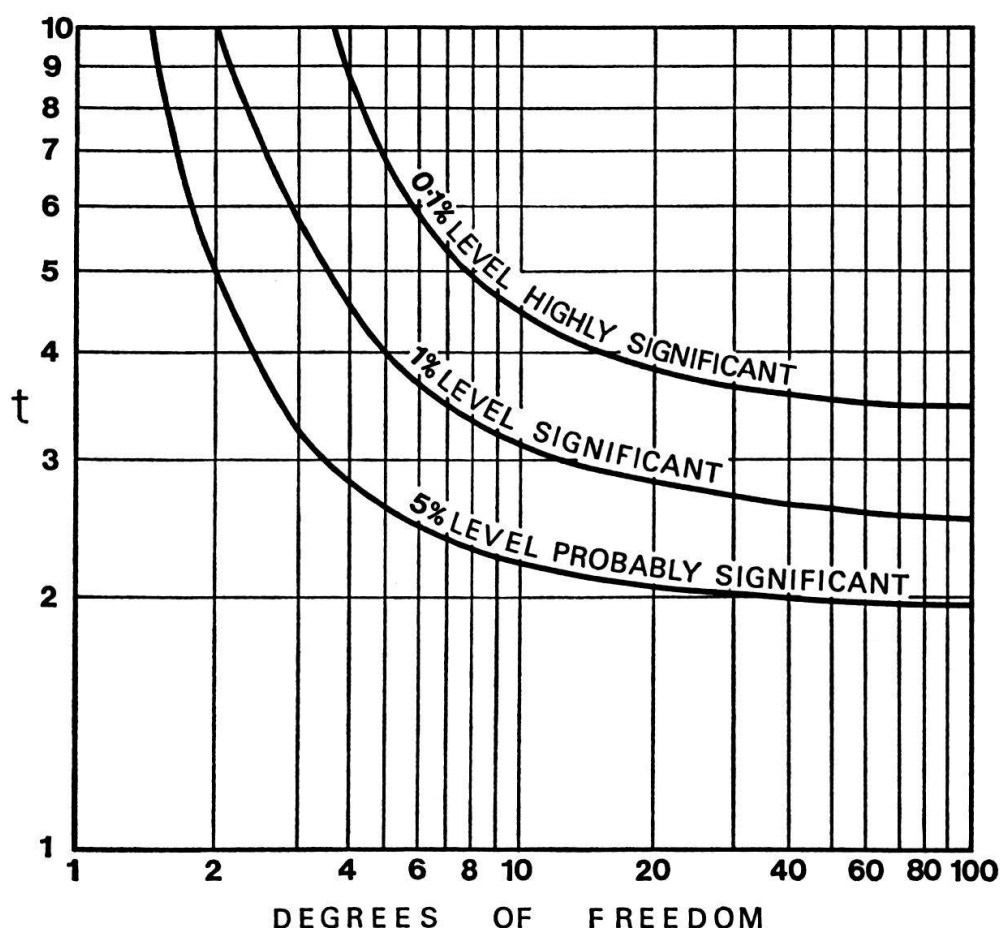


Diagram iv.

<sup>25</sup> So called from the pseudonym of its formulator, W. S. Gosset, who worked on the research staff of Guinness Brewery in Dublin (the Brewery did not, so the story runs, allow its em-

Since 16 OTUs considered in pairs means (16—2) or 14 degrees of freedom, we look for the point of intersection of the relevant crosslines, the eye running up from a point roughly between the 10 and 20 verticals to a point roughly midway between the 9 and 10 horizontals. This point is well to the right of the curve denoting the significance level of 0.1% probability, which means no more and no less than that the odds against so good a concord between the two placing-lists being fortuitous are higher than 1000 to 1. It seems to be an inescapable conclusion that the similarity matrix has yielded a satisfactorily stable result in what are quite critically testing circumstances, in so far as one of the two elements in the comparison was a shorter sample than that thought theoretically desirable.

5.32. Much tedious calculation in what follows will be avoided if at this point we work out the values for the function  $\Sigma d^2$  in relation to 14, 13 or 12 degrees of freedom (corresponding to comparisons of 16, 15 or 14 OTUs) which will assure us of a significance level of the order of 0.1% or better, i.e. the odds against chance being at work being in excess of 1000 to 1. Inspection of digram IV shows that the critical value for  $t$  is very close to 4 in these cases. The results are as follows:

For 16 OTUs (i.e. 14 degrees of freedom)  $\Sigma d^2 = 180$  ( $R = 0.725$ )

For 15 OTUs (i.e. 13 degrees of freedom)  $\Sigma d^2 = 145$  ( $R = 0.74$ )

For 14 OTUs (i.e. 12 degrees of freedom)  $\Sigma d^2 = 105$  ( $R = 0.75$ )

It might be objected that these figures are on the optimistic side, because a ms. such as P is virtually located on the near-neighbour scale before the count begins, so that we may be allowing ourselves too many degrees of freedom; this would tend to make the result look more impressive than it really is. Although in fact one is never operating in what follows with fewer than 12 degrees of freedom, I have, for interest sake, added the appropriate values of  $\Sigma d^2$  for 11 and 10 degrees of freedom, which work out at 91/11° ( $R = 0.73$ ) and 33/10° ( $R = 0.82$ ) respectively. Where there are fewer than 10 degrees of freedom the usefulness of Student's  $t$  is diminished: this however does not arise.

5.33. Once the rationale of the calculations in the previous paragraph has been grasped, it will be clear that from now on comparison of consistency can be made on the basis of the critical values for  $\Sigma d^2$ , a matter of simple addition.

5.4. We have presumptive evidence, to put it no higher, that we have forged a tool reliable enough for practical purposes and more precise than anything else so far available. What happens when it is applied to the concluding section of satire VI, from 438 to 661? It will be remembered that inspection had detected something peculiar here (4.27); does the similarity-matrix confirm this? Although this section is only about 220 lines long, it happens to be rich in variants, so that when those common to P and G alone are included 63 entries have to be made. The matrix resolves as under:

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ployees to publish research under their own names). Diagram IV is reproduced by permission from Fisher and Yates, *Statistical Tables for biological, agricultural and medical research* (Oliver & Boyd, Edinburgh/London 1963).

B	H	V	Z	T	O	K	L	A	N	J	U	F	G	P
B	$\lambda$	$\lambda$	$\kappa$	$\kappa$	$\vartheta$	$\vartheta$	$\eta$	$\eta$	$\eta$	$\eta$	$\zeta$	$\zeta$	$\alpha$	$\alpha$
H		$\kappa$	$\vartheta$	$\vartheta$	$\vartheta$	$\vartheta$	$\eta$	$\eta$	$\eta$	$\eta$	$\zeta$	$\zeta$	$\alpha$	$\alpha$
V			$\kappa$	$\vartheta$	$\vartheta$	$\vartheta$	$\vartheta$	$\eta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\alpha$	$\alpha$
Z				$\vartheta$	$\vartheta$	$\eta$	$\eta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\zeta$	$\alpha$	$\alpha$
T					$\vartheta$	$\vartheta$	$\eta$	$\eta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\alpha$	$\beta$
O						$\vartheta$	$\vartheta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\alpha$	$\alpha$
K							$\eta$	$\zeta$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\varepsilon$	$\beta$	$\alpha$
L								$\eta$	$\zeta$	$\zeta$	$\zeta$	$\varepsilon$	$\alpha$	$\alpha$
A									$\zeta$	$\eta$	$\varepsilon$	$\delta$	$\beta$	$\alpha$
N										$\eta$	$\varepsilon$	$\varepsilon$	$\alpha$	$\alpha$
J											$\eta$	$\delta$	$\beta$	$\beta$
U												$\delta$	$\beta$	$\beta$
F													$\alpha$	$\beta$
G														$\vartheta$
P														—

Key:  
 $\alpha = 0-5$   
 $\beta = 6-10$   
 $\vdots$   
 $\kappa = 40-45$   
 $\lambda = 46$  or over

Diagram v.

The comparison with the spectrum-line for v 97 to vi 437, as given in 5.23 above, is interesting, although R has to be dropped from the calculation, as it ceases to be available at vi 437.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
v 97 to vi 437	V	B	H	T	Z	L	G	K	F	N	O	J	U	A	P
vi 438-661	B	H	V	Z	T	O	K	L	A	N	J	U	F	G	P
$\Sigma d^2$	$1^2+1^2+2^2+1^2+1^2+5^2+1^2+2^2+5^2+0+1^2+1^2+4^2+7^2+0$														

This adds up to 130/13° and comes within the limits for 0.1% probability as defined in 5.32 above. The left hand portions are consistent, except in so far as O has dropped 5 places. This ms. has eccentric tendencies and, as a Beneventan document (written in South Italy and probably at Monte Cassino) may have had a different textual history from the rest. A has also come down 5 places, but in effect has only descended from the top of the JNU-complex to the bottom of it, changing places as it were with F which in vi 438-661 comes out at the top of that small group. The surprising performer however is, as had been suspected on independent grounds, G, which at about line 475 suddenly begins to exhibit unmistakable P-characteristics. Study of the matrices as resolved suggests that the taxon-divisions may be tentatively located as follows:

vi 438f. BHV, ZTOKL, ANJUF, GP

This compares with those presumed for v 97 to vi 437 thus:

v 97f. VBH, TZLGK, FNOJU, A, (R)P

6.1. I proceed to set out the spectra for the several sections of the satires, broken down into short lengths, as computed. An asterisk indicates cases where

the critical value for  $\Sigma d^2$  as worked out in 5.32 is exceeded; these invite further investigation or need explanation. Where successive stretches of the satires show low values for  $\Sigma d^2$ , an 'averaged' spectrum covering the longer length may be substituted and used in further comparisons.

6.21. Consider first I 1 to II 60, taken against III 31-316 (see 4.23)

I 1 to II 60	Z	K	V	G	T	B	H	L	A	J	U	N	O	W	R	P
III 31-316	B	V	G	L	A	H	K	T	Z	J	N	O	U	W	R	P
$\Sigma d^2$	$5^2+1+1+4^2+4^2+1+5^2+3^2+8^2+0+1+1+2^2+0+0+0$															

This adds up to 164/14° and is still within the critical limit for 0.1% probability, but much of this is contributed by Z's aberrant behaviour. If this ms. gives further evidence of instability, it will merit closer scrutiny; on the assumption that this is an isolated instance, it will be legitimate to combine these to form the following 'averaged' spectrum:

I 1 to III 316 BVH, G(Z)LKTA, NJOU, (WR)P

6.22. We next compare this with the spectrum for III 317 to v 96, which takes a rather unexpected form, though the reason is not far to seek:

III 317f.	O	B	V	K	J	Z	L	H	A	G	T	U	N(F)	W	R	P
I 1 to III 316	B	V	H	G	Z	L	K	T	A	N	J	O	U	W	R	P
$\Sigma d^2$	$1+1+5^2+6^2+1+1+3^2+3^2+0+3^2+6^2+11^2+1+0+0+0$															

Here we reach a total of \*260/14°. Although nearly half of this is contributed by O, whose instability has already attracted attention, G, J and, to a lesser extent, H share some of the responsibility. If this irregularity is sustained elsewhere, we must either re-work the count or find a sufficient explanation otherwise; failing this, it might have to be admitted that the whole process falls under suspicion. We might take refuge in the less ambitious course of accepting a lower level of significance, such as 1% (i.e. the odds against chance being no higher than 100 to 1). Plainly there is something here for future enquiry. If however this proves to be isolated, as it does, then it may be said that an occasional aberration of this kind is not unwelcome. If nothing at all of the sort were found, there might arise an uneasy suspicion of inherent oversimplification, or even doubts as to the reality of the problem in the first instance.

6.23. Normality returns when we compare I 1 to III 316 with v 97 to vi 437:

I 1f.	B	V	H	G	Z	L	K	T	A	N	J	O	U	R	P
v 97f.	V	B	H	T	Z	L	G	K	N	O	J	U	A	P	R
$\Sigma d^2$	$1^2+1^2+0+4^2+0+0+3^2+1^2+1^2+2^2+0+1^2+4^2+1^2+1^2$														

The figure of 50/13° is extremely low, and goes some way to reinforcing our faith, if that were shaken by the last calculation. It is interesting to note in passing that a comparison of the aberrant section (III 317 to v 96) with v 97 to vi 437 yields a total for  $\Sigma d^2$  (as the reader may verify for himself) of \*256/14°. This confirms the behaviour-pattern of III 317f. which we had already observed, and



again pin-points the source of the instability: O as before, abetted to a lesser degree by H, with, on this occasion T assisting.

6.3. There is no need to reproduce the count of  $\Sigma d^2$  for the comparison of v 97 to vi 437 with vi 438–661, which was (see 5.4 above)  $130/13^\circ$ . Over one-third of that was accounted for by G's shift of allegiance, previously suspected.

6.4. I pass to consider satires vii–ix, that is Book III. The spectrum-lines for vii and viii are as follows:

vii	B	V	T	H	K	O	Z	L	A	U	J	N	F	G	P
viii	V	Z	B	K	T	H	O	F	L	A	N	U	J	G	P
$\Sigma d^2$	$1^2+5^2+2^2+1^2+2^2+2^2+1^2+5^2+1^2+1^2+1^2+2^2+2^2+0+0 = 76/13^\circ$														

In ix F is defective from line 40 to the end, and so must be dropped from the comparison, which thus takes the form:

vii	B	V	T	H	K	O	Z	L	A	U	J	N	G	P
ix	K	V	H	B	Z	L	T	O	N	J	U	G	A	P
$\Sigma d^2$	$4^2+0+1^2+3^2+2^2+2^2+4^2+2^2+3^2+1^2+1^2+1^2+4^2+0 = 82/12^\circ$													

A here has jumped 4 places, which may not mean much, but scrutiny of the apparatus in this area of the satires shows something unexpected towards the end of ix:

118 *recte ... tum] recte est ... tunc est* PA (almost uniquely)

119 follows 118 in PA and Val. 410 only; elsewhere it comes after 123.

122 *non erit illis] noverit illos* PA (uniquely)

134a is only found in PA

148 *vocatur* PA: *rogatur* rell.

149 *adfixit* PA: *adfigit* rell.

There is also a gloss on 106 which is the peculiar property of these two mss. The first foreshadowing of this liaison seems to be at 68, where the correct *puerorum* is unique to them, having been everywhere else ousted by the gloss *servorum*, while the following word *aquilone* has been similarly displaced by the colourless *mense* in all mss. except PA, GJU. It looks as if A is behaving somewhat in the manner of G at the end of vi, though not so spectacularly. For the moment we suspend judgment, awaiting further evidence of its subsequent conduct.

For completeness here is the comparison of viii and ix:

viii	V	Z	B	K	T	H	O	L	A	N	U	J	G	P	(F omitted)
ix	K	V	H	B	Z	L	T	O	N	J	U	G	A	P	
$\Sigma d^2$	$3^2+1^2+3^2+1^2+3^2+2^2+2^2+1^2+1^2+2^2+0+1^2+4^2+0 = 60/12^\circ$														

While therefore we have picked up one possibly interesting deviation from normalcy in the matter of A's behaviour and have to do a piece of common-sense 'averaging' to estimate F's position in view of its absence from the count in ix, we shall stay within the acceptable limits of accuracy if we use for further comparison the following 'averaged' ranking list to cover the whole of Book III:

vii-ix    V   B   K   H   Z   T   O   L   F   A   N   J   U   G   P

6.5. Satire x, though part of Book IV, is conveniently treated on its own. It is the last satire for which we have the evidence of J; this ms. carries only a debased fifteenth-century text of the six concluding satires. The spectrum is

x        L   B   V   Z   F   H   O   K   T   U   A   N   J   G   P

When set against that for vii-ix (6.4 above), it appears thus:

vii-ix    V   B   K   H   Z   T   O   L   F   A   N   J   U   G   P

$\Sigma d^2$          $2^2+0+5^2+2^2+1+3^2+0+7^2+4^2+1+1+1+3^2+0+0$

This gives a total of 120/13°. When we come to compare the spectrum of x with that for xi-xiii (see 6.62 below) we shall find that  $\Sigma d^2$  works out at 122/12°.

6.61. The spectra for xi, xii and xiii come out with a remarkable regularity. To some extent this may be due to good luck, as xii is short, though fairly interesting textually. For this section the readings of Val. 410 have been incorporated, and it behaves in a consistent manner, sufficient, it would seem, to justify further scrutiny at the next stage of enquiry. The respective spectra are:

xi        H   B   V   O   L   Z   K   T   Val F   N   A   U   G   P

xii       B   V   H   O   T   L   K   Val Z   N   F   U   G   A   P

xiii       H   B   V   T   L   K   O   Val Z   N   A   F   G   U   P

The overall homogeneity will strike the eye, but for completeness I gave the figures for  $\Sigma d^2$  in each of the three comparisons:

(a) for xi against xii     $\Sigma d^2 = 34/13^\circ$

(b) for xi against xiii    $\Sigma d^2 = 44/13^\circ$

(c) for xii against xiii    $\Sigma d^2 = 32/13^\circ$

6.62. For so satisfyingly homogeneous a section the combined spectrum may be confidently taken to be:

H   B   V   T   L   O   K   Z   (Val) N   F   A   U   G   P

6.71. No particular problems arise in plotting the spectra of xiv and xv-xvi (which should obviously be taken together for this purpose). These come out as under:

xiv        B   H   L   K   Z   V   O   G   F   T   N   A   U   P

xv/xvi    V   B   H   K   Z   L   G   F   T   N   O   A   U   P

$\Sigma d^2$          $5^2+1^2+1^2+0+0+3^2+1^2+1^2+1^2+1^2+4^2+0+0+0$

This produces the low total of 56/12° and enables us with confidence to substitute the averaged spectrum for this section as under

xiv-xvi   B   H   V   K   L   Z   G   F   O   T   N   A   U   P

6.72. This last bears a superficial resemblance to some earlier spectra. We had previously noted that from xiv to the end the supply of distinctive readings shared by G with U and P dries up, and in consequence this manuscript reverts to its earlier undistinguished level.

6.73. It is interesting to look back at this point and compare the last spectrum to be derived (xiv-xvi 60) with those for the earlier sections. Expressed in terms of  $\Sigma d^2$  the relations may be tabulated as under:

(a) XIV-XVI 60 against I 1 to III 31	$\Sigma d^2 = 36/11^\circ$
(b) XIV-XVI 60 against III 32-316	$\Sigma d^2 = 98/11^\circ$
(c) XIV-XVI 60 against III 317 to v 96	$\Sigma d^2 = *142/12^\circ$
(d) XIV-XVI 60 against v 97 to vi 437	$\Sigma d^2 = 68/12^\circ$
(e) XIV-XVI 60 against vi 438-661	$\Sigma d^2 = *122/12^\circ$
(f) XIV-XVI 60 against VII-IX	$\Sigma d^2 = 82/12^\circ$
(g) XIV-XVI 60 against x	$\Sigma d^2 = *114/12^\circ$
(h) XIV-XVI 60 against XI-XIII	$\Sigma d^2 = *106/12^\circ$

Thus the impression already forming in one's mind about the reversion of some of the less stable mss. in XIV-XVI to their behaviour in the earlier portions receives arithmetic confirmation. The asterisked figures, indicating a probability-factor of 'worse' than 0.1% (1000 to 1 against) occur just where they ought to occur, in view of what was to be expected from the observed conduct of G, O and one or two others.

6.81. For convenience of reference and to present the outcome synoptically, the several spectrum-lines for the 8 sections (4.23 above) into which it proved easiest to divide the work are set out below:

I 1 to III 316	-	B	V	H	G	Z	L	K	T	A	N	J	O	U	(WR)	P
III 317 to v 96	O	B	V	K	J	Z	L	H	A	G	T	U	N	F	(WR)	P
v 97 to vi 437	B	V	H	T	Z	L	G	K	F	N	O	J	U	A	(R)	P
vi 438-661	B	H	V	Z	T	O	K	L	A	N	J	U	F	G		P
VII-IX	V	B	K	Z	H	T	O	L	F	A	N	J	U	G		P
x	L	B	V	Z	F	H	O	K	T	U	A	N	J	G		P
XI-XIII	-	H	B	V	T	L	O	K	Z	N	F	A	U	G		P
XIV-XVI 60	-	B	H	V	K	L	Z	G	F	O	T	N	A	U		P

6.82. Finally a comparison of the values for the function  $\Sigma d^2$  for all possible combinations of the eight sections; this is most plain when presented in tabular form (see p. 132).

Although there are a number of asterisked entries in the table below, it is noteworthy that only in the three cases in italics is the value of  $\Sigma d^2$  such as to bring the probability level down to the 1% line. Of the 16 asterisked entries no less than 12 are brought about by two stretches of the satires, III 317 to v 96, which has already attracted attention (6.22), or satire x. This observation offers an obvious starting-point for further enquiry. Of the remaining 4 cases, some are only slightly above the critical values for the appropriate degrees of freedom. If satire x is left aside for the moment, it is remarkable that from v 97 on the degree of internal consistency is high: this raises a suspicion that the first book of satires, which include the justly popular Third, and the equally famous Tenth may have attracted

interpolation to a greater extent than the rest. While a recount of some sections may yield a slightly different answer, in the main it would appear that so satisfactory a result has justified the not inconsiderable labour involved.

	III 317 to v 96	v 97 to vi 437	vi 438 to 661	vii-ix	x	xi-xiii	xiv-xvi
i 1 to							
iii 316	*260/14°	50/13°	*134/12°	*138/12°	*166/12°	*115/11°	36/11°
iii 317							
to v 96		*314/13°	*174/13°	*158/13°	*270/13°	*128/12°	*142/12°
v 97 to							
vi 437			130/13°	122/13°	*166/13°	84/12°	68/12°
vi 438-661				52/13°	*164/13°	38/12°	*122/12°
vii-ix					116/13°	72/12°	82/12°
x						*122/12°	*114/12°
xi-xiii							*106/12°

### 7.1. *Implications for an editor*

Opinion may differ on the implications of the foregoing study for a future edition of Juvenal's text, which need to be stated with circumspection at this stage. The following, *inter alia*, suggest themselves:

#### 7.11. Selection of mss. for citation.

(a) N (Vat. Reg. 2029) and, for i 1 to x 366 only, J (Vat. 3286) should be cited regularly. Further scrutiny may perhaps justify the same claim for Val. 410.

(b) B, H and V can be dropped with an easy conscience. Their performance is consistently opposed to that of the sincerer witnesses, and not only are they heavily interpolated, but appear to contain little or nothing that may reflect ancient tradition.

I take the view that the return on the labour of further full-scale collation is unlikely to be justified. The use of reliable sampling techniques to associate promising witnesses, as yet imperfectly collated or unknown, with well-established taxa is obviously a matter that will have to be faced at the next stage. It is obviously impracticable to re-calculate similarity-matrices *de novo* to take account of new material as it arises; fortunately other techniques (such as 'split matching-scores' and the like) may enable the classification to be achieved with an acceptable economy of effort.

7.12. Grouping of mss. – Nothing fresh is added to our knowledge of P and its congeners, but they have served a useful purpose in this study as a datum-line at one end of the near-neighbour spectrum. Knoche's *I*-group however is seen to have only a limited validity, and to have been in the event unfortunately

named, in that G ( $\gamma$ ) is its least constant member. It might well be redesignated the Y-taxon, to associate it with U, its staunchest adherent. If P had not survived, this taxon could have led to a tolerable text, though there would have been more scope for editorial imagination and ingenuity. The chief representatives of Knoche's  $\Psi$ -group, BHV, can be discarded. His  $\Lambda$ -group is somewhat shapeless: F and Z converge at times, but hardly for long enough for their association to acquire useful meaning. Rather more promising perhaps is the proximity of K and L. Throughout xi–xvi they are never widely separated: they come next-door to each other in vi 438–661 and only G stands between them in v 97 to vi 437. Thus it may not be without significance that they both report the Nicaeus-subscriptio where they do (at the start of vi in K and in the left-hand margin opposite vii 4 in L). Otherwise their behaviour is a little too erratic to inspire confidence. However *linigero* in K at vi 533 may perhaps be more than a lucky guess or felicitous miswriting, for it occurs in a section where K comes as close to the right-hand end of the spectrum as it ever does.

7.13. Presentation of the near-neighbour relationship. – The shifts in this relationship might best be represented by printing the spectrum-line appropriate for each satire (or portion thereof) at the beginning of the apparatus relating to it, immediately above the entries of variants. If not thought unsightly, the spectrum-line might be repeated, for ease of reference, at the same place on each page. This would show at a glance the relative value of the 'unstable' witnessess in the particular context. One's evaluation of an unusual reading found in some of these mss. would no longer be based, as it tends to be at present, on a rough-and-ready 'count of heads', however subconscious this may be, but on something nearer to qualitative judgment taking account of the behaviour of the ms. or mss. concerned in that vicinity.

7.14. Confidence in some isolated readings cropping up in unexpected mss. may, in certain circumstances, be increased. Where idiosyncratic but attractive readings are found in mss. which either generally or over limited stretches of the text sort consistently with those regularly occupying the right-hand section of the spectrum-line, one may well be rather more disposed to think that these reflect genuine tradition than one might have felt otherwise. Thus at vi 167 the reading *Venustinam* found in N (and, incidentally, in Val. 410) gains, I think, in probability somewhat, when account is taken of the general behaviour of N and, it may well prove, of Val. 410 too. So O's contribution towards the truth at xv 75 might be taken for a freak of chance or a lucky emendation made long ago, were it not that in xv–xvi O comes out well towards the right-hand end of the spectrum, between N and A and close to U and P. There is no need to lengthen the list of these things, which might seem to smack of advocacy rather than argument.

7.15. Whether there may be other uses for this method, such as the extraction of clues to the provenance of mss., I do not know. I would personally deprecate



the overworking of a promising technique which may be efficient for one job but inappropriate to another. This is not in any way to discourage the search for other approaches which may resolve worth-while problems; rather I welcome the use of any reputable technique which may successfully transcend the arbitrary frontiers of disciplines.

7.2. One question has been deliberately left undiscussed. Has the taxonomic process effectively disentangled the underlying relations of the interpolated mss. and penetrated to the pattern of their 'genetic histories' latent beneath the overburden of interpolation they now present to our observation? The degree of regularity of their behaviour as disclosed in this study makes this at least a tenable supposition. It has been put to me that the position should be stated more strongly: that it is incumbent on those who are sceptical of the method or of its results in Juvenal's case to indicate the kind of explanation which in their view would cover both the superficial disarray of the data and the underlying regularity revealed by the analysis, certified as this is by rigorous and accepted criteria of statistical probability.

8.1. At this point I call off the hunt. Assuming that the strategy of this approach and its results survive criticism, there is still room for debate about tactics: there may be less laborious ways of processing the information or clearer means of presenting the conclusions. There is also the unresolved question of weighting factors, which are sometimes thought inappropriate to the logic of numerical taxonomy, though there is dissent on this matter. In this study they have been left aside, deliberately, except in so far as the omission or inclusion of a piece of information is in itself a form of weighting activity. Meanwhile *libelle, perge provocare crabrones*<sup>26</sup>.

---

<sup>26</sup> I had completed this study before I knew of the interesting arithmetical analysis by P. Canivet and P. Malvaux of 15 selected mss. of the *Περὶ τῆς θελας ἀγάπης* of Theodoretus Cyrensis (Migne, PG 82, 1497–1521) in *Byzantion* 34 (1964) 385ff. (I owe this reference to Mr. N. G. Wilson). This short homily is rich in variants, from which Canivet and Malvaux draw up a divergence-count, which is plotted onto a matrix (p. 395). Hence well-marked families of mss. are deduced and represented on a stemma of conventional form (p. 403). Their method bears an obvious resemblance to mine, but is applied to material that is only marginally polythetic, in so far as little more than one-third of the entries show alignments which cut across the established boundaries of families (206 out of 585; the use of weighting factors probably accounts for the high totals). This is in harmony with Canivet's concluding remarks (p. 413) "... la tradition de la *Charité* soit plus cohérente que celle de la *Philothée* [the so-called *Historia Religiosa*, which precedes the *Theia Agape* in most of the mss.] et surtout ne comporte pas les contaminations qui demeurent possibles dans les recits de la *Philothée*."

Although in places Canivet comes within sight of the near-neighbour concept (e.g. in his remarks on his F, p. 401), he speaks throughout in stemmatic language ("chefs de file", "apparenté" and so on) as befits the predominantly monothetic nature of his material, so that discussion of contamination and the effects of variants does not arise. I am glad however to record my interest in this earlier study, which is not diminished by the realization that its points of contact with my own amounted to rather less on closer inspection than I had expected on first reading.

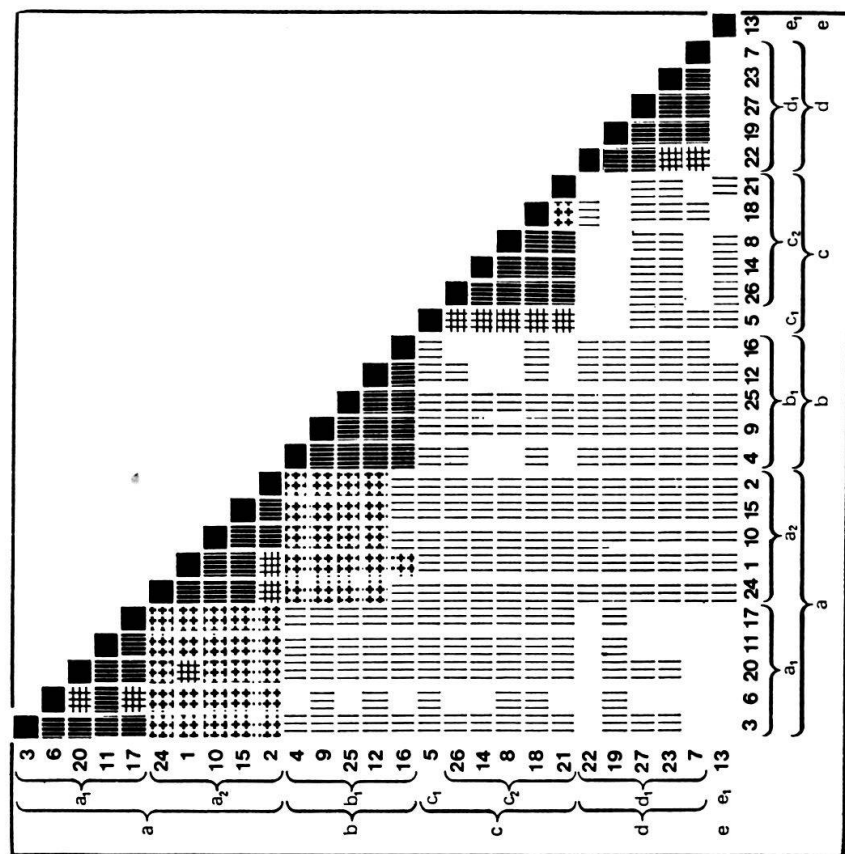
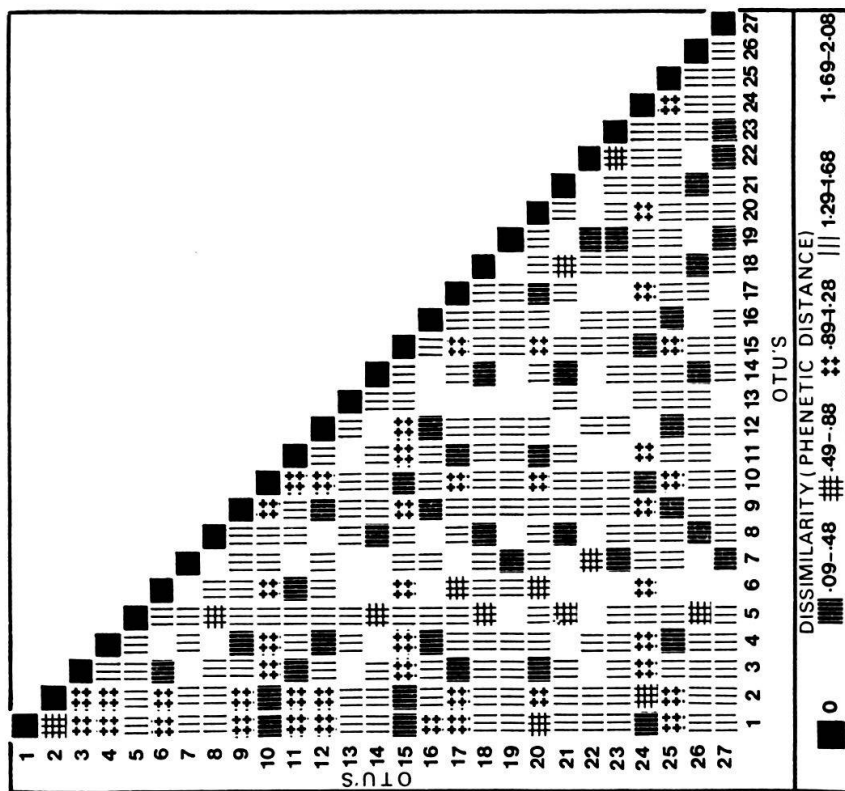


Diagram VI.

Reproduction of similarity-matrices establishing groups or "taxa" for 27 specimens of nematode worms, whose characteristics form a "polythetic" array and as such cannot be classified by traditional or "family tree" methods (taken, by permission, from the Scientific American, December 1966, p. 110).

*Appendix 'B': the Protean behaviour of G (Paris. 7900a)*

9.2 (cf. 5.4). It remains to document the shifting allegiances of this ms. As far back as 1910 Leo observed a link between G and U, but had neither space nor need in the introduction to his text to enumerate the distinctive common features. Out of a list of 60 singular readings in G and U compiled from Knoche's apparatus (see too his *Grundlagen* 142 n. 3) about three quarters are peculiar to them<sup>27</sup>. Among the more notable are:

- |      |     |  |
|------|-----|--|
| VII  | 134 | <i>spondet]</i> <i>splendet</i> GU   |
|      | 153 | <i>isdem]</i> <i>idem</i> GU (see 2.642)                                   |
|      | 163 | <i>fulmina]</i> <i>fulgura</i> GU  |
|      | 235 | <i>Anchemoli</i> N, Servius: <i>Anchemori</i> GU, K: <i>Archemori</i> rel. |
| VIII | 51  | <i>hinc</i> GU, N: <i>hic</i> rel.   |
|      | 114 | <i>quid]</i> <i>namquid</i> GU   |
|      | 179 | <i>Pontice]</i> <i>Regule</i> GU   |
|      | 237 | <i>modo]</i> <i>bene</i> GU  |
| IX   | 14  | repeated after 11 in GU, with variant <i>calida</i> for <i>calidi</i> rel. |
|      | 53  | <i>munera]</i> <i>numera</i> GU  |
|      | 96  | <i>qui]</i> <i>cui</i> GU  |
| X    | 198 | <i>membra]</i> <i>labra</i> GU, J  |
|      | 199 | <i>et iam leve caput]</i> <i>ut leve omne caput</i> GU                     |
|      | 358 | <i>ponat]</i> <i>ducat</i> GU, J   |
| XI   | 12  | <i>miserrimus]</i> <i>novissimus</i> GU                                    |
|      | 38  | <i>crumina]</i> <i>culina</i> GU, F  |
|      | 41  | <i>pecorum]</i> <i>pecoris</i> GU (unmetrical)                             |
|      | 67  | <i>humilis]</i> <i>viridis</i> GU  |
|      | 100 | <i>mirari]</i> <i>imitari</i> GU   |
| XII  | 71  | <i>praelata]</i> <i>prolata</i> GU   |
|      | 81  | <i>stagna]</i> <i>stacta</i> GU  |
|      | 86  | <i>praestat]</i> <i>restat</i> GU  |
|      | 104 | <i>concipitur]</i> <i>conspicitur</i> GU                                   |
| XIII | 64  | <i>egregium]</i> <i>egregiumque</i> GU                                     |
|      | 119 | <i>statuamque]</i> <i>statumque</i> GU                                     |
|      | 142 | <i>pulli]</i> <i>populi</i> GU   |

Confronted with this sample, one would need to think twice before disputing Knoche's observation (p. 144) "the yield of variants peculiar to G and U which are limited to satires 7–13 is large, nevertheless close affinity persists in 1–6 and 14 (doch so daß nähere Verwandtschaft auch in den Satiren 1–6 und 14 weiterbesteht). Matters are harder in 15–16". In regard to xv and xvi there is indeed a

<sup>27</sup> Some have to be cancelled, because inaccurately reported, as e.g. VII 81 (*enim* G: *erit* U rel.).

difficulty in that G, in common with a large group of heavily interpolated mss.<sup>28</sup> (Knoche's *E*-class) transposes the unfinished xvi with xv, though in other ways it has little enough in the way of *E*-features. Leaving this aside, it will not have escaped notice that the taxonomic survey has already disposed of any link between G and U in i-vi and xiv. Search discloses only a handful of readings in i-vi which are common to G and U and in every case they are accompanied by other witnesses as e.g. at ii 117, where for the correct *quadringenta* GU and ALO, as well as JN, all offer *quadraginta*<sup>29</sup>. In vain one looks for a case of significant agreement in xiv unique to GU. On the other hand there are over 30 places in this piece where they are sharply at variance, as at xiv 30 (*moechum* PU: *-chos* G *rell.*), 115 (*acquirendi* Pac Uac: *atque verendi* G *rell.*), 239 (*quantum* G *rell.*: *-to* U), 287 (*lacernis* P, UN: *lacertis* G *rell.*). Others, some no less distinctive, could be added, *sed quousque haec?*

Thus the testimony of figures and the evidence from inspection of instances conspire to determine the very limited area where the GU-link is valid, and to show the different relationship which obtains outside satires vii-xiii. This does not however dispose of the matter. We have seen that from i 1 to a point in the vicinity of vi 475 G has nothing of interest to offer. From that point to the end of the poem there are about 40 cases of agreement between P and G, 20 of which are the peculiar property of these two, as appears from the list below:

- vi 479 *flagello* PS G<sup>pc</sup>: *-is* *rell.*
- 486 *domus* PG: *-o* *rell.*
- 511 *gravis est rationibus*] *gravi rationibus* PG
- 528 *portabit*] *potabit* PG
- 539 *lacrimae* Pac G<sup>ac</sup>, O: *lacrimas* U: *-is* *rell.*
- 541 *Osiris*] *orisis* PG
- 542 *faenoque*] *phanoque* P G<sup>ac</sup>
- 546 *manum set*] *manus et* Pac G<sup>ac</sup>
- 571 *dentur tempora lucro*] *dantur temporalia lucra* PG
- 582 *utrimque* PG<sup>ac</sup>: *utrumque* *rell.*
- 584 *poppisma* PG: *popisma* *rell.*
- 585 *dabunt* PG: *dabit* ATU: *feret* *rell.*
- 606 *omni* Pac G<sup>ac</sup>, F: *omnes* *rell.*
- 612 *desipis*] *desidis* Pac G<sup>ac</sup>
- 626 *tanti una* P G<sup>ac</sup>: *quantum (-tum) una* *rell.*
- 632-3 *om.* Pac G<sup>ac</sup>
- 638 *vani set*] *vanis et* P G<sup>ac</sup>

<sup>28</sup> This numerous but unhelpful class, possibly compiled for school use, has been excluded from this enquiry. It would not, I think, have affected the argument and would certainly have complicated it.

<sup>29</sup> The list comprises i 68; ii 24 (but this should be withdrawn because of homoearchon), 107. 117 (above); iii 67. 120. 166. 259; iv 117; v 74; vi 196. 322. 490 and elsewhere.

- 648 *impendente]* *impendere* Pac G<sup>ac</sup>  
 652 *fata]* *et fata* (-*ma* G<sup>ac</sup>) Pac G<sup>pc</sup>  
 660 *praegustabit* PS G: -*vit* U: -*ret* rel.

In addition at 486 (*praef-*), 490, 491 and 494, 527 (see 3.23 above), 541, 551, 553, 558–59, 560, 563, 565, 569, 592, 604, 647 and 648 (*rabiem*) there are lections shared by P and G with others.

Closely as P and G are associated in this section by these common features, there are sufficient divergencies to exclude the possibility that G is here offering readings derived direct from P (which would be possible chronologically). It is enough to consider vi 504 (*cedo* G rel.: *credo* P), 518 (*nisi* om. P), 569 (*haec* G rel.: *nec* Pac S, A), 563 (*mitti* G rel.: -*it* P).

Although the junction of G with the P-stream can be located with precision around line 475, it is indeed hardly to be expected that the point where G margs with U should be similarly well-marked. This has certainly happened by vii 134, but for the early lines of vii G and P keep company, as at 2, 3, 4, 20 and, most obviously, 40 (*maculonis* Pac G<sup>ac</sup>), but only sporadically thereafter, as perhaps at 73 (*alveolos* PCG, F: *albiolos* rel.), 99 and (in common with NFH) at 105. After the break-up of the partnership of G with U there might seem to be an isolated case of P and G making common cause at xiv 152 (*foede* PG: -*dae* rel.), but this is probably a fluke of spelling, as for the previous 150 lines G has reverted to the erratic behaviour it had shown in i 1 to vi 475. More can hardly be said, but this probably suffices to document the shifting allegiances of G in so far as these concern this discussion.