

πρῶτόν τε καὶ ὕστατον αἶέν ἀείδειν
*Relative chronology and the literary history of the early
 Greek epos*

Richard Janko

The relative chronology of early Greek epic poetry is an essential question for anyone who wishes to understand the literature, society, history, religion and mythology of early Hellas. Yet the dating, both relative and absolute, of the poems themselves is a field where chaos and confusion reign. With the exception of Martin West, who famously stated that Hesiod's *Theogony* 'may well be the oldest Greek poem we have' (1966: 46), until about 1980 most scholars believed that the Homeric poems were composed later in the eighth century BC, and the Hesiodic poems a generation or two later. The belief that the poems of Homer are older than Hesiod's concurred with the views of Eratosthenes¹ and Aristarchus, who marshalled much evidence, principally from geography, that Hesiod was later than Homer, since Hesiod knew of more remote places in the Mediterranean and named them more accurately, e.g. the Nile. Aristarchus argued this in a lost monograph entitled *Περὶ ἡλικίας Ἡσιόδου*, 'On the age of Hesiod'.² Now, however, many scholars who have made important contributions to our understanding of different aspects of the early Greek epic tradition either profess uncertainty about Homer's date or argue for dates in the seventh or even the sixth centuries BC.

I am most grateful to Øivind Andersen and Dag Haug for their invitation to contribute a paper on this topic, and to them and the other members of the audience for lively and stimulating discussion.

¹ Reported by Strabo, 1.2.14, 1.2.22.

² Cf. e.g. schol. A (Aristonicus) ad *Il.* 10.431a Erbse, ὅτι Ὅμηρος οὐκ οἶδεν καλουμένους Λυδούς, ἀλλὰ Μήοντας. πρὸς τὰ Ἐπιπέρι ἡλικίας Ἡσιόδου'. The existence of this title was first recognized by Schroeder (2007); for the form of the cross-reference cf. schol. A (Aristonicus) ad *Il.* 13.197, ὅτι συνεχῶς κέχρηται τοῖς δυικοῖς. ἡ δὲ ἀναφορά πρὸς τὰ Ἐπιπέρι τῆς πατρίδος'. Ἀθηναίων γὰρ ἴδιον. In his lost work *On the Homeland* (sc. of Homer) Aristarchus argued that Homer was an Athenian (Janko 1992: 25, 71). It is symptomatic of the present confusion that the new Loeb edition of Hesiod (Most 2006) omits from the *Testimonia* the opinions on Hesiod's date of two of the best scholars in antiquity. M. L. West mentioned neither these scholia nor Aristarchus when he so influentially argued that Hesiod antedates Homer; his silence is curious, since he refers to Aristarchus' arguments and conclusion elsewhere (1966: 40–8, cf. 260–1).

Thus I broach this topic with some trepidation. This is not because I do not know what I believe about it; indeed, that belief has been further strengthened over time. In 1982 I published my doctoral thesis under the title *Homer, Hesiod and the Hymns: Diachronic Development in Epic Diction*. This title reflects what I believe that I proved in that book, namely that the poems of Hesiod post-date the Homeric epics, and that the long *Homeric Hymns* are mostly later still. The sole exception to this is the *Hymn to Aphrodite*, where I then accepted arguments from the detection of *exemplum* and *imitatio*, which I felt dated the poem after the *Theogony* (Janko 1982: 25, 225–8, 200 fig. 4), rather than my linguistic methodology, which strongly suggested a date soon after that of the *Iliad* (Janko 1982: 151–80, 74 fig. 3). I now think I ought to have had more faith in my own method, and prefer to date the *Hymn to Aphrodite* to Homer's time, with its oddities explained by the influence of the original Aeolic tradition on which it also drew.³ My primary focus throughout was on the relative chronology of the poems in the corpus, and not on their absolute date, although I did offer some very tentative suggestions about absolute dates.

At the time, my conclusion that Homer's poems are earlier than Hesiod's did not seem much of a revelation, since, with the notable exception of West's view, it matched the scholarly *communis opinio*. What was innovative was that I had argued for this chronology from statistics, rather than from the relatively impressionistic methods that had been used hitherto (though the conclusions of Eratosthenes and Aristarchus were better founded than that). I based my conclusions on a study of eleven linguistic changes that are very common in the corpus of early hexameter poetry.

This was not a completely new approach. In the nineteenth century several German doctoral theses had tried to quantify one linguistic criterion, the observance or neglect of the effect of digamma. In the 1960s Hoekstra (1965, 1969) had revealed how the modification of traditional formulae was instrumental in introducing recent and indeed post-Homeric forms into the *Homeric Hymns*, although he did not quantify his results. Garbrah

³ My reason for believing that the *Hymn* postdates the *Theogony* was that *Hym. Ven.* 261, [νύμφαι] καλὸν χορὸν ἐρρῶσαντο, seemed to imitate the proem to the *Theogony*, [Μοῦσαι] χοροῦς ἐνεποιήσαντο | καλοῦς, . . . ἐπερρῶσαντο δὲ ποσσίν (*Th.* 7–8). However, the case of the *Hymn to Hermes*, where the opening verses survive in two versions with oral variants (Janko 1982: 3), shows that short passages comprising the beginnings of Homeric Hymns were so traditional that they could have a relatively fixed form over extended periods of time: cf. *Il.* 16.179–92, where Homer draws on an earlier version of the proem to the *Hymn to Hermes* (Janko 1992: 342). Once the *Hymn to Aphrodite* is correctly placed just after the *Iliad*, my diagram of the relations between the poems (Janko 1982: 200 fig. 4) concurs to a remarkable degree with the results of the fine cluster-analysis which Brandtly Jones presents in this volume.

Brandtly Jones

presented in Oslo
but regrettably has
not included in
Chapter 2 of

this volume.

(1969) had undertaken a statistical analysis of the language of portions of the *Odyssey*. Still more significantly, Edwards (1971) had quantified frequently occurring aspects of Hesiodic diction, above all the usage of o- and a-stem accusative plurals, in order to explain the peculiar a-stem short-vowel accusative plurals that sometimes occur in his poems. But nobody had tried to correlate the behaviour of a number of different linguistic indices in order to see whether any consistent pattern emerged.

The work was at first undertaken by hand, with months of mind-numbing counting. I was encouraged to persevere by the late Sir Denys Page, to whom I had sent my early ideas (Professor Page did not live to see that my results disprove his analytic theories, which I had set out to try to support). However, before long, with the help of Dr John Chadwick, the Reverend A. Q. Morton and Dr John Dawson of the Literary and Linguistic Computing Centre at Cambridge, I was able to use computerized texts and key-word-in-context concordances of the early epos, at a time when computer-files still consisted of stacks of punch-cards and reels of paper tape.

I well recall how, after all the counting had been done, in 1977 I stayed up one March night until three o'clock in the morning, while the wind howled outside my garret, in order to see whether any pattern emerged from all the results. To my excited eyes, as I plotted the first simple graph by hand, it seemed obvious that there was indeed such a pattern, which made me feel that all the effort had been worthwhile.

Statistical analysis is a method which, if properly applied and understood, affords a basis for a kind of objectivity that seems to belong more to the natural and social sciences than to the humanities. Accordingly, I felt that the reliability of my conclusions went beyond the intuitions of previous researchers. The conclusions to which statistical analysis leads are simply numerical measurements of probability. At first sight, these differ in nature from the results of philological arguments. However, experience leads me to conclude that this is a false dichotomy, because the conclusions to which we come in philological or historical inquiry are very often probabilistic also. Even in philology, our conclusions are only provisional; we must always be prepared to abandon them if a new hypothesis better explains the evidence. A statistical conclusion too is probabilistic, but with the difference that we can state with some degree of precision how likely it is that the alternative interpretation of the same data (what statisticians call the 'null hypothesis') is in fact correct. It seemed then, and still seems to me now, that the statistical odds that these phenomena in the language of the early Greek epic are random are almost infinitesimally small.

Against this background, for the benefit of those who have not absorbed the argument of *Homer, Hesiod and the Hymns*, I shall try to summarize it in a way which I hope will easily be understood; I shall also integrate into the argument some fresh observations that I have made during the intervening decades. Let me first set out the four presuppositions of my method.

First, the language of Greek epic is a *Kunstsprache* made up of elements of different dates, some of them very old; indeed, a few demonstrably go back to the middle of the second millennium BC.⁴ This *Kunstsprache* is more complex than that of any other oral poetry that we know, probably because of the demands of the hexameter, which is more fixed in its form than are the verse-patterns of other traditions, like the South Slavic *deseterac*. However, even South Slavic epic has metrically convenient oddities of *Kunstsprache* like the interchange of the vocative and the nominative. Just as the Greek bards use νεφεληγερέτα Ζεὺς or ἵππότα Νέστωρ as nominatives, when these adjectives were originally vocatives, so too the singers readily replace the nominative form of a name with a vocative when they need to fill an extra syllable; thus when Avdo Međedović wanted to say ‘Vuk asks’ in four syllables, he would sing *pita Vuče* (Međedović 1974b: 120 l. 2311). Once oral poets master their special epic language, it becomes a language like any other. The fact that it is a *Kunstsprache* does not imply that it will not change; on the contrary, so long as it is part of an oral tradition, its morphology changes over time, as older forms are replaced by younger ones.⁵ This process of morphological replacement occurs more rapidly outside fixed formulae and more slowly within them, since formulae play an essential role in composition and may be retained for a long time even after they become unmetrical. While the *Kunstsprache* is being used by oral bards for composition-in-performance, it will change along with changes in their own spoken language. To quote Milman Parry, to whom this crucial point is owed:

The language of oral poetry changes as a whole neither faster nor slower than the spoken language, but in its parts it changes readily where no loss of formulas is

⁴ Several linguists have recently denied this (Hackstein 2002: 5–16; Haug 2002: 39–68; Hajnal 2003: 80–93). However, those who hold this view seem to neglect the archaeological evidence for objects that go back to the Early Mycenaean period, like the body-shields that inspired some of the phrases which once had syllabic *r* like σάκος ἀμφίβροτον. Of course, such phrases could have been developed in Central Greece (where a mixture of Mycenaean and West Greek was probably spoken) rather than Southern Greece, so the ‘Mycenaean’ phase of the epic tradition may be more chronological than geographical in significance.

⁵ As Haug (this vol.) shows, the rate of replacement in syntactical features like tmesis is likely to have differed from that of morphological features; but this does not affect my argument.

called for, belatedly when there must be such a loss, so that the traditional diction has in it words and forms of everyday use side by side with others that belong to earlier stages of the language. (in A. M. Parry 1971b: 333)

This admirable insight was long overshadowed by Parry's other claim that all or most epic verse consists of formulae, which would of course imply that their language hardly changes over time. We owe the rebuttal of this latter assumption to Hoekstra (1965) and Hainsworth (1968) in particular.

My second presupposition was that the decipherment of Linear B by Ventris and Chadwick and the reconstructions of historical linguistics, many of them decisively confirmed by the decipherment, are in their outlines largely accurate and reliable, so that we can say that, for instance, phrases with the digamma preserved are older than those which presuppose its loss. Indeed, the method can sometimes be used to confirm those results. For example, when statistics show that the contraction of $\epsilon\upsilon$ to $\epsilon\bar{\upsilon}$ spreads through the epic as the poems become later, this helps to confirm that $\epsilon\upsilon\varsigma$ 'good' is derived from Indo-European $*\epsilon_1s\upsilon\varsigma$ and is cognate with the Hittite adjective $as\check{s}u\check{s}$ of the same meaning.⁶

My third presupposition was that, in order to obtain statistically dependable results, it is in most cases useless to study elements of the language that are rare, like infrequent linguistic forms⁷ or isolated items of vocabulary; the occurrence of the latter depends too much on the vagaries of content, and one cannot usually know against which other words they must be measured. Instead, one must study common features, like phonetic changes and morphological endings. Fortunately I was able to identify more than ten of these (and a few more since) that are so frequent that they yield statistically sound samples in all but the shortest poems in the corpus. Each statistic is based on counting dozens or even hundreds of occurrences ('populations', in statisticians' terminology) of tiny things, about many of which the poets could hardly have been aware. One can usually obtain some usable results from poems as short as 150 to 200 lines, but everything depends on the frequency of the particular phenomenon that is being studied.

My fourth presupposition, related to the third, was that this method applies only to large bodies of text. It cannot date brief passages or particular lines, although one can certainly observe more recent or more archaic forms within them, and doing so afforded me many insights into the creativity of Homer when I was commenting on the *Iliad*. (It is important to emphasize that the minimum size of the body of text cannot be precisely defined, since

⁶ See Janko (1992: 14 n. 19), with further examples.

⁷ This is the weakness of Hackstein (2002).

it is contingent upon the number of instances of the phenomenon that is under observation and the level of probability that is desired from the result.) Because of this restriction, the method presupposes the general integrity of the transmitted texts of the early epic. But so do most scholars, except for a few diehard analysts and the surprising number of those who think there never was a fixed text. The work of the unitarians and neoanalysts seems to me decisively to support the literary integrity of the bulk of the Homeric epics. Indeed, if we were to seek to date Homer's poems on the basis of the date of their latest lines, we ought to put them into the Roman or Byzantine eras, when concordance-interpolations were still entering the papyri and codices in significant numbers. Thus I am not comfortable with efforts to date whole poems solely on the basis of single lines or brief passages.⁸

Subsequent study of the textual transmission of the Homeric poems convinced me that they may contain minor interpolations that arrived there at a very early date, before many manuscripts of the poems existed. I concluded that 'a pessimistic editor is entitled to suspect that some spurious lines permeated the whole paradosis so early that the Alexandrians could not detect them' (Janko 1992: 28). These would be far harder to discover than are the later concordance-interpolations, which are betrayed by their absence from earlier papyri and/or quotations, but are not all expelled even in West's new text of the *Iliad*. In that text West also identifies a number of early short interpolations.⁹ While I rarely share his confidence that they can be recognized,¹⁰ I agree that in principle they are possible and perhaps even probable; but they are unlikely to affect the statistics. On the other hand, the years that I spent in writing a commentary on the longest books of the *Iliad* strongly reinforced my confidence that so skilfully structured a poem could only have been composed by a single poet at a single time, even though he was using the techniques of oral composition-in-performance, and that we have his poem much as he dictated it.¹¹ Thus a position that is both oralist and unitarian is fully compatible with this statistical method.

My original aim had in fact been to uncover consistent patterns of variation in the separate books of the *Iliad* or *Odyssey*, where clusters of more archaic or innovative diction would indicate the existence of passages of divergent date and origin. However, I never found any, despite months of arduous labour; indeed my results converted me to a unitarian

⁸ This is effectively the error that I made myself when I dated the *Hymn to Aphrodite* after the *Theogony* (above, n. 3).

⁹ West (1998–2000), with West (2001a: 11–14).

¹⁰ See Janko (2000, 2003). ¹¹ See Janko (1998).

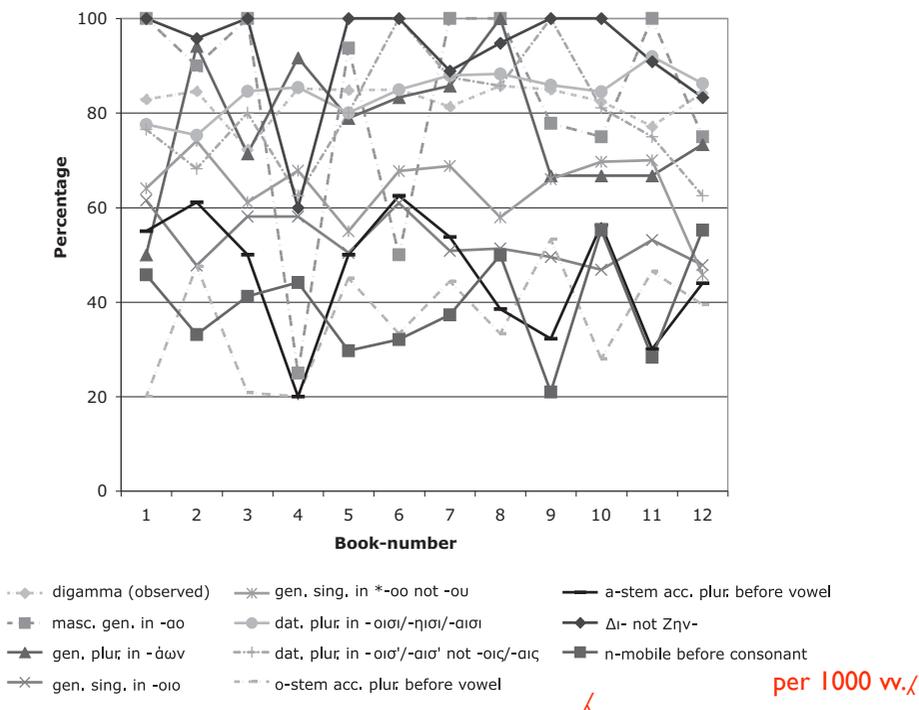


Fig. 1.1 Linguistic variation in the *Iliad*, books 1–12.

position. Since a book that managed to prove nothing in particular would not have been interesting to read or to write, and I had made other discoveries that seemed important, I presented those instead.¹² I supplied in an appendix (Janko 1982: 201–19) the raw data for the individual books of Homer so that others could test for themselves my denial that such variations exist. Few have done so, except for Danek (1988; this volume: 106–121), who discerns only qualitative differences between the Doloneia and the rest of the *Iliad*, in aspects like vocabulary and formulaic usage. Qualitative divergences such as those to which he points will never be able to be detected by a quantitative method such as mine, and their existence and significance will always be a matter of judgement rather than of statistical evidence. As he acknowledges, there are no decisive quantitative differences between the diction of the Doloneia and that of the rest of the *Iliad*; I failed to detect any elsewhere either. Figure 1.1

¹² Janko (1982: 219). Also, the format of my publication limited the number of illustrations and of words. My omission was regretted by one reviewer (Fowler 1983).

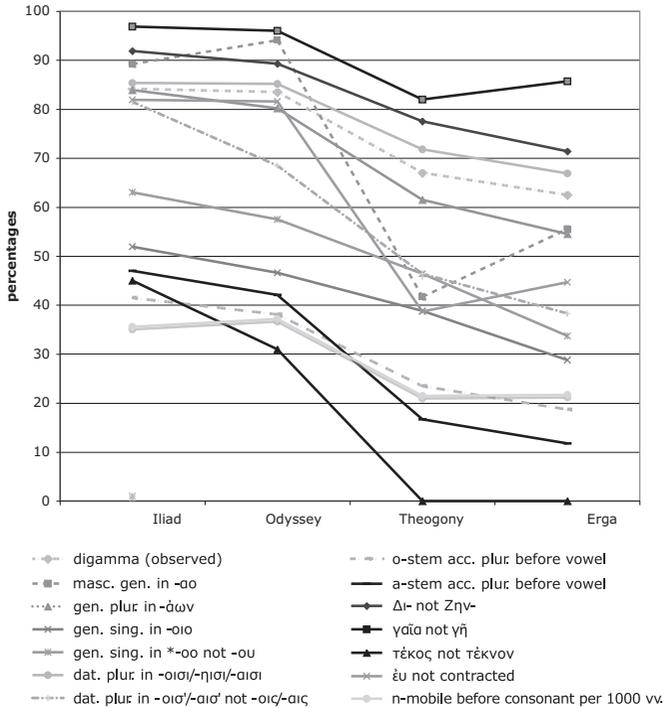


Fig. 1.2 Diminishing archaisms from Homer to Hesiod.

presents as a graph the variations in eleven very common features of epic diction (which I shall explain below) in the first twelve books of the poem, based on the data in my appendix A.¹³ One can see even by eye that the divergences between books exhibit no consistent pattern. The rest of the *Iliad* and *Odyssey* presents a similar picture, indicative apparently of random variation.

When, on the other hand, I plotted the results for the whole epic corpus, I at once found an almost consistent decline in these eleven very common features, as they evolve from the *Iliad* to the *Odyssey* to the *Theogony* and *Works and Days*. Figure 1.2 shows the different choices made in the four poems in fourteen features of epic diction, including three additional ones – the incidence of γᾶια as against contracted γῆ, the failure to contract εὔ and εὔ- to εὔ̄ and εὔ̄-, and the use of τέκος rather than τέκνον. According to the evidence, each feature comprises the retention of an

¹³ Janko (1982: 201–19). I have revised the calculations of the results for -σo and -ᾶων in accord with the suggestions of Jones (this vol.).

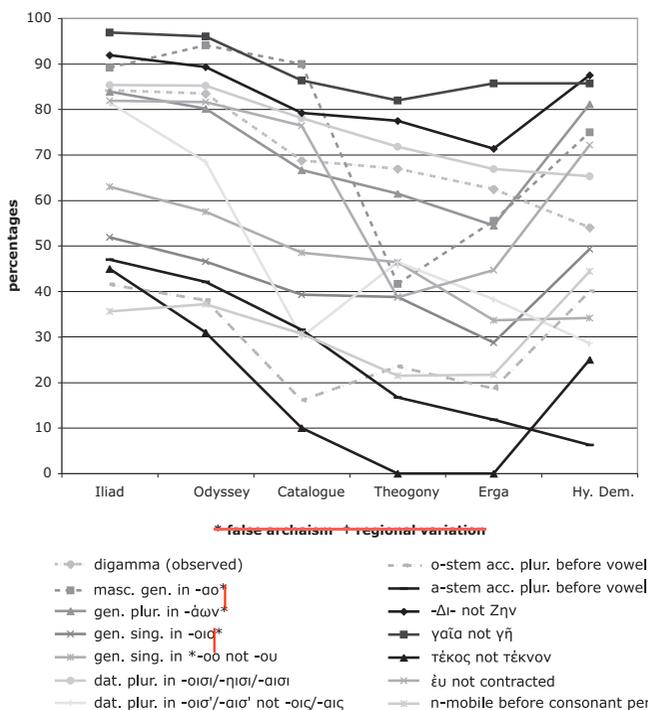


Fig. 1.3 Diminishing archaisms in epic diction.

archaism that could have been replaced by a more recent form. The only feature among the fourteen that does not belong to this pattern is an Attic-Ionic innovation, the use of n-mobile (νῦ ἐφέγκυστικόν) before a consonant; I include this feature to show that it disappears in works of Boeotian origin like those of Hesiod.

Classicists are notoriously averse to numbers, but figure 1.2 should make it clear to anyone that there *is* a coherent pattern in these results. It emerges that the *Odyssey* is nearly always more advanced than the *Iliad*, the *Theogony* is always more advanced than the *Odyssey*, and the *Works and Days* is nearly always more advanced than the *Theogony*. Yet the *Odyssey* is extremely close to the *Iliad*, and the *Works and Days* stands in a similar relation to the *Theogony*. On the other hand, the frequency of n-mobile before a consonant increases over time in Ionic and Attic poems and diminishes in Boeotian ones, so that in this Hesiod seems more archaic than Homer. In figure 1.3 I have added the results for the *Catalogue of Women* and the

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Homeric *Hymn to Demeter*. The data on which this graph is based are given in table I.I, to show that the numbers of instances involved are high; we are not counting just a few occurrences.

As one expects with statistics, figure I.3 reveals two outlying results in the Hesiodic *Catalogue of Women*. However, both are based on small samples, and most of its other results fall between those for the *Odyssey* and those for the *Theogony*. There is, however, a tremendous scatter in the Homeric *Hymn to Demeter*, a phenomenon to which I shall return. In the first five poems, a general pattern is evident. The *Iliad* is the most archaic poem in terms of its diction, the *Odyssey* is next, and Hesiod comes last, generally in the sequence *Catalogue of Women*, *Theogony* and *Works and Days*. This is of course only a *relative* sequence; it is not translated into centuries or generations, let alone years. But it *is* a sequence, and unless the statistics are somehow wrong, it is therefore a fact. As a fact, it is a phenomenon in need of explanation.

Even before I wrote, great effort had gone into arguing that linguistic differences between the poems depend on genre alone;¹⁴ the claim was that some genres are more conservative in their use of old formulae than are others. Much of my book was devoted to examining this claim. It has some plausibility only in the case of genealogy, where the *Catalogue of Women* is consistently slightly more archaic than the *Theogony*, an acknowledged poem of Hesiod which contains rather less genealogy as a proportion of its total number of lines. Yet the *Catalogue* must be either contemporary with Hesiod or later than he, since it is attached in content, structure, and the manuscripts to the end of the *Theogony* (Janko 1982: 82, 191–2). Almost everyone ‘knows’ (or thinks they know) first, that the *Catalogue* is spurious,¹⁵ and secondly that it was composed after the foundation of Cyrene in 630 BC; indeed, many hold that it was written in the sixth century.¹⁶ I shall argue below that neither of these views need be correct.

When poems such as the *Iliad* and *Odyssey* display a greater usage of archaic diction than do the *Theogony* or *Works and Days*, and when they do so consistently, with ‘clusters’ of early results (e.g. the *Odyssey* is nearly always slightly more ‘recent’ than the *Iliad*),¹⁷ it is counter-intuitive to

¹⁴ Notably Pavese (1972, 1974).

¹⁵ Dräger (1997), who ascribes to Hesiod both the *Catalogue* and the end of the *Theogony*, is a recent exception; likewise Arrighetti (1998: 445–7).

¹⁶ So, notably, West (1985: 164–8), Fowler (1998). West’s arguments are successfully rebutted by Dräger (1997: 1–26).

¹⁷ Brantly Jones (this vol.) has proved that the *Odyssey* is more archaic than the *Iliad* in one result, the genitive in -οο, once ambiguous cases before vowels are discounted. I accept his correction, which hardly affects the overall picture. My tables reflect this correction.

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Table 1.1 *The statistical basis of figure 1.3 (%)*

	Iliad	Odyssey	Catalogue	Theogony	Erga	Hy Dem
digamma (observed)	84.2	83.5	68.8	67	62.5	54.1
masc. gen. in -αο	89.2	94.1	90	41.7	55.5	75
gen. plur. in -άων	83.9	80.2	66.7	61.5	54.5	81.2
gen. sing. in -οιο	51.9	46.6	39.3	38.8	28.8	49.3
gen. sing. in *-οο not -ου	63	57.5	48.5	46.4	33.7	34.2
dat. plur. in -οισι/-ηισι/-αισι	85.4	85.2	78.1	71.8	66.9	65.3
dat. plur. in -οισ'/-αισ' not -οις/-αις	81.5	68.5	30	46.4	38.3	28.6
o-stem acc. plur. before vowel	41.5	38.1	16.1	23.5	18.7	40
a-stem acc. plur. before vowel	47	42.1	31.6	16.7	11.8	6.3
Δι- not Ζην-	91.9	89.3	79.2	77.5	71.4	87.5
γαῖα not γῆ	96.9	96	86.4	82	85.7	85.7
τέκος not τέκνον	45	31	10	0	0	25
εὔ not contracted	81.9	81.6	76.4	38.7	44.7	72.2
n-mobile before cons. per 1000 vv.	35.6	37.2	30.7	21.5	21.7	44.4
<i>total no. of instances (sample size)</i>						
digamma (ἐρύκω excluded)	1779	1667	109	88	144	61
masc. a-stem gen. sing.	194	85	30	12	9	4
a-stem gen. plur.	224	162	24	39	11	16
o-stem gen. sing.	2106	1534	163	201	125	75
gen. sing. in *-οο or -ου	1012	619	99	123	89	38
o- and a-stem dat. plur.	1788	1502	137	199	142	101
o- and a-stem short dat. plur.	260	222	30	56	47	35
o-stem acc. plur.	627	422	31	51	32	5
a-stem acc. plur.	383	266	19	42	17	16
oblique cases of Ζεύς	271	112	24	40	21	8
γαῖα and γῆ	128	174	22	61	21	14
τέκος and τέκνον	79	49	10	11	2	8
εὔ and εὐ	360	370	34	31	38	18
n-mobile before consonant	559	451	23	22	18	22
no. of verses	15693	12110	c. 750	1022	828	495

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suppose that this could be explained by anything other than linguistic change through time. I shall look at other possible explanations shortly; but we should only embrace a different kind of explanation if we have other, compelling types of evidence that point us in that direction. It is both simpler and more logical to accept that the preponderance of early diction in Homer as compared with Hesiod is because the texts of Homer were somehow fixed at an earlier date than were those of Hesiod.

There is one major complexity to be addressed before we turn to the implications of this relative chronology of Homer and Hesiod. Figure 1.3 reveals that the Homeric *Hymn to Demeter* does not behave like the texts of Homer and Hesiod. The *Hymn* has more recent diction than Hesiod in six features, but more archaic diction in seven others (I exclude n-mobile, as this phenomenon varies by region). The more recent features are (a) the rarity of γᾶ rather than contracted γῆ; (b) the infrequency of elided o- and a-stem dative plurals in -οισ' and -ηισ' before vowels; (c) the rarity of a-stem accusative plurals before vowels; (d) the rarity of long dative plurals in -οισι and -ηισι; (e) the rarity with which the uncontracted o-stem genitive singular in *-οο could be restored; and (f) the rarity with which the effect of digamma is observed. Of these six criteria, all are based on statistically reliable samples. The reason why the poet does not archaize in observing the digamma, or in observing the earlier frequency of uncontracted genitives singular in *-οο, is that he saw no reason to do either thing; he probably did not know that there were archaisms at stake in these features of his language, since the existence of digamma and of the uncontracted genitive in *-οο was long in the past of the Attic dialect when he composed his *Hymn*.

The pattern is different in other linguistic features, some of which poets could have consciously manipulated in order to achieve particular literary effects. The seven more archaic features of the *Hymn to Demeter* are (a) the frequent use of the stem Δι- instead of Ζην- in the name of Zeus; (b) the frequent retention of the archaic genitive plural in -άων; (c) the retention of the archaic genitive singular in -οο; (d) the retention of uncontracted εὔ both on its own and in compounds; (e) the retention of the archaic genitive singular in -οιο; (f) the frequent use of the accusative plural in -ους before a vowel; and (g) the frequent use of archaic τέκος rather than τέκνον. However, no fewer than four of these phenomena rest on unreliably small samples of eight occurrences or fewer: these are (c) the retention of the archaic genitive singular in -οο, (d) the retention of uncontracted εὔ instead of εὐ, (e) the frequent use of the accusative plural in -ους before a vowel, and (g) the frequent use of τέκος rather

than τέκνον. Accordingly, no weight can be placed upon these criteria. But the other three are statistically significant, and are distinctive because they are features which the composing bard certainly could have manipulated consciously. First, the retention of the Attic stem Δι- instead of the East Ionic Ζην- in the name of Zeus is a feature which one can attribute to the vernacular Attic dialect of the poet. The second and third features are the retention of the archaic genitive plural in -ᾶων and the retention of the archaic genitive singular in -οιο. Both features are sonorous disyllabic endings, and I believe that the poet consciously favoured them in order to enhance the solemnity and archaic flavour of his verse.

Examination of most of the other long *Homeric Hymns* and of the Hesiodic *Shield of Heracles* reveals even more numerous departures from the pattern, so much so that they have no ‘cluster’ of results at all (Janko 1982: 74–81, with table 24 and fig. 2); I forbear to demonstrate this here, and refer you to my book and to a related article (Janko 1986b), where I argued from historical evidence that the *Shield of Heracles* and the *Hymn to Pythian Apollo* were both composed in central Greece in the first two decades of the sixth century BC, with the *Shield* being prior. I explained the presence of anomalous results like those in the *Hymn to Demeter*, the *Shield of Heracles* and the *Hymn to Pythian Apollo* as follows. Once we are dealing with poems from a very late stage in the tradition, the poets are no longer learning their diction exclusively from their older living contemporaries. They are also hearing renditions of texts fixed at much earlier stages of the tradition, from which they are learning to adopt certain archaisms, i.e. those which are immediately noticeable to a listener. These poets did not care whether they put a formerly uncontracted disyllable in heavy or light position in the hexameter, but they felt passionately about producing a grand-sounding poem, packed with euphonious polysyllables like -ᾶων and -οιο. The same can be demonstrated for all subsequent composers of Greek hexameters, from Theocritus and Apollonius of Rhodes to Quintus of Smyrna and Nonnus (Janko 1982: 76).

I called this phenomenon ‘false archaism’, which still seems the best name for it. Poets who practise ‘false archaism’ are imitating older models selectively. They could not imitate them perfectly, even if they wished to do so. However, if poets are free to imitate aspects of older models from the same tradition, one may well ask what value an analysis of their language can possibly have for chronological purposes. In effect, this phenomenon of ‘false archaism’ might seem completely to demolish the edifice of gradual linguistic innovation that I had carefully constructed.

Yet I do not believe that it does. Just because some epic poets practised ‘false archaism’, it does not logically follow that they all did so. The consistency of the results that I obtained for Homer and Hesiod shows that their poems are not subject to ‘false archaism’ but are genuinely archaic compositions, whose the bards learnt their diction from their contemporaries and not from some far older, fixed text. If such texts were available in any form to Homer and Hesiod, they have had no discernible effect on the texture of the language of those poets. The later poets, on the other hand, like those of the Hellenistic and Roman periods, offer no such consistent patterns.

So what is the significance of the fact that the poets of the long *Hymns* to Hermes, Demeter and Pythian Apollo practise ‘false archaism’, whereas the texts of Homer and Hesiod, including the *Catalogue*, show no sign of it? The only explanation is that those poets who had access to texts fixed at much earlier stages of the tradition were themselves later than Homer and Hesiod, who had no such access to such ‘fossilized’ material. Later poets knew the work of much older predecessors, as did their Hellenistic and Roman successors. This accords with the growing diffusion of literacy in the Greek world, and with the gradual increase in the number of written texts and of performances based on them, such as are documented for Athens in the time of the Pisistratids. The picture of a coherent evolution of the epic diction remains consistent.¹⁸

Hence one must ask what it means that the texts of the two poems of Homer and the three Hesiodic poems were ‘fixed’ at different stages in the evolution of the diction. I have already suggested that this reflects a relative chronological order. But the fact that the poems were fixed at different times has other important implications. If we believe, as I do, that the *Theogony* and the *Works and Days* are by a single poet, the evolution in Hesiod’s diction from the *Theogony* to the *Works and Days* shows that the epic *Kunstsprache* could evolve even during the lifetime of a single poet; the same is true, *mutatis mutandis*, for Homer, where ‘Longinus’ (*Subl.* 9.13) plausibly suggested that the *Odyssey* is the work of the poet’s old age. Others are willing to accept Kirk’s hypothesis (1962: 301–34) that the epics were accurately transmitted by memory over a considerable period before they were written down; but I have never seen any decisive argument in favour of this view, since these are not sacred texts like the *Rig Veda*. It is

¹⁸ Hence the account of my method in Martin (2005: 167) is gravely mistaken when it is said that there is no criterion for distinguishing false archaism, when the criterion is lack of consistency in the diction, i.e. the lack of ‘clusters’.

far easier to accept that the epics acquired a fixed form at different times, that we have them in much the form in which they acquired this fixity, and that their language differs in these subtle ways precisely because they were fixed at different times. Others, again, may prefer to believe that the authors of all these poems wrote them down in this form. However, like Kirk's theory, this too is an unnecessary hypothesis, because it assumes that these poems could not have been composed without their authors using writing. This belief, in my view, reflects our own lack of imagination and of experience of an oral culture rather than the realities of skilful oral epic composition in a largely illiterate society.

Since the evidence shows that the poems were fixed at different stages in the ongoing evolution of the tradition, those who wish to ascribe their present form to editors must accept that they have been uniformly and continuously updated by experts in historical linguistics to keep them distinct from each other in terms of language. This is not credible. Accordingly, I supported very tentatively in my book, and more definitely since, the hypothesis of Milman Parry and Albert Lord that these epics are oral dictated texts, and that their linguistic form accurately reflects the different stages during the tradition when they were taken down. I believe, following Lord, that the slower pace of dictation permitted the best poets to produce better poems than they could have sung.¹⁹ The simplicity, coherence and comprehensiveness of this explanation of how the texts were fixed have never remotely been matched by any other theory.

Although some reviewers of my book wrote that they could not follow all the statistics, on the whole they accepted my results, at least as regards relative chronology. However, as time went on it began to appear that the book was being more cited than read. Unfortunately, it went out of print within ten years. Also, it presupposes much expertise in historical linguistics and the Greek dialects, as well as an intimate knowledge of early Greek hexameter poetry. Although I kept the statistics as elementary as possible, the fifty-two tables of numbers are dreary stuff in themselves. In an account of the language of early Greek epic I supplied further statistics which support my case (Janko 1992: 8–19, esp. 12–14), but none of my recent critics takes account of these additions to my theory.

During the 1990s I began to notice some really strange pronouncements about the date of Homer. These often began by setting aside the linguistic evidence, allowing scholars to adopt other approaches, above all the notion

¹⁹ Lord (1953, 1960: 124–8, 1991: 11–12, 38–48, 98–9, 109–10, 1995: 102–3); cf. Parry in A. M. Parry 1971b: 451.

that the texts were never fixed. This was in my view already refuted by the statistical results presented in my book. Such critics usually cited, sometimes apparently at second hand, my appendix E. There I offered, among a number of tables giving different scenarios for the absolute dating of the epics, provided that their relative dates were upheld, the observation that, if the date of the composition of the *Iliad* were set at around 750–725 BC, that of the *Odyssey* would fall at around 743–713, the *Theogony* in 700–665, and so on. The citation of these exact dates became a sort of talisman for these scholars,²⁰ allowing them to overlook the mountain of statistical evidence, because it seems absurd to give an exact date in a case where there will never be documentary evidence for the precise years in which these texts were fixed. I quote an example of such a critique, written by a scholar whose work I admire in other respects:

[T]here is still the internal evidence of linguistic forms to consider. On the basis of such evidence, Janko (1982) has insisted that the Homeric poems must date from the eighth century, and he came to conclusions about the relative dating of many other poems as well. Janko's study displays an unsettling confidence that epic verse everywhere was developing in the same way in a robotically steady manner. The statistical quantification of forms here does not make enough allowance for the varying length of the different poems that are being compared, and indeed the very meagerness of what survives in some cases can only lead to misleading percentages. Variance in diction and form between poems was *undoubtedly* [my emphasis] caused by subject matter, poetic function, local dialects, and the preferences or ability of composers. The epic tradition on the whole was a swirling flux of crosscurrents, which a rigid statistical analysis cannot hope to measure. And even if we accept conclusions about chronological relativity, that means nothing about the historical time of the poems. An unverifiable argument about the Lelantine War is the peg on which Janko hangs the whole frame of his relative dating to a historical timeline. Finally, the very desirability of assigning specific dates to individual poems is open to question. Assuming a moment in time for the fixation of early epic does not change the fact that a lengthy process of oral composition lay behind it. (Burgess 2001: 52–3)

A footnote adds '[t]o be fair, Janko is certainly not unaware of these issues', and gives page-references that amount to 15 per cent of my book.

²⁰ E.g. Taplin (1992: 33 n. 39). Others make a similar error over the level of exactness in dating, when they hold that I believe that, because the *Theogony* is linguistically slightly more advanced than its sequel the *Catalogue*, the latter was necessarily composed before the former (e.g. Haubold 2005: 87 n. 8). As I had noted (Janko 1982: 85–6), 'linguistic tools are inevitably somewhat blunt': greater divergence than this is found within the attested work of Hesiod himself, i.e. between the *Theogony* and *Works and Days*. It is of course possible that Hesiod dictated the *Catalogue* before he dictated *Theogony* 1–885.

As for ‘epic verse developing everywhere in a robotically steady manner’, I wrote many pages on the influence of local dialects, since I wanted to establish the origin of each poem. I found that one can easily distinguish Ionian from mainland compositions, which have Boeotian influence in Hesiod and Attic features in the *Hymn to Demeter*. As for ‘not mak[ing] enough allowance for the varying length of the different poems that are being compared’, what matters is the number of occurrences, not the length of the poem, although the two things are related. Part of the purpose of statistics is that it enables one to compare populations of different sizes. Every result was subjected to tests such as *t*-tests and χ^2 tests to make sure that it was statistically reliable, as my footnotes reveal; results based on inadequate sample-sizes were set aside. Again, when it is suggested that the diction was affected by ‘the preferences . . . of composers’, this can only be partly true; in many cases, such as how many uses of the contracted genitive singular in *-ου* can be replaced by the uncontracted form in **-οο*, the bards cannot have been aware of this difference and had no experts in historical linguistics to advise them. West (1995: 204–5) suggests that ‘the major determinant of the quantity of younger forms in a given poet is the extent to which his language diverges from the formulaic, and this depends on many other factors apart from his date’. However, it is not clear how one could be certain that this is the major determinant rather than the date: in the Homeric poems the proportion of speeches, which constitute the least formulaic part of the diction,²¹ is the highest in the corpus, yet these are the poems with the most archaic diction. Moreover, I did examine, at some length, the question of ‘variance in diction and form between poems’ being caused by ‘subject matter, poetic function, local dialects, and the preferences or ability of composers’, and wrote as follows:

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It might be alleged, for example, that battle-poetry has more ancient antecedents than stories of nautical adventures, genealogies or gnomic and rustic verse. Such an approach is almost impossible to test on present evidence, at least within the early Greek tradition, with one important exception, and that is in Hesiod. It is sometimes assumed that *gnomai* and accounts of rural activities are likely to be the least traditional of all the types of hexameter poetry of the archaic period. Certainly it is true that the diction of the *Works and Days* is very advanced: but it is nonetheless not as advanced, by a good margin, as the narrative in the *Hymn to Demeter*, which is in terms of content reminiscent of scenes in Phaeacia and Ithaca in the *Odyssey*. Moreover, *Works and Days* 1–200 consists of mythic narrative, and

²¹ Griffin (1986). It does not of course follow from this fact that the Homeric poems cannot be orally composed, as Griffin argued.

not of moral and agricultural precepts . . . And yet, when the language of these two hundred lines is studied scientifically,²² it is established that, although it is somewhat less advanced in the linguistic criteria than the remainder of the poem, it is on the whole more advanced than the diction of the *Theogony*. Hesiod's diction advanced, as we know, but it advanced despite the content. (Janko 1982: 192)

However, the critic whom I quoted is partly correct that 'if we accept conclusions about chronological relativity, that means nothing about the historical time of the poems'; it is true that relative and absolute chronology are different questions. I would only say that, if the artistic or historical evidence for the date of Homer did point decisively to the seventh or sixth century, then Hesiod would have to be later still by some margin or other. If people wish to accept this, they will need to explain how it is that Hesiod is imitated not only by Alcaeus but also by Semonides of Amorgos, since in another appendix in my book (225–8) I surveyed all the apparent cases, in the epos and other early poetry, of *exemplum* and *imitatio*. There I showed that there are a number of ways in which this can reliably be recognized from the adaptation of formulaic diction. As for the eighth-century date of Homer and the Lelantine War, what I actually wrote was 'if . . . the Lelantine War can indeed be dated to Archilochus' time, and if Hesiod was connected with it, and if Homer was not prior to the mid-eighth century and not later than its end, then the relative datings will be corroborated, but no universal agreement on these points exists at present' (195–6). There were a lot of 'ifs' in this sentence, and deliberately so, since the historical evidence is hard to confirm; but the arguments about the Lelantine War have not changed, even if West is no longer so confident about them.²³

Finally, if critics question the desirability of assigning dates to individual poems, I do not see how one can understand the literary history of early Hellas without knowing at least the relative dates of the major poems. One might as well say that ignorance is better than knowledge. Other critiques of my general approach have been similar to those discussed above.²⁴

²² I gave the figures in appendix B (220–1).

²³ West (1995: 218–19): contrast West (1966: 43–4).

²⁴ Here is another: '[Janko] uses various linguistic criteria to establish that the language of the *Iliad* and *Odyssey* is less "modern" than that of other hexameter poems. On the basis of this observation he establishes a relative chronology of Homer, Hesiod and the *Homeric Hymns*; he then assumes an eighth-century date for the *Iliad* and uses it to calculate the absolute dates of the other poems. There are various possible objections . . . : in the first place, the eighth-century date is, by his own admission, simply assumed. Secondly, Janko's own linguistic criteria measure choices between alternative epic forms which sound more or less archaic, but which were all current within the Ionian poetic language, given that they feature, with greater or lesser frequency, in all or most early

When one critic suggested that ‘assuming a moment in time for the fixation of early epic does not change the fact that a lengthy process of oral composition lay behind it’, nobody denies this; in fact, projecting back from Homer’s usage to the beginnings of various linguistic phenomena confirms the view of Meister, Parry, West, Ruijgh and now Haug (2002: 161–2) that the epic tradition resided in Asiatic Aeolis until shortly before Homer’s time (Janko 1982: 87–94, with fig. 3 on p. 88). Here I must address the discussion in this volume by Brandtly Jones, who earns my thanks for being the first to acknowledge the statistical dimension of the prehistory of the epic diction. His account of my sequence of changes in the prehistory of epic diction seems to misunderstand my argument in several ways.²⁵ However, he is right that my method of calculation of the introduction of quantitative metathesis in the a-stem genitives was flawed. I should have excluded from the calculation the cases of $-\epsilon\omega$ before vowels, because these could, as he suggests, be instances of elided $-\alpha\omega$. When this is done, the relative date at which a monosyllabic scansion of the a-stem masculine genitive singulars enters the epic diction turns out to be -0.7 units (rather than -1.7 as I had thought) on the Common Scale, where the *Iliad* is set at 0 and the *Theogony* is set at $+3.0$, because the result for the *Iliad* changes. Similarly, I should not have omitted the forms in $-\tilde{\omega}v$ and $-\tilde{\alpha}v$ from the totals of a-stem genitive plurals that admit quantitative metathesis, since

(Chapter 2
below)

hexameter texts. Choices and preferences between available words and expressions may thus be influenced by the subject-matter of the poem, the specific area in which it was composed, and the tastes of particular bards and audiences, and not simply by the date of composition’ (Graziosi 2002: 91–2). These are very similar arguments.

A different critique is that of Sauge (2007). (I am grateful to M. Sauge for a copy of his book.) In order to set aside my results and argue that the epics were recorded in sixth-century Athens, Sauge has to reject Hoekstra’s overwhelming proof that epic poets modified their formulae (81 n. 67). Instead, he suggests that the epic diction (but not the texts) was fixed in the early eighth century(!), perhaps by Cynaethus on Chios (67, 365); but Cynaethus was alive in c. 525–500 BC (Burkert 1979). He holds that the poets learned and employed two different pronunciations at once. Thus, he believes (74–83), in phrases involving initial digamma they pronounced the phoneme or ignored it according to whether it is ‘observed’ or ‘neglected’ (and in order to furnish an objection to one of my results he rejects the consensus that ‘initial’ digamma in enclitics is in fact in a medial environment, as the usage of Sappho and Alcaeus proves). I have not seen Blümer (2001: I. 130–8); like Sauge, he does not address my argument from ‘clusters’ (Fowler 2003: 8), which is the crucial one.

²⁵ Regarding $-\omicron\iota\omega$, $-\acute{\alpha}\omega\nu$ and $-\alpha\omega$, my point is indeed that the metrical and not the phonological shape of the termination changed, but I do not see how this affects the argument: if $-\eta\omicron$ was ever present in the epic diction, why was it changed back to $-\alpha\omega$? Regarding the change in the admission of elision in the dative plurals, Greek has or has not admitted elision in different words at different times, and it is surprising that Jones misconstrues me as thinking that such a change could have reflected a general change in the practice of elision in all words. Nor is it ‘bizarre’ to suggest that the datives of o - and a -stems were treated differently, since these represent syncretisms of different endings: in the o -stems Mycenaean has distinct endings in $-o-i$ and $-o$ (from the instrumental in $*-ois$), whereas in the a -stems it has only $-a-i$.

in the case of $-\tilde{\omega}ν$ these contracted forms go a step beyond quantitative metathesis and in that of $-\tilde{\alpha}ν$ they are an equivalent response in a dialect other than Ionic. When this is done, the relative date at which a-stem genitive plurals with a monosyllabic scansion enter the tradition turns out to be -2.15 rather than -1.7 .²⁶ The sequence of changes that results from his corrections is as follows:²⁷

1. $-\omicron\iota\omicron$ ceases to be sole o-stem genitive singular ending: -11.0 .
2. Contraction in $*-\omicron\omicron$ first appears: -6.7 .
3. o- and a-stem dative plurals begin to admit elision: -3.2 .
4. Initial prevocalic digamma begins to be neglected: -3.0 .
5. Monosyllabic scansions begin to replace $-\acute{\alpha}\omega\nu$: -2.15 .
6. The Ionic declension Ζηνός etc. first appears: -1.6 .
7. ‘Short’ o- and a-stem dative plurals which cannot be elided first appear: -1.5 .
8. Monosyllabic scansions begin to replace $-\alpha\omicron$: -0.7 .
- 9, 10. o-stem and a-stem accusative plurals begin to be biased towards appearing before consonants: not before -1.4 and -0.4 respectively.

Jones notes that the small sample of a-stem genitive singulars in the *Theogony* (only 12 in total) is a ‘narrow and insecure footing on which to base the date of the Aeolic phase of the epic tradition’ (this vol.: n. 19). The sample is indeed too small, but of course we cannot obtain another sample of Hesiod’s diction as it was at that moment – this is all we have; and it is not the sole basis for the chronology, since there are also the results for the other works of the corpus and the behaviour of the genitive plurals. Thus the *Works and Days* has a small population of a-stem genitive singulars (only 9 instances); again this is a low number, but 55 per cent of them are in $-\alpha\omicron$, as opposed to 41.7 per cent in the *Theogony*. Hence their incidence in the *Theogony* may be abnormally low; if the true percentage of forms in $-\alpha\omicron$ there were higher, it would yield an earlier date for the appearance of the monosyllabic singular endings.

Jones’s corrections, while valid, do not in my view disprove the existence of the Aeolic phase. The innovations that I associate with the start of the Ionic phase all appear between -2.2 and -0.7 . The introduction of the declension Ζηνός, which is unquestionably Ionic, falls in the middle of this range at -1.6 . If the arrival of monosyllabic genitives did predate the Ionic phase, a question where the data point in different directions, they could at first have been in the contracted forms $-\alpha$ and $-\tilde{\alpha}ν$ known in Lesbian

²⁶ Jones’s calculations of this (his n. 000) are correct in both cases.

²⁷ For the original list see Janko (1982: 87–8).

Aeolic.²⁸ It is unlikely, given their phonetic similarity to each other, that the forms $-\alpha\omicron$ and $-\acute{\alpha}\omega\nu$ entered the Ionic tradition gradually, by borrowing from an adjacent tradition; the discrepancy in the dates of their appearance is likely to be a result of the vagaries of statistics based on small populations, especially in the case of the genitive singular.

The opponents of the Aeolic phase have still not explained why the tradition did not retain uncontracted $-\eta\omicron$ and $-\acute{\eta}\omega\nu$ if it formerly used them (not to mention why it first used Aeolic $\pi\omicron\tau\acute{\iota}$, rather than Mycenaean *po-si*, and then replaced $\pi\omicron\tau\acute{\iota}$ with $\pi\rho\acute{\omicron}\varsigma$).²⁹ Jones (this vol.) is right that the Bosnian singers put together their *Kunstsprache* from different dialects of Serbo-Croatian where these supply metrically convenient alternatives; but this analogy will not explain why the Ionian bards used $-\alpha\omicron$ and $-\acute{\alpha}\omega\nu$, since their own archaisms $-\eta\omicron$ and $-\acute{\eta}\omega\nu$ would have been metrically equivalent. The words cited by Jones (this vol.) with different vocalisms like $\sigma\tau\acute{\epsilon}\iota\omicron\mu\epsilon\nu$ and $\sigma\upsilon\lambda\acute{\eta}\tau\eta\nu$ are all rarer morphophonemically than the a-stem genitives. Accordingly, they are more likely to have been created by bards by analogy with the vocalism of their current speech than to have been retained from an older phase of the tradition.

Finally, Jones (this vol.: 000) ~~rightly reminds us that~~ Ionic and Attic had two successive phases of quantitative metathesis: the first occurred where there had been an intervocalic $*-s-$, the second where there had been an intervocalic $*-w-$. Hence Ionic experienced quantitative metathesis in $-\acute{\alpha}\omega\nu$, deriving from $*-āsōm$, before it lost intervocalic digamma. However, the sequence of changes in the traditional diction, even as corrected by Jones, still places the introduction of both of the monosyllabic a-stem genitives (nos. 5 and 8 in the list above) *after*, indeed well after, the loss of initial prevocalic digamma (no. 4). Initial prevocalic digamma and medial intervocalic (and therefore, regarding the second syllable, prevocalic) digamma constitute very similar phonetic environments; in those dialects which we can observe in inscriptions, the loss of digamma in both environments takes place in the same manner and at the same time. Hence this sequence of changes supports the existence of the Aeolic phase.

Thus there can be no doubt that the epos acquired its East Ionic veneer not long before the time of the *Iliad* – whenever that was – and that it had already had these features for some considerable time when it was taken back across the Aegean to appear in the poems of Hesiod. Recent (non-statistical) linguistic study by the late C. J. Ruijgh suggests that in

²⁸ So already Janko (1982: 250 n. 48).

²⁹ See my statistical article ‘The use of $\pi\rho\acute{\omicron}\varsigma$, $\pi\rho\omicron\tau\acute{\iota}$ and $\pi\omicron\tau\acute{\iota}$ in Homer’ (Janko 1979), to which only D. Haug (this vol.) refers.

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fact the linguistic gap between Homer and Archilochus is actually greater than I and others had argued, and that the Greek alphabet was borrowed from the Phoenicians in about 1000 BC, far earlier than has been thought; Ruijgh (1995, 1997) dates the Homeric texts to the ninth century BC. His arguments, which adversaries of the Aeolic phase have not yet addressed, merit serious consideration.

I wish to conclude by considering the date of one particular poem – the *Catalogue of Women*. Hirschberger's new commentary (2004: 43–9) offers an exhaustive discussion of the scholarly views about its date. As we have seen, its language is almost identical with that of the *Theogony*, of which it is formally the continuation. Its *terminus ante quem* is the date when the *Shield of Heracles* was composed, since the *Shield* is a continuation by a different poet of the *ehoie* of Alcmena in book 4 of the *Catalogue*; anyone who doubts this must deny that the *Catalogue* is a unitary composition, a denial to which Hirschberger raises cogent structural objections (2004: 47). I argued on historical grounds (Janko 1986b) that the *Shield of Heracles* was composed by a pro-Theban poet in c. 600–590 BC, just before the First Sacred War; the *Shield* was itself cited by Stesichorus, who said it was by Hesiod.³⁰ Nearly everyone's *terminus post quem* for the composition of the *Catalogue* is the foundation of Cyrene in Libya by colonists from Thera in about 631 BC, since the poem referred to the nymph Cyrene. According to a scholiast on *Pythian* 9 (*Pyth.* 9.6a Drachmann), Pindar took the story from an *ehoie* of Hesiod. By this argument, anyone who wishes to date the *Catalogue* to before c. 631 must deny the unity of the poem, and assume that the passage about the nymph Cyrene was added later.

However, matters are not so simple. The fragments of the *Catalogue* that are quoted tell us that the nymph Cyrene lived by the river Peneios in Thessaly (fr. 215 M–W) and gave birth to Aristaeus, presumably by Apollo (fr. 216 M–W). Pindar locates her wrestling match with a lion on Mount Pelion, although Callimachus (*Hymn* 2.91–2) and the Hellenistic historian Acesander (*FGrH* 469 F 4) put it in Libya. Hesiod mentioned a lion somewhere in his poetry (fr. 328 M–W). A Thessalian location for this combat is perfectly possible, since there were lions in northern Greece until much later times.³¹ On the other hand, there is simply no evidence that the *Catalogue* made Apollo take his bride to Libya, even though most scholars assert this.³² Pindar may have invented this episode.

³⁰ In the hypothesis to the Ps.-Hesiodic *Scutum*.

³¹ Cf. Hdt. 7.125–6, Xen. *Cyr.* 11.1 and Paus. 6.5.5, with Alden (2005: 336–7).

³² E.g. West (1985: 88). Contrast Janko (1982: 86, 248), Köhnken (1985: 101, 103), Dougherty (1993: 147), Dräger (1993: 221–8).

It is also disputed whether Chiron's dialogue with Apollo on Mount Pelion goes back to Hesiod, or is another invention of Pindar's; this conversation does not match the style of the *Catalogue*.³³ Cyrene's name, by the way, originated in Greece, since its form follows familiar principles of pre-Hellenic onomastics: for the very common suffix *-ānā* cf. Athānā (Athene), Mycene, Messene, Peirene and so on, and for the root compare (Anti)cyra. It is simply bizarre that the city of Cyrene be linked with Thessaly, unless that was where the nymph was originally imagined to have lived before the city was founded. D'Alessio has now proposed (2005: 206–7) that, although the *Catalogue* said that Cyrene was beloved by Apollo and the father of Actaeon (fr. 217A), the *ehoie* of Cyrene on which Pindar drew was in the *Megalai Ehoiai*, which was a different and probably later poem. His suggestion rests on the fact that the scholia to Pindar consistently cite the *Megalai Ehoiai* rather than the *Catalogue*, and may well be correct.

Another Hesiodic passage brings Chiron into contact with Aristaeus' son Actaeon, again on Mount Pelion. They meet in Hirschberger's F 103, a fragment which Merkelbach and West mistakenly chose to omit.³⁴ Actaeon was Aristaeus' son by Autonoe in Thebes. According to this genealogy, if Apollo took Cyrene to Libya for her honeymoon, he brought her back to Greece (unless Aristaeus flew back once he had become a god). I am joking, of course, but the need for such fanciful hypotheses shows how improbable it is that Cyrene went to Libya in the *Catalogue*. Hence the foundation of Cyrene in Libya cannot be used as a *terminus post quem* for the *Catalogue*. Hirschberger can find no other solid evidence for dating the poem so late. She places the poem between the foundation of Cyrene and the composition of the *Shield*, i.e. between 631 and c. 590, and certainly not later in the sixth century (2004: 49). It would be more prudent to rely on the language of the poem, and on its degree of geographical knowledge (which is comparable to that of the *Theogony*), than on the unproven and unlikely hypothesis that the *Catalogue* made Apollo whisk Cyrene from Thessaly to Libya and back.

Despite the fact that the diction of the *Catalogue* is in some ways fractionally more archaic than that of the *Theogony*, I see no reason why it should not be by the same poet as the *Theogony*, of which it presents itself as the continuation. That poet calls himself Hesiod. He moves from listing the gods' unions with goddesses, starting with Zeus and Metis (*Theog.*

³³ Köhnken (1985: 100–1); cf. Dräger (1993: 226).

³⁴ West (1985: 88); contrast Janko (1984). It is included by Most (2006). The piece could however be from the *Megalai Ehoiai*.

886–929), to their unions with nymphs or mortals (930–62); then, with an appeal to the Muses, he lists the unions of goddesses with mortals (963–1018). This is followed by a summation, and then a renewed appeal to the Muses leads into the *Catalogue*. Even Merkelbach and West think that the end of the *Theogony* was composed by the same poet as was the *Catalogue*. My results showed that the end of the *Theogony*, the authenticity of which West had questioned,³⁵ belongs statistically with the *Catalogue of Women*, and so I believe that the whole composition is likely to be by Hesiod. Thus I hope that this linguistic method can contribute, among other insights, to a re-evaluation of the *Catalogue*; for we apparently have more poetry by Hesiod than we thought. Recent studies show that such a re-evaluation is already under way (Dräger 1997; Arrighetti 1998; Hirschberger 2004; Hunter 2005).

³⁵ West (1966: 397–9). It is interesting that Haug (this vol.) has found a discrepancy in the use of tmesis in *Theog.* 900–1022; again I wonder whether Hesiod dictated the *Catalogue* first.