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Cato's Origines and the non-Roman
historical tradition about ancient Italy

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by

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ABSTRACT

As its title suggests, this study falls into two parts.

The purpose of the first part is to assess the character of Cato's lost historical work. I have tried to refute the prevalent opinion that the Origines was a posthumous compilation of two or more separate works (Chapter I), and to explain why Cato omitted the early history of the Republic and wrote instead about the origins of Italy (Chapter II). Chapters III and IV contain a more detailed study of the form and content of the individual books of the Origines, while Chapter V examines the notion that Cato's account of Italian origins was inspired by a branch of Greek literature concerned with foundations. (Detailed evidence about the Ktiseis is presented at the end of the thesis in Chapter XI).

The second part of the thesis investigates the sources used by Cato in his Italian researches. Cato was the first writer to make a serious study of the earliest history of peninsular Italy. Previous researchers had not done his work for him, and I have argued that his account was founded on a first-hand study of the primary evidence (Chapter VI). A large amount of historical evidence in the form of oral tradition, documents and literature would have been available to Cato in the communities of

non-Roman Italy. The extent and character of this material, collectively labelled "the non-Roman historical tradition", is illustrated by the test case of the Etruscans. Chapters VII and VIII are devoted to a wide-ranging study of Etruscan historiography and other kinds of historical tradition in Etruria. Finally, taking as specific examples his researches into the history of Campania (Chapter IX) and the origin of the Sabines (Chapter X), I have tried to show more precisely how Cato made use of indigenous sources.

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CHAPTER I *

(i) Introduction

The historical work with which Cato the Censor occupied his leisure towards the end of his long and active life has not come down to us; but the fragments and testimonia which are preserved in quotations and references in other ancient writers are numerous enough to show clearly that the Origines (as the work was called) bore the unmistakable marks of individualism and opinionated honesty that had sustained the author throughout his public career.⁽¹⁾ From the little we know about the Origines we can assert confidently that Cato lived up to the precept he had laid down in the preface: "clarorum hominum atque magnorum non minus otii quam negotii rationem exstare oportere".⁽²⁾

The work was in many respects quite revolutionary. Nothing in the whole field of Roman historiography can be compared to it. "Sein Werk", wrote Niebuhr, "steht in der ganzen römischen Annalistik allein".⁽³⁾ The slightest glance at the surviving fragments shows that the Origines was totally unlike the later 'Annales', whose general characteristics are familiar to us from Livy and Dionysius of Halicarnassus; as we shall see, Cato explicitly dissociated himself from that type of

(* For notes to Chapter I, see below, pp. 46-54.

historical writing.⁽⁴⁾ In a sense it can be said that Cato had more in common with the earliest Roman historians, who wrote in Greek, than with the later annalists; his work, like that of Fabius Pictor, was a unique product, and its character was determined more by the historical situation in which it was composed than by the influence of any established literary form.⁽⁵⁾

And yet Cato deliberately turned his back on Fabius and his followers by writing in his native language. As far as one can tell, the Origines was the first Roman history of any importance to be written in Latin.⁽⁶⁾ We know of the contempt with which Cato dismissed the work of Postumius Albinus, who had been rash enough to begin by apologising for his inadequate knowledge of Greek.⁽⁷⁾

But the originality of Cato's work was not confined to his use of Latin. Perhaps its most unusual feature was the fact that it contained an account of the origins and early history of the whole of Italy, not just of Rome. This attention bestowed on the history of non-Roman Italy was in fact unique in Roman historiography.⁽⁸⁾ Other Roman historians touched on the early history of the Italian communities only when it had some bearing on the affairs of Rome.

Of course this interest in the non-Roman historical tradition of Italy led Cato to study matters which had been entirely ignored by his Roman predecessors; and he was far more thorough, and covered a far larger area, than the Greek writers such as Timaeus who had dealt with the history of Italy. Cato's methods of research, a great deal of his subject matter, and, indeed, his view of Roman history in general, were entirely new.

It is obviously necessary to preface any examination of the fragments of a lost work of literature with a general discussion of what it must have been like in its original form. In the case of Cato's Origines, the question of formal structure is of particular importance, for three main reasons:- first, the work's manifest peculiarities in this respect constitute one of its most revolutionary features; secondly, there has been a great deal of scholarly controversy on the question of the form of the Origines; and thirdly, many other problems which deserve attention, such as the method of dating followed by Cato, his attitude to the records of the pontifices, the date of the work's composition, its curious title, etc., are all to a greater or lesser extent dependent on the one fundamental problem of its formal structure.

(ii) The testimony of Cornelius Nepos

If our evidence consisted solely of the extant fragments and of a handful of testimonia of the kind given us in passing remarks by writers such as Cicero, Gellius and Fronto, we should, perhaps, be able to infer only that Cato's Origines was planned on unconventional lines; but we should probably be unable to obtain any precise idea of the scope and content of the various books, or of the main groupings of material etc. But by a piece of good fortune we happen to possess a description of the Origines which, despite its brevity and numerous inadequacies, contains a table of contents as well as some useful comments of a more general nature. The passage occurs in Cornelius Nepos' "Life of Cato" (3.3):-

"senex historias scribere instituit. earum sunt libri VII. primus continet res gestas regum populi Romani, secundus et tertius unde quaeque civitas orta sit Italica, ob quam rem omnes Origines videtur appellasse; in quarto autem bellum Poenicum est primum, in quinto secundum. atque haec omnia capitulatim sunt dicta. reliquaque bella pari modo persecutus est usque ad praetoram Ser. Galbae, qui diripuit Lusitanos: atque horum bellorum duces non nominavit, sed sine nominibus res notavit. in eisdem exposuit, quae in Italia

Hispanisque aut fierent aut viderentur admiranda; in quibus multa industria et diligentia comparet, nulla doctrina".

In view of the number of scholarly hypotheses that have been constructed solely on the basis of this passage, it seems to me that a detailed discussion of its reliability is required. Nepos' description of the Origines, although brief in itself, is out of all proportion to the rest of the "Life" in its detail. This is because the short "Life of Cato" which has come down to us is an excerpt from the book entitled "De Latinis Historicis". (9) Thus, in the "Life" we possess, Nepos was writing about Cato primarily as a historian. (10) It is therefore likely that he had at least some reliable knowledge of what the Origines contained. (11)

In any case, even if Nepos took his information about the Origines from an intermediary source such as Varro, his resumé can often be shown to be correct when it is checked against independent evidence.

But Nepos is not to be relied on too heavily if his statements are unsupported. In one particular he can be shown to be inaccurate: he tells us that the fourth book contained the First Punic War, the fifth book the Second. But the evidence of the fragments tells a different story. There are three fragments

of the Origines which can with certainty be related to the Second Punic War. One of them concerns the violation by the Carthaginians of the treaty with Rome at the outbreak of the war;⁽¹²⁾ the other two both relate to the conversations between Hannibal and Maharbal after the Battle of Cannae.⁽¹³⁾ But, although they refer to events of the Second Punic War, these fragments come from the fourth, not the fifth, book of the Origines.⁽¹⁴⁾

The discrepancy between the fragments and the statements of Nepos is increased still further when we turn to Book 5; here the evidence is for the most part uncertain, but one fact is indisputable; the speech which Cato delivered on behalf of the Rhodians in 167 B.C. was inserted into the fifth book. This is attested by two reliable and mutually independent witnesses, Gellius and Livy.⁽¹⁵⁾ If we reject ingenious attempts to save Nepos' reputation by the suggestion that Livy and Gellius were both using the same intermediary source in which the wrong book-number was given (!),⁽¹⁶⁾ we must assume that Nepos' description of the books has gone seriously astray. It must be admitted that the fourth book contained not only the First Punic War but at least some of the Second as well; and even if book 5 included the latter part of the Second Punic War, its contents were certainly not restricted to the events of that war alone. Whatever

the explanation of these facts might be, it cannot be denied that Nepos' description of the scope of the fourth and fifth books, even if not entirely wrong, is none the less somewhat inadequate.⁽¹⁷⁾

Many of Nepos' statements, however, can be vindicated. It is certain that the Origines contained seven books, in view of Cicero's evidence in the Cato Maior where the elderly Cato is presented as saying that he is engaged on the seventh book of his Origines⁽¹⁸⁾ - the dramatic date of the dialogue is 150 B.C., at the very end of Cato's life. Again Cicero tells us that Cato inserted his speech against Galba into the Origines "a few days or months before he died",⁽¹⁹⁾ and we learn from Gellius that the speech "contra Galbam" was recounted in the seventh book of the Origines.⁽²⁰⁾ The seventh book was obviously the last. This evidence also confirms Nepos on ^{to} other points: first that the Origines extended as far as the praetorship of Servius Galba, and secondly that Cato wrote his histories as an old man. (On the precise date of the composition of the Origines, see below, p. 29 ff.)

Nepos says that the first book contained the "res gestae" of the kings of Rome: the fragments that can be assigned on independent evidence to book 1 concern the foundation legend (F.15), and events of the reigns of

Tullus Hostilius (F.22), Servius Tullius (F.23), and Tarquinius Superbus (F.24). This seems to bear out Nepos' description. Similarly, the definitely assigned fragments of the second and third books support the view of Nepos that those books treated the subject "unde quaeque civitas orta sit Italica". His opinion that the content of these two books caused the work to be called "Origines" was shared, it seems, by Festus, who pointed out that the title was only appropriate to a small part of the work. (21)

The curious remark that Cato did not name the commanders in the wars recounted in the Origines is supported by the testimony of Pliny, and by the fragments themselves. (22) The fragments also confirm the statements that the Origines contained "quae in Italia Hispanisque aut fierent aut viderentur admiranda". (23)

It seems that Nepos' description of the work's peculiar characteristics is sound enough in general, but that he is less reliable when dealing with the precise contents of the individual books. His account of the first three books is probably acceptable as far as it goes, but he gives a very unhappy resumé of the last four. We have seen that he is seriously at fault on the content of books 4 and 5, and on 6 and 7 he has

nothing precise to say at all.

All this suggests that Nepos had some reliable information about the Origines, but that his analysis of the contents is not based on a careful reading of the whole text at first hand. If he had read any of it for himself, it seems likely that he perused the earlier books more thoroughly than the latter - which perhaps reveals something about Nepos' methods in general.

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(iii) The omission of the early history of the Republic

The elements of Nepos' summary which can be substantiated by independent testimony, together with the evidence of the relevant fragments, give us a general picture of the form and content of the Origines which can be used as a starting point for further discussion. Prima facie, the evidence we have been looking at suggests the following scheme:

- Book I: Foundation of Rome and monarchic period.
- Book II-III: Origins of the cities of Italy.
- Book IV-VII: Roman history from c.264-150 B.C.

As it stands, however, this is an extremely curious picture which gives rise to a lot of problems.

We may begin by recalling once more the peculiar character of the second and third books. By extending the field of historical study to include the communities

of non-Roman Italy, these two books mark an entirely new departure in Roman historiography; moreover, it appears from the fragments that the material contained in them was of a different kind from that handled in the other parts of the work.

We find ourselves faced here with two fundamental problems: first, what do the two books on Italian origins imply about Cato's general view of Roman history? And secondly, how do these books fit into the formal structure of the work as a whole? These two problems are, of course, interdependent, in the sense that the answer to the first is likely to emerge from a discussion of the second. Moreover, they are both in their turn dependent on yet a third problem, which can be formulated as follows:

The other books of the Origines contain what we may, for the present, call the narrative history of Rome; it begins, conventionally enough, at the beginning - with the origins of the city in Book 1 - and ends with events that occurred at the end of Cato's life in the seventh and last book. The books about Italian origins obtrude upon this narrative, but it could hardly be said that, if they were removed, we should be left with a continuous account of Roman history from the origins of the city down to the middle of the second century B.C. The prima facie indications of the evidence are that

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the first book ended with the downfall of the monarchy, that the narrative history began again in the fourth book with the outbreak of the First Punic War, and that the intervening period, comprising the first two and a half centuries of the Republic, was omitted.

It would be extremely surprising if Cato did omit to recount the history of this period, not only because of the consequent disruption of his account, but also because he might have been expected to dwell on the achievements of the heroic Romans in those early days of the young Republic.

Cato is, of course, presented by tradition as a reactionary figure who idealised the discipline of the past; we need only recall the story of the young Cato's visit to the nearby house of M' Curius Dentatus; being much impressed, we are told, with the humility of the house, he attempted to model himself on its former owner. (24)

His evident admiration for the harsh way of life of the primitive Romans, together with the didactic character of all his literary works, gives one the impression that a historical work by Cato ought to have been written with the explicit purpose of recommending to his contemporaries the discipline of earlier times. (25)

Plutarch tells us that when his elder son was learning to read, Cato composed for him, in his own hand

and in large letters, an elementary handbook of Roman history,⁽²⁶⁾ and it is usually supposed that when he came to write the Origines his primary intention was once again to instruct by example, this time addressing himself to his fellow citizens.⁽²⁷⁾ Much of what we read in the fragments of the Origines confirms this impression - for example the story of the military tribune in the First Punic War who sacrificed himself and a handful of followers so that a Roman army might be saved,⁽²⁸⁾ and above all the preface of the work, in which the utility of history was affirmed (F.3).

One might have thought that the greatest scope for historical "exempla" would have been provided by an account of the struggles of the old Romans in the early centuries of the Republic. Cato's total omission of the period, if indeed he did omit it, is in need of some explanation.

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(iv) Some earlier views

Some commentators have been extremely reluctant to believe that Cato could have left out the history of the early Republic, and have consequently been glad to seize upon the fact that the main theme of the period in question, as it is presented in the received tradition,

is the story of Rome's struggle for domination in Italy. This has led them to conclude that the two books on Italian origins also incorporated an account of the wars between Rome and the peoples of Italy, and of the subjection of the latter to the former. That the account should have concentrated entirely on external wars to the exclusion of the internal history of the city would not be a major difficulty - on the contrary, this would be entirely consistent with the fact that in the later books too, Cato seems to have dealt chiefly (or exclusively) with Rome's wars - a conclusion supported by Nepos, who speaks only of "bella"; the fragments of the later books do not contradict the impression given by Nepos, because they all deal with external military affairs, or with events in Rome directly related to external affairs, such as Cato's speech Pro Rhodiensibus, or the trial of Servius Galba. (29)

The whole work would thus have been a complete account of Roman military history, down to Cato's own day, coupled with a section on the origins of Rome (in Book 1) and of the Italian peoples whom she had conquered (in Books 2 and 3). And in the later books we find what could be signs of a similar format: in the fourth book there are fragments on the constitution of Carthage (F.80), on Punic "mapalia" (F.78) etc., which suggest

that Cato prefaced his account of the Punic Wars with a constitutional and ethnographical description of Carthage, which at least one scholar has ventured to call the "origo" of Carthage.⁽³⁰⁾ Further evidence suggests that Cato did the same for places in Illyria and Spain.⁽³¹⁾ Cato's theme will therefore have been twofold: the "res gestae" of the Roman people (cf.F.1), and the "origines" of the peoples whom they fought against.

There are two possible ways in which Cato could have combined a narrative account of military history with a descriptive account of origins. It is conceivable that the whole work was constructed around a chronological account of Roman history interspersed with digressions on the origins of the various peoples as and when each appeared for the first time in the story. The evidence of the numbered fragments of the second and third books does not necessarily preclude the possibility of a chronological arrangement; A. von Gutschmid thought that this would be more likely than a geographical order: "viel natürlicher ist die Annahme, dass von den Ligurern (Bk.2) bei Gelegenheit des Einbrechens der Gallier in Oberitalien, die sich auf Kosten derselben ausbreiteten, von Rhegium (Bk.3) bei Gelegenheit der Einnahme durch die Römer 271 die Rede war."⁽³²⁾

Not but what a more systematic type of arrangement would seem to be far more likely: one suggestion is that the Origines was divided up into geographical sections, in each of which Cato described the origins of the inhabitants,⁽³³⁾ their way of life,⁽³⁴⁾ the extent of their territory,⁽³⁵⁾ the agricultural produce of the area,⁽³⁶⁾ and various other matters, including admiranda.⁽³⁷⁾ This would serve as a background for their subsequent history, including their dealings with the Romans, down to the time when they were finally conquered by Rome. The second and third books will therefore have contained a complete history of the wars between the overthrow of the kings and the beginning of the struggle with Carthage, but arranged in a geographical, not chronological, order.⁽³⁸⁾ Attempts have been made to extend these broad geographical divisions to cover the whole work. A. Bormann suggested the following scheme:-

Book I:	Rome and Latium.
Book II:	Northern Italy.
Book III:	Southern Italy.
Book IV:	Sicily (and Corsica and Sardinia?).
Book V & VI:	Illyria, Macedon, Greece and the East.
Book VII:	Spain. ⁽³⁹⁾

Within these geographical sections the historical narrative would be chronological, and would treat the

activities of the Romans in the area concerned; if Spain formed the subject of the seventh book, we are provided with a neat explanation of the last-minute inclusion of the trial of Servius Galba in the book. Moreover, Bormann was able to point to an obviously parallel arrangement in the Historiae Philippicae of Pompeius Trogus, a universal history divided into geographical sections and incorporating "origo digressions". The title that precedes the extant prologues of Trogus is "Liber Historiarum Philippicarum et totius mundi Origines et terrae situs". (40)

A rather more subtle reconstruction, but one which is based similarly on the idea that the Origines was arranged in systematic sections according to subject matter (a method described by Ephorus as $\kappa\alpha\tau\grave{\alpha}$ γένος (41)), keeps more closely to the implied suggestion of Nepos that the main theme of the Origines was "bella"; since the Romans were often, especially in their more recent history, engaged in warfare in separate and independent areas of the Mediterranean world at one and the same time, it is suggested that Cato could have treated the wars in each theatre separately and in turn. This method also is not unparalleled: both Appian and Procopius dealt with Roman history in this way.

Indeed, Niebuhr believed that the arrangement of the early books of Appian's Romaika was directly inspired by Cato's Origines.⁽⁴²⁾ Certainly there is a striking similarity in the division of the subject matter in the first three books of each work. Appian subtitled his first three books "Ῥωμαϊκῶν Βασιλική", "Ῥωμαϊκῶν Ἰταλική", and "Ῥωμαϊκῶν Σαυνιτικῆ" (App. Praef. 14). But as far as we can see, these books were not at all concerned with origins; nothing in the surviving fragments of Appian looks remotely like the sort of material we find in the second and third books of Cato.

There are numerous suggestions concerning the possible arrangement of the later books: Gutschmid, for example, tabulated their contents as follows:-

- Book 4: The two Punic Wars.
- Book 5: The Macedonian Wars.
- Book 6: The War against Antiochus and related events in the East.
- Book 7: The Spanish Wars.⁽⁴³⁾

The hypotheses that have been discussed here share in common a desire to introduce a measure of 'normality' into a situation which, it is felt, would otherwise be impossibly idiosyncratic. The more moderate expressions of this tendency argue merely that some sort of account of Roman history between the end of the Monarchy and the

First Punic War must have been included somehow in the second and third books: but this in itself implies a reluctance to accept (1) that the material contained in those books did not conform to the general pattern of the rest, and (2) that Cato may have omitted the period altogether.

Leaving aside for the present the question of whether the later books of the Origines were arranged chronologically or according to subject-matter, we may confine ourselves to the basic problem of whether or not the second and third books included an account of Roman history during the early Republic as well as a description of Italian origins. There is no concrete evidence for the view that they did contain such an account; on the contrary, the evidence that is available has had to be forced to conform to a priori assumptions. The statement of Cornelius Nepos that the books treated "unde quaeque civitas orta sit Italica" has had to be ignored⁽⁴⁴⁾ as well as the fact that, of the 46 fragments of the second and third books in Peter's collection, not a single one can be said to deal with events of Roman history during the period of the early Republic. Moreover, a combination of historical narrative and a description of the origins of all the peoples of Italy would surely be unnecessarily cumbersome, however arranged. There is

no positive evidence to suggest a composition of this kind, and it must be said that the whole hypothesis rests ultimately on the supposed impossibility of the alternative.⁽⁴⁵⁾

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(v) The 'separatist' theory

Even if Cato did give a narrative account of events of early Republican history in the second and third books of the Origines, he must have done so very briefly; it can hardly be denied, in the face of the evidence of a relatively large number of fragments, together with the confirmatory statement of Nepos, that the origins of the Italian cities was the main topic of these books, and that most space was devoted to this theme. But although the material contained in books 2 and 3 appears to separate them from the rest of the work as a special section on their own, it does not require a great effort of imagination to see that this section on origins could also be made to include the first book, which, in addition to the "res gestae" of the kings of Rome, also contained the origins of the city.

Most scholars in recent times have recognised the structural unity of the first three books, on the subject of the origins of all the cities of Italy including Rome. For example, R. Helm writes:

"Er verfolgte die einzelnen italischen Gemeinden und beschrieb sie mit ihren Entstehungslegenden An die Spitze kam natürlich Rom im ersten Buch, dann schlossen sich in zwei weiteren die anderen Städte Italiens an". (46)

Viewed thus, the work falls neatly into two parts - three books of "Origines", four books of "bella". The inappropriateness of the title, noted by Festus and Nepos, would thus be due specifically to the fact that the section on wars was larger than that on origins. The absence of any account of the archaic republic, to connect the narrative of Roman history in book 1 with that in 4-7, appears to accentuate the division of the work into two parts.

Some scholars have been tempted to infer from this that the Origines was not in fact a single work at all, but rather a compilation of two separate and originally independent works, and that Cato himself had never intended that they should be combined into one. This view is built up on a reconstruction of Cato's activities as a historian which can be outlined briefly as follows:-

At about the time of the war against Perseus (171-168 B.C.), Cato was writing an account of the origins of Rome and the other Italian communities, an account which occupied three books (Books 1-3 of the "bipartite

corpus"). He gave these three books the characteristic title "Origines". Some years later, at the very end of his life, Cato again began to write history - this time a history of events from the outbreak of the first war with Carthage down to his own day. He wrote four books on this subject, and was still engaged on the fourth at the time of his death. When he died, this second work remained unfinished, but was published posthumously by an editor, perhaps one of Cato's own grandchildren, who combined it with the earlier work, and wrongly applied that work's title to the whole compilation. In this form, a seven-book compilation under the title "Origines", Cato's historiographical efforts were transmitted to posterity. (47)

This radical hypothesis requires careful examination. Once again we are dealing with a thesis which has the advantage of eliminating altogether the problem of the missing history of the early Republic, but which has no direct evidence to support it. The evidence on which it is based is entirely circumstantial; of the arguments that have been adduced in its favour the majority seem to me to support a rather different conclusion. But there is one contention that requires full discussion, and it concerns the date or dates of the work's composition.

If it can be proved that Cato was writing the last

four books of the Origines some fifteen years or so after completing the first three, there is clearly a strong argument in favour of a double composition. We must therefore examine all the existing indications of when Cato was writing.

(vi) The date of the Origines

We know that the Origines was the work of Cato's old age. Nepos says "senex historias scribere instituit", and this is in any case what one would expect from a man who was prominent in public affairs for most of his life. We know also that the seventh book of the Origines contained an account of the events which occurred at the very end of Cato's life, including the trial of Servius Galba (inserted shortly before his death - Cic. Brutus, 89). In the Cato Maior de Senectute Cato is quoted as saying "septimus mihi liber Originum est in manibus".

It seems very unlikely that Cicero had any direct external evidence that Cato was working on the seventh book at the time when the dialogue de Senectute was set (150 B.C.). The remark he attributes to Cato here is clearly based on an inference from the fact that he inserted his speech contra Galbam into that book. (48)

But the inference seems justified, and we too may conclude that Cato was working on the last books of the Origines at the end of his life.⁽⁴⁹⁾

The evidence for the date of the earlier books consists of one fragment only:-

"Ameriam supra scriptam Cato ante Persei bellum conditam annis DCCCCLIII prodit" (F.49 = Plin.n.h.3.114).

We do not know which book of the Origines this fragment is taken from, but as it is concerned with the foundation of an Italian city it seems reasonable to suppose that it is from the second or third book. It is clear that the fragment gives us a "terminus post quem" of either the beginning (172-1 B.C.), or the end (168 B.C.), of the Third Macedonian War for the composition of this particular part of the Origines.

But many scholars have gone on to conclude from F.49 that Cato was writing the second (or third) book of the Origines at the time of the war with Perseus, or at any rate very soon thereafter.⁽⁵⁰⁾ If this were so, it would imply that a considerable interval elapsed between the composition of the earlier and later books. But this interpretation of F.49 will not bear examination. To my knowledge, only one scholar has been moved to justify in precise terms the assumption that F.49 gives a near absolute date; R. Helm writes:-

"Die beiden historischen Werke (sic) liegen zeitlich auseinander. Das zweite Buch ist, wie Frg. 49 zeigt, zur Zeit des makedonischen Krieges mit Perseus (171-168) geschrieben, da die Zeitbestimmung der Gründung Amerias durch den Abstand von diesem Kriege nur Sinn hatte, wenn dies Ereignis für den Leser in unmittelbarer zeitlicher Nahe lag."⁽⁵¹⁾

But this statement is simply untrue. As it stands Cato's date for the foundation of Ameria is perfectly comprehensible to the modern reader, and, what is more to the point, it evidently made sense to the elder Pliny, writing more than two centuries after the Third Macedonian War. And presumably Pliny thought that it would make sense to his readers, because his purpose in citing Cato was to give them some idea of the date of the foundation of Ameria. It is clearly mistaken to suggest, as Helm does, that a date fixed in relation to some great event only has meaning for a person living in close proximity to that event. Cato's date for the foundation of Ameria will have been understood by any person of Pliny's time (or any other time) who could calculate the chronological relationship between the war with Perseus and the time in which he himself was living; and this would not have been difficult, in view of the historical importance of the war.

I believe that Cato used the Third Macedonian War as a terminus for precisely this reason, that his date should be more widely understood than if he had used a consul date or some more ephemeral event which could not be fixed chronologically in the reader's mind as easily as a famous battle or war.

Cato did not use consular years as a method of dating; the use of such a system requires the reader to refer to a regular list, because dating by eponyms cannot easily indicate duration of time or relative chronology. This difficulty can be alleviated to some extent by an annalistic arrangement, with the events of each year being enumerated under the heading of the names of the eponymous officials of the year in question; but Cato did not arrange his material annalistically. I intend to justify this statement in due course, but I state it as a fact for the time being. The evidence of the fragments suggests, rather, that Cato dated events by stating the distance in years before or after some epochal event.⁽⁵²⁾

In the case of F.49 it is likely that he chose the most recent event of world-wide importance: but it is clear also that the Third Macedonian War remained "the most recent great event", at least in international terms, until the outbreak of the third war against

Carthage. No event in the last decades of Cato's life would have made the same impact on later readers as the war against Perseus, and it is therefore reasonable to suppose that he could have written F.49 at any time between that war and his death. (53)

F.49, then, gives us only a "terminus post quem" for the composition of the part of the Origines from which it is taken. Even if it could be shown that Cato had begun to work on the second and third books already in the 160's, there is no way of telling whether he then went on to compose the later books of the Origines slowly but fairly continuously from then until his death, or whether he broke off at some point and began again after a long period of time elapsed - and even if the second of these suggestions be the truth, there could still be no certainty about exactly where the break occurred. (54)

Two further points may be mentioned here. First, Cicero says that Cato was working on the Origines very shortly before his death, and this perhaps indicates that the work was not finished. It has also been suggested that the trial of Servius Galba, the last event recorded in the Origines, was not a very suitable point at which to end a historical work, and this suggests that Cato left it unfinished at his death. (55) This is perfectly

possible, but even if the Origines was published posthumously, this in itself would not constitute evidence for the theory that it was a compilation of originally separate works. Secondly, we have seen that F.49 gives a "terminus post quem" of 171-168 B.C. for the second, or third, book of the Origines; but there are two pieces of evidence which suggest, on the contrary, that Cato had produced a historical work before that date.

Livy (34.5.7) tells a story in which a Tribune of the Plebs, L. Valerius, makes a speech against Cato; in it he purports to quote from the "Origines", in order to attack the author with his own words - "tuas adversus te Origines revolvam..." etc. The date of the incident is 195 B.C., the year of Cato's consulship. But it is quite clear that this is a speech created entirely by Livy, who has failed to see the chronological impossibility of what he is saying - for it is quite impossible that the Origines had been written and published in 195 B.C., in view of Cato's own evidence in F.49. It is extremely doubtful, moreover, whether the tribune's words bear any resemblance whatever to what was contained in the Origines. (56)

The second piece of evidence is more serious. In his biography of Cato, Plutarch describes the care which Cato took in the education of his son, and among

illustrations of this he gives the following information:-

"καὶ τὰς ἱστορίας δὲ συγγράψαι φησὶν αὐτὸς ἰδίᾳ χερσὶ καὶ μεγάλῃς γράμμασιν, ὅπως οἴκοθεν ὑπάρχοι τῷ παιδί πρὸς ἐμπειρίαν τῶν παλαιῶν καὶ πατρῶν ἰφελήσθαι." (Plutarch, Cato Maior 20).

Cato's son was born probably about 192 B.C. (57)

If the boy was learning to read when Cato wrote these "ἱστορίαε" (and this must be the implication of "μεγάλῃς γράμμασιν"), we must assume that they were composed some time during the 180's. If "ἱστορίαε" is supposed to mean the Origines (Plutarch certainly uses the word to signify the Origines in ch.25 of his "Life of Cato") then there is a problem of chronology, because, as we have seen, the other evidence points to a date after the third Macedonian War. This is odd because Plutarch claims to be basing his statement on the authority of Cato himself ("φησὶν αὐτός"), and neither Cato nor Plutarch was a deliberate liar. There are two possible solutions to this problem:

(1) Cato wrote a portion of the Origines in 180's for the benefit of his son, but wrote the rest of the work later in his life. E. Badian has lent his authority to this view, stating that "Book I (the early history of Rome, self-contained and most suitable

for Cato's purpose) was written first, and for this purpose, in the 180's, and the rest added later".⁽⁵⁸⁾

A possible objection to this view is that the Origines was a serious work of scholarship, and not an elementary primer, which would have been more suitable for a boy learning to read.⁽⁵⁹⁾ Moreover, there is no immediately obvious reason for thinking that book 1 was "self-contained and most suitable for Cato's purpose".

(2) More plausible perhaps is the theory that Plutarch misunderstood his source, if by "*ιστορίαι*" he meant the Origines, and that the history which Cato wrote for his son was a different work altogether.⁽⁶⁰⁾

It seems more likely that Cato wrote for his son a reader of Roman history, which was not meant for publication, and which was entirely independent of the Origines. Of course, it may well be that Cato's interest in history developed as a result of the work which he wrote for his son; in the same way the De Agri Cultura and the De Medicina were serious works on topics which he had previously treated in a more elementary fashion in the Libri ad Filium.⁽⁶¹⁾ Plutarch's statement does not, therefore, prove anything about the date of the composition of the Origines.

There is no other evidence, either external or internal, that might bear on this question; all in all,

it seems fair to say that what we know of the date of the Origines is inconclusive, and tells us nothing about its structure. There is certainly no evidence to suggest that several years elapsed between the completion of the first three books and the composition of the last four.

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(vii) The title: "Origines"

A second argument that has been advanced in support of the theory of a double composition concerns the title of the work. Most scholars today are agreed that the title Origines applies only to the first three books. That it was at any rate not very suitable was already acknowledged by the ancients themselves (cf. above p. 15 and n. 21). But in spite of R. Meister's assertion "Wie schon die Alten beachtet haben, trifft dieser Titel nur auf die ersten drei Bücher zu",⁽⁶²⁾ it is worth pointing out that no ancient writer said anything so precise; Cornelius Nepos says that the work was called Origines because of the content of the second and third books; he does not say that it applies 'only to the first three books'. The sense of the Latin seems clear enough: "primus (liber) continet res gestas regum populi Romani; secundus et tertius unde quaeque civitas

orta sit Italica, ob quam rem omnes Origines videtur appellasse". Verrius Flaccus contrasted "origines" with "res gestae" (see n.21), and yet we have the explicit statement of Nepos that the first book contained "res gestae" (cf. F.1).

In addition it must be said that even if the title was suitable only to the first three books, this would not in itself justify the inference that books 4 to 7 did not originally belong to the same work, or that Cato, had he lived long enough, would have published them separately and under a different title.⁽⁶³⁾ In fact the assumption is entirely arbitrary; moreover, it is open to the objection that a later editor would have been unlikely to do such violence to Cato's literary legacy, and that if he had come across two separate works he would have published them as such. One cannot see what would have been gained by combining them into one.

Finally, and this seems conclusive, the notion of two separate works "die ursprünglich nichts miteinander zu tun hatten"⁽⁶⁴⁾ is entirely unnecessary; it is not the only solution of the problem of the title Origines, nor is it the most likely. R. Meister claimed that "die neuere Forschung nimmt daher, und zwar ziemlich übereinstimmend als wahrscheinlich an, dass Cato zwei

nach Konzeption und Abfassungszeit verschiedene historische Werke geschrieben hat" (art.cit. pp.2-3). In fact this statement is untrue; to my knowledge the only modern works in which this eccentric notion is expressed are those enumerated in note 47 (above), and they are by no means a majority.

Admittedly the most widely held opinion on this matter is prima facie very similar to the extreme position of Schanz, Rosenberg, Helm and Meister, but a brief analysis will show that it is in fact totally distinct in one all important respect.

The "traditional view" argues simply that the work developed during the course of its composition, and that it was published gradually and in stages; thus, while Cato began it as a work on origins (and named it accordingly), he did not adhere to his primitive design, but continued it with an account of more recent history down to his own time. There seems little doubt, as we have seen, that Cato was still engaged on the last book at the time of his death, so that at least the last portion of the work will have had to be published posthumously.

Seemingly unexceptionable as it stands, this general proposition is normally interpreted to mean something very similar to the view that the Origines was a compilation of two separate works, because it is assumed

that the gradual publication of the work occurred in two stages; that the first three books originally stood alone under the title Origines, and that the later books formed a continuation of it which had not been part of the original plan; the title, Origines, remained for the second, enlarged, edition in seven books, because it was not finished at the time of Cato's death and had to be published afterwards.⁽⁶⁵⁾

This reconstruction is seriously weakened by the fact that, like the hypothesis of two separate works, it too is based to a great extent on the assumption that the first three books were written considerably earlier than the last four - an assumption which we have seen to be unfounded. There is in fact no evidence that books 1-3 were completed and published first, and the rest added later; it is sometimes said that Cato's famous statement about the tablet of the Pontifex Maximus (F.77), which occurred in book 4, formed part of a new preface at the start of that book and introducing the second, additional, part of the work; this is possible, but F.77 is not to be used as evidence for a composition in two stages. E. Badian's remark "book IV has a new preface, indicating later publication "⁽⁶⁶⁾ is of course circular. There is nothing, apart from the preconceived opinions of scholars, to suggest that F.77 comes from

the beginning of book 4: we know nothing whatever about its context. Of course, if there were independent proof that books 4 to 7 were a later addition, one could argue that F.77 formed part of a new preface; but there is no reason why F.77 on its own should be indicative of a double composition.

It must be recognised that there is no unequivocal external evidence that might tell us how Cato came to compose his Origines. We do not know to what extent its final shape was determined by such factors as the date(s) of composition and publication⁽⁶⁷⁾ of the various sections of the work, or even whether it was composed in stages at all.

The arguments based on internal evidence are equally uncertain. The fact that the material contained in books 1-3 appears to be different in character from that in the later books is not necessarily a sign that only the first three books were included in Cato's original plan. F. Leo argued that the first three books had "einen einheitlichen Plan und eine geschlossene Komposition", and that "die Anlage des 2 und 3 Buchs deutet nicht auf den Plan einer fortgehenden Erzählung",⁽⁶⁸⁾ but this seems excessively dogmatic. It is mistaken to assume that distinct groups of books, which can be differentiated on internal grounds, necessarily coincide with well defined stages in composition and publication; it is surely perfectly possible

that Cato's original plan, if it is right to speak of such a thing, could have incorporated the subject matter of the later books. After all, the combination of "Origines" and "bella" in a single historical work would not be so unusual, as e.g., E. Norden pointed out: "Diese Verbindung von Ethnographie und Kriegen war in der griechischen Historiographie seit Herodot ganz gewöhnlich".⁽⁶⁹⁾

The title of the work is admittedly rather odd, and it may very well be that the final product did not precisely accord with Cato's original intentions when he first started out, and when he first formulated the title Origines. But this is not to admit the need for an artificial explanation: it seems to be generally assumed that if Cato himself had edited and published the complete work of seven books, he would not have allowed the title Origines to remain. But this conclusion is surely arbitrary - and the traditional explanation, that the work was published in two stages, of which only the first (books 1-3) was edited by Cato himself, seems unnecessarily contrived, and is entirely unsupported by the available evidence.

The Origines of Cato has some distinguished parallels, for example the Cyropaedia and Anabasis of Xenophon, in each of which the title strictly applies only to the

first part of the work.⁽⁷⁰⁾ The reason for this curious feature is a matter for conjecture. As for Cato's work, he may have called it Origines simply because the part that dealt with Italian origins was the most distinctive feature of it - this seems to be the implication of Nepos' remarks on the subject. But whatever view one takes of this problem, there is no reason to deny that the application of the title to the whole work of seven books could go back to the author himself; there is nothing incredible in the situation imagined by Cicero, in which Cato is made to say:

"Septimus mihi liber Originum est in manibus".⁽⁷¹⁾

(viii) Conclusion: The unity of the Origines

We conclude, therefore, that the seven books of the Origines formed a single composition and were published as one work in accordance with the intentions of the author.⁽⁷²⁾ If this much is admitted, it follows as a consequence that, however much the work can be subdivided into smaller sections, and however abrupt the divisions between the sections may be, there must nonetheless be some sort of underlying unity to bind them

together. It was for this reason that I saw fit to distinguish carefully between the hypothesis of Rosenberg et al. on the one hand, and that of Leo and his school on the other. Adherents of the former theory (that the Origines was the result of the combination of two entirely independent works, brought together and published after Cato's death) assert that it would be a fundamental error to search for elements of continuity in the surviving fragments of "Cato's Origines". The latter view, on the other hand, even in its most extreme form, necessarily implies that Cato himself intended books 4-7 to be a continuation of books 1-3, and that there must be some connecting thread, however slight, between the two parts.

If the later books were a deliberate continuation of what went before, one is bound to assume that the narrative contained in them was intended to follow on logically from the material included in the first three books; however much one separates the composition of the two halves of the work, it cannot be denied that books 1-3, in terms of their formal function, constitute a preliminary to the later narrative. Unless the two parts are separated completely, we cannot altogether evade the question of why Cato's narrative history of Rome broke off at the end of the monarchic period and apparently began again

only with the outbreak of the Punic Wars. (73)

It seems to me that a more satisfactory approach to the whole problem would be to look for possible reasons why Cato should have omitted the history of the early Republic altogether, and replaced it with an account of the origins of the communities of non-Roman Italy; because, after all, the evidence suggests that that is exactly what he did.

(ix) Notes to Chapter I

- (1) We possess about 145 fragments of the Origines, which is a very large number when compared with the 28 surviving fragments of Fabius Pictor, 40 of Cassius Hemina, 45 of Calpurnius Piso Frugi, 34 of Cn. Gellius and 67 of Coelius Antipater - to name only the best preserved of the Roman historians of the second century B.C. This must reflect to some extent the importance of the Origines in antiquity, and its influence on later writers.

References to fragments of the Origines in this thesis are all based on the standard edition of H. Peter, Historicorum Romanorum Reliquiae (abbr. HRR) Vol. I, 2 ed. Leipzig 1914; (It has now been reprinted, with a bibliographical appendix by J. Kroymann). Peter's introductory discussion (pp. CXXVII-CLXIV) is valuable, as is the prolegomenon of H. Jordan's M. Catonis praeter librum de re rustica quae extant, 1860, pp. XLIX-LXI. The dissertations of A. Wagener (M. Porcii Catonis originum fragmenta... Bonn 1849) and A. Bormann (M. Porcii Catonis libri septem... Brandenburg 1858) are still worth consulting. The fragments of the first book have recently been re-edited and published with exhaustive commentary by W. A. Schröder, M. Porcius Cato. Das erste Buch der Origines, "Beitr. z. klass. Phil." Bd.41, Meisenheim 1971.

The more important discussions of Cato's Origines include A. von Gutschmid, Catos Origines (1866), "Kleine Schriften" 5, 1894, 518-526; F. Leo, Geschichte der römischen Literatur, I. Berlin 1913, 290 ff.; A. Rosenberg, Einleitung und Quellenkunde zur römischen Geschichte, Berlin 1921, 163 ff.; M. Schanz, C. Hosius, Geschichte der römischen Literatur, I, 4 ed. Munich 1927, 186 ff.; F. Klingner, Cato Censorius und die Krisis Roms (1934), "Römische Geisteswelt", (4 ed. Munich, 1961), 34 ff; R. Helm, R.E. XXII, s.v. 'Porcius (no.9) Cato Censorius' (1953), 156 ff; R. Meister, Zu römischen Historikern 1: Der Titel von Catos Geschichtswerk, "Anzeiger oesterr. Akad. Wissens.", Phil. hist. Kl. 101, 1964, 1 ff;

D. Timpe, Le "Origini" di Catone e la storiografia latina, "Atti e Memorie Accad. Patavina Sc.Lett. ed. Arti" 83, 1970-71, 5 ff; of the discussions included in biographies of Cato we may mention in particular D. Kienast, Cato der Zensor: Seine Persönlichkeit und seine Zeit, Heidelberg 1954, esp. 103 ff; F. Della Corte, Catone Censore, La vita e la fortuna (1949), 2 ed. Florence 1969, esp. 77 ff; much important material is to be found in the numerous papers of P. Fraccaro, now collected in "Opuscula" I, Pavia 1956; N.B. also his article "Catone" in "Enciclopedia Italiana" IX (1931), esp. p.459. Discussions of the Origines are to be found in most of the standard histories of Rome - especially useful is that of G. De Sanctis, Storia dei Romani IV, 2.1, Florence 1953, 60 ff.

- (2) F.2 (= Cic. Pro Planc. 27.66). The sentiment seems to be borrowed from Xenophon. Cf. Peter's note ad.loc.
- (3) B.G. Niebuhr, Vorträge über röm. Geschichte, 1846, I.26, cited in R. Helm, R.E. 'Porcius', 157.
- (4) F.77; cf. below p. 64f, 97f.
- (5) Cf. especially the remarks of F. Altheim in Epochen der römischen Geschichte II, 1935, 309 ff., and in "Welt als Geschichte" 2, 1936, 81-3; on the place of Cato in early Roman historiography see also M. Gelzer, Der Anfang römischer Geschichtsschreibung (1933) in "Kleine Schriften" 3, 1964, 93 ff; and Nochmals über den Anfang röm. Geschichtsschr. (1954) in "Kl.Schr." 3, 104 ff. And the contrary view of F. Bömer, Thematik und Krise der röm. Geschichtsschr., "Historia" 2, 1953-4, 189 ff., who attempts to eliminate Gelzer's distinction between the early 'pragmatic' historiography of Fabius and Cato, and the later "Annalistik"; but B. goes much too far when he argues that Cato's Origines was a more or less routine example of Roman annalistic writing, and minimises the peculiarities of Cato's work. Cf. below Chapter II, note 38.
- (6) Cicero, Brutus 18.69; on the chronological relationship of Cato's Origines to Cassius Hemina's Annales see below Chapter II, note 3.

- (7) Gell. N.A. 11.8.2. (= Postumius Albinus F.1 Peter). Cf. Macrobian Sat. praef. 14, Polyb. 39.12., Plut. Cato Maior 12, id. Apophthegm. Cat. 29. On this matter see e.g. H. Bardon, La littérature latine inconnue I, Paris 1952, 70 ff.
- (8) Cf. E. Pais, Storia Critica di Roma I. 1. (Rome 1918) 92-3, and the important papers of F. Klingner, Italien..., and Cato Censorius... in "Römische Geisteswelt", esp. pp.18 ff.,59 ff.
- (9) At the end of the "Life of Cato" Nepos says that he has written a more extensive biography of Cato at the request of Atticus. The extant "Life" is probably a resumé of this larger work, with the addition of the chapter on the Origines.
- (10) Cf. F. Leo, Gr. - röm. Biographie, 212; G. Wissowa, R.E. s.v. 'Cornelius Nepos', 1412 ff; P. Fraccaro, "Opuscula" I, 143 ff.; M. Schanz - C. Hosius, Röm. Lit. (cit. note 1) I, 356 ff.
- (11) Cf. H. Jordan, Prologomenon (cit. note 1) XXII; H. Peter, HRR I. CXLIX: A.von Gutschmid, art.cit. (note 1) 519; D. Kienast Cato der Zensor p.14.
- (12) F.84 (= Non. s.v. duodevicesimo p.100).
- (13) F.86 (Gell. N.A. 10.247), F.87 (= Gell. N.A. 2.19.9).
- (14) F. 86 is not specifically stated to come from book 4, but it is obviously part of the same anecdote as F.87 (on the story in general see Coelius Antipater F.25 P., Liv. 22,51 etc.) which, according to Gellius was to be found "in quarto originum".
- (15) F.95 (= Gell. N.A. 6.3): cf. Liv. 45.25.3.
- (16) The intermediate source being a collection of Cato's speeches extracted from the Origines. See A. Bormann op.cit. (note 1) 40; H. Jordan prol. XXIII; contra, Gutschmid, art.cit. p.524, who argued that F.96 and 97 relate to the war against the Illyrian prince Gentius, in 168 B.C. Cf. Peter's note ad.loc.

- (17) Cf. A. von Gutschmid, art. cit., 519, 523; H. Peter, HRR. I CXXIV; F. Leo, Röm. Lit. 294-5; P. Fraccaro, "Opuscula" I, 195-6; F. Bömer, "Historia" 2, 1953-4, 196.
- (18) Cic. Cato Maior de Senectute 38: "Septimus mihi liber est in manibus".
- (19) Cic. Brutus, 89: "quam orationem in Origines suas rettulit, paucis antequam mortuus est an diebus an mensibus."
- (20) F.108 (= Gell. N.A. 13,24 (25),15)..
- (21) Festus s.v. "Originum" p. 216 L: "Originum libros quod inscripsit Cato non satis plenum titulum propositi sui videtur amplexus, quando praegravant ea, quae sunt rerum gestarum populi Romani".
- (22) F.88 (= Plin. n.h. 8,11)... "cum imperatorum nomina annalibus detraxerit..." cf. F.82,83,86, 87, etc.
- (23) e.g. F.33,39,52 (Italy), 93,110,113 cf. Artemidorus ap. Strabo 3.4.17 p.164 (Spain). N.B. also the sentence (not in Peter's edition) of the paradoxographicus Palatinus ch. 21 p.360 Giannini:- "κάτω φησὶν ἐν ταῖς κτίσεσιν, ἐπὶ τῶν Ἀδελφῶν Δούκους πέντε λαγύρους πίνεσθαι, μὲν δ' ἐνδεκαλίτρουσ. Ἔς δὲ πονοχῆλους καὶ κύνες δασεῖς καὶ βόας ἀκεράτους".
- (24) Plut. Cat. Mai. 2. 1-2; Cic. Cat. Mai. de Sen. 16. 55-6; id. de re publ. 28.40.
- (25) Cf. F. Leo, Röm. Lit. 290 f.; F. Klingner, Cato Censorius... "Röm. Geisteswelt", 58; G. De Sanctis, Storia dei Romani IV. 2.1 p.60.
- (26) Plutarch, Cato Maior 20; cf. below p. 34 ff.
- (27) Cf. F. Leo, Röm. Lit. p.290 f.; K. Barwick, "Würzburger Jahrbücher" 3, 1948, 128 f.; D. Kienast, Cato der Zensor, 107.
- (28) F.83; notice also the words attributed to Cato in Cic. Cat. Mai. de Sen. 20.75: "legiones nostras, quod scripsi in Originibus, in eum locum

saepe profectas alacri animo, unde se redituras numquam arbitrarentur".

- (29) Cf. P. Fraccaro, "Opuscula" I, 195; contra D. Timpe, Le "Origini" di Catone... (cit. n.1) p.26.
- (30) F. Klingner, Cato Censorius... "Röm. Geisteswelt" p.62.
- (31) F.96,97 (Illyria) F.93,94, cf. Strabo 3.4.18. p.165, 110, 113 cf. Artemidorus ap. Strabo 3.4.17 p.164 (Spain). Cf. R. Helm R.E. 'Porcius', 159.
- (32) A.von Gutschmid art cit. 520-1; C. Wachsmuth, Einleitung in das Studium der alten Geschichte, 1895, 625; cf. K. J. Beloch, Römische Geschichte, 1926, 102; contra, H. Peter HRR I. CXXXVIII; M. Schanz - C. Hosius, Röm. Lit. I.188.
- (33) e.g. F.31,37,40,41,42,45,48,50,51,53,54, 56,59,69,71.
- (34) e.g. F.33,34,50,51,61,73,76,78,79,80,71, 94,103,113.
- (35) e.g. F.38,50,68,71,96.
- (36) e.g. F.35,39,43,57,97,110.
- (37) e.g. F.33,39,46,52,93,97,110.
- (38) K. Nipperdey, Cornelius Nepos, comm. ad. loc: "An die Gründungssagen der einzelnen Städte und Völker schloss sich ihre spätere Geschichte, namentlich ihre Kämpfe mit den Römern an, so dass hier die ganze römische Kriegsgeschichte von der Königszeit bis auf den ersten punischen Krieg erzählte war, aber in geographischen, nicht in annalistischen Ordnung"; cf. A. Schwegler, Römische Geschichte I, 1853, 81; L. Pareti, Storia di Roma e del mondo romano I, 1952, 30: contra, F. Leo, Röm. Lit. 292.
- (39) A. Bormann, op.cit. LVII.
- (40) Cf. L. Pareti, Storia di Roma I, 30-1.
- (41) F. Gr. Hist. 70 T. 11 = Diodorus V.1.4.

- (42) Niebuhr, Römische Geschichte². I. 9n.2: "Diese Eintheilung ist offenbar das Vorbild, wonach Appian seine Geschichtsbücher angelegt, deren drei erste den namlichen Inhalt hatten"; cf. L. Pareti, loc.cit.; D. Timpe, Le "Origini" di Catone... 25-6.
- (43) A.von Gutschmid, art.cit. 525; cf. Schanz-Hosius, Röm. Lit. I. 187-9. The scheme suggested by Fraccaro, "Opuscula" I. 193 ff., will be discussed presently (Below, pp. 101-2).
- (44) Niebuhr, Röm. Gesch.². I.9 argued that Nepos gave a one-sided account of what was contained in books 2 and 3: because his primary purpose was to explain the title Origines, he exaggerated the role of the origin-stories. This seems unlikely. Cf. H. Jordan prol. XXIII.
- (45) e.g. C. Wachsmuth, Einleitung in das Studium... 625 n.2: "Gutschmid... die ganz richtige Ansicht von Niebuhr fester begründet und die unmögliche Annahme, Cato habe die ganze ältere republikanische Zeit - bis zum ersten punischen Krieg weggelassen, zurückweist".
- (46) R.E. 'Porcius', 157; cf. F. Leo, Röm. Lit. 292 ff; A. Rosenberg, Einleitung und Quellenkunde... 163; G. De Sanctis; Storia dei Romani, IV.2.1. 61; D. Kienast, Cato der Zensor 107-9; R. Meister, Zu römischen Historikern (cit.n.1). 1-8 etc.
- (47) A. Rosenberg op.cit. 163-9; M. Schanz - C. Hosius, Röm. Lit. 186-9; R. Helm, R.E. 'Porcius' 160-1; R. Meister, art.cit: contra, H. Peter, Wahrheit und Kunst, 1911, 283 n.2; cf. the remarks of P. Fraccaro Encicl. Ital. s.v. 'Catone', p.459, and F. Altheim, Epochen... II.310.
- (48) Cf. P. Fraccaro, "Opuscula" I. 141; D. Timpe Le "Origini" di Catone, 13-14.
- (49) It has been argued that F.84, dealing with the outbreak of the Second Punic War, is a tendentious statement which must have been influenced by the discussions which were going on in Rome in the late 150's B.C. about Rome's treaty relations with Carthage. If this is right, we can say that the last four books were written in the years between

- 154 and 149. See K. J. Neumann, "Hermes" 31, 1896, 528 f; cf. F. Leo, Röm. Lit. p.290; M. Gelzer Nasicas Widerspruch gegen die Zerstörung Karthagos, "Philologus" 86, 1931, 269; F. W. Walbank, A Historical Commentary on Polybius I, 1957, p.171 on Polyb.II.13.7; G. Nenci Il trattato romano - cartaginese... "Historia" 7, 1958, 267 ff; id., La testimonianza di Catone sulla "Decessio de foedere" saguntina, "Studi Annibalici" (Convegno 1961), Cortona 1964, 74 f.
- (50) W. Drumann, P. Groebe, Geschichte Roms, V, 155 n.13; H. Peter, HRR I, CXXXIX; A. von Gutschmid, art.cit. 518; A. Rosenberg Einleitung u.Quellenkunde... 164; F. Leo, Röm. Lit. 291; R. Meister, Zu römischen Historikern, 3; etc.
- (51) R. Helm, R.E. 'Porcius', 160.
- (52) Hence F.13,17,49,69,84. Cf. D. Kienast, Cato der Zensor, 109; D. Timpe, Le "Origini" di Catone... 12 n.29.
- (53) Cf. G. De Sanctis, Storia dei Romani IV,2.1., 62 n.140; U. Knoche, "Historia" I. 1950, 290; D. Timpe, Le "Origini" di Catone... p.11; L. Moretti (Le Origines di Catone, Timeo ed Eratostene, "Riv. Fil.Class". 80, 1952, 300 n.3.) follows E. Pais (Storia Critica di Roma I.1.87 n.2.) in thinking that the reference to the war with Perseus could have been inserted in a later version of a work that had been published before 171-0. This seems unlikely.
- (54) The arguments of Neumann (above, n.49), that the first three books were written soon after 168, and that the fourth was not begun until the late 150's, are obviously not compelling.
- (55) On the trial of Ser. Galba see N. Scriverletto, "Giorn. Ital. Filologia" 14, 1961, 63 ff.
- (56) Cf. H. Peter, HRR I, CXXIX; P. Fraccaro "Opuscula" I.122.
- (57) He saw his first military service in 173 B.C. under Popillius Laenas. See R.E. 'Porcius' (no.14) 167-8; H. Peter, HRR I. CXXIX - CXXX.

- (58) E. Badian The Early Historians, in "Latin Historians" (ed. T. A. Dorey, London 1966), p.31 n.44.
- (59) D. Timpe, Le "Origini" di Catone... p.11.
- (60) H. Peter, HRR I. CXXIX; K. Barwick, "Würzb. Jahrb." 3, 1948, 128.
- (61) Schanz-Hosius, Röm. Lit. I.184; K. Barwick, art.cit. 117 ff; D. Kienast, Cato der Zensor.107.
- (62) R. Meister, Zu römischen Historikern... p.2.
- (63) R. Meister (art.cit. p.8) even speculated about what the title of this second work might be.
- (64) R. Helm, R.E. 'Porcius', 160.
- (65) K. J. Neumann, "Hermes" 31, 1896, 528 f; F. Leo, Miscella Ciceroniana (1901), "Ausgewählte Kleine Schriften" I, 319 f; id., Röm. Lit. 294 ff; G. De Sanctis, Storia dei Romani, IV,2.1, 62 ff; E. Badian, The Early Historians 7 ff.
- (66) E. Badian, loc.cit; cf. D. Timpe, Le "Origini" di Catone... p.14-15; H. Peter, HRR I. CXXXIX.
- (67) It is obviously doubtful whether one can really distinguish so clearly between these two processes when dealing with Roman literature of the second century B.C.
- (68) F. Leo Röm. Lit. p.294. Cf. above, n.46.
- (69) E. Norden, Die römische Literatur, 5 aufl., 1954,25.
- (70) U.von Wilamowitz - Moellendorff, Aristoteles und Athen, I,1893, 30 n.2.
- (71) Although the ancients were aware of the difficulty of the unsuitable title, and tried to explain it, none of them ever suspected that the Origines was anything other than a unitary composition, composed and put together by Cato himself. It is worth pointing out that ~~the~~ authorities such as Cicero, who had read and admired the Origines (Brutus 17.66), were able

to consult the whole text, not a handful of fragments.

(72) Cf. D. Timpe, Le "Origini" di Catone p.13 etc.

(73) H. Peter argued that when Cato decided to continue his Origines with an account of his own age, he began by inserting at the start of his fourth book a brief summary of the early republican "Zwischenzeit", in order to connect the second part of his work with the earlier books. (H. Peter, HRR I, CXXXVI; cf. F. Bömer, "Historia" 2, 1953-4, 196). While this suggestion cannot be rejected out of hand, it is not based on any evidence, and seems unlikely on general grounds. Cf. D. Timpe, art.cit. p.15 n.38.

CHAPTER II *

(i) Cato's Origines and the development of early Roman historiography

The strange manner in which Cato treated the narrative history of the city of Rome, while it contrasts sharply with the pattern of the later, fully developed, "annales", does not appear quite so eccentric when compared with the practice of his own age.

The earliest Roman historians conformed to a regular and well established tradition in the formal arrangement of their works. Each traced the history of the city from its beginnings down to his own time; and in every case the allocation of space seems to have been the same: the earliest times were treated at length, as was the history of the writer's own age, but the intervening period was handled sketchily.

This curious 'hour-glass' shape is clearly attested in the remnants of the Annals of the poet Ennius; the first three of the eighteen books treated the origins of the city and its earliest history down to the end of the monarchic period. In the sixth book, he recounted the events of the Pyrrhic War, and in the remaining books he continued the story down to his

(*) For notes to Chapter II see below, pp. 79-84.

own day. Thus, only two books out of a total of eighteen, containing about 1500 lines each,⁽¹⁾ were given over to the first two centuries of the Republic.⁽²⁾

The same characteristic tendency is observable in the work of Cato's contemporary, Cassius Hemina.⁽³⁾ The first book of his account (it comprised at least four books in all) dealt with the legendary period before the foundation of the city (F.1-10 Peter); book 2 covered the period from Romulus (F.11) to the Gallic catastrophe (F.20), if not beyond (F.21?), and in book 3 Hemina had already reached the year 219 B.C. (F.26). We find precisely the same scheme in the annalistic notices of Diodorus; as E. Gabba points out, this is in itself a good reason for supposing that Diodorus was using an early annalist of the second century B.C.⁽⁴⁾

How the earliest Roman historians, who wrote in Greek, distributed their material, cannot be ascertained with certainty from the evidence of the fragments;⁽⁵⁾ but that they too devoted more space to the origins of the city and to the events of their own times than to the period in between is firmly stated in an important passage of Dionysius of Halicarnassus:

" τούτων δὲ τῶν ἀνδρῶν ἑκάτερος (sc. Κοίντος τε Φάβιος καὶ Λεύκιος Κίγκιος) οἷς μὲν αὐτὰς ἔργους παρεγένετο, διὰ τὴν ἐμπειρίαν ἀκριβῶς ἀνέγραψε, τὰ δὲ ἀρχαῖα τὰ μετὰ τὴν κτίσιν τῆς πόλεως γέγονενα κεφαλαιωδῶς ἐπέδραμεν " (6).

This evidence points to a consistent pattern in early Roman historiography, the importance of which has recently been illustrated in a fundamental paper by E. Gabba (cit.n.4). Gabba points out that the legends of the origins of Rome had already been dealt with many times and in considerable detail by Greek writers before Fabius Pictor.

The Greeks first started to take notice of the Romans after the middle of the fourth century B.C., when the expansion of their power in Central Italy began to have serious repercussions for the Greeks of the South. Then the historians of Magna Graecia began to enquire about the identity of the Romans - an enquiry which, in accordance with the practice of contemporary Greek erudition, entailed a study of their origins.⁽⁷⁾

Even before that, however, the legend of Aeneas had been connected with Rome by such writers as Hellanicus of Lesbos and Damastes of Sigeum.⁽⁸⁾ But at that time (the late fifth century B.C.), Rome was a remote and obscure place which had no distinct meaning for them; their primary concern was with Aeneas and not with the Romans.⁽⁹⁾ The presence of the Aeneas legend in Italy was well established at an early date, especially among the cities of Southern Etruria, where his cult was widely diffused already in the sixth century B.C.⁽¹⁰⁾

But the growing political importance of Rome in the fourth century led Greek scholars to make a more careful investigation of the origins of Rome for its own sake, and to take into account for the first time the traditions of the inhabitants themselves; this gave rise to the reconciliation and conflation of the two conflicting legends of Aeneas and of Romulus, by making the latter a descendant of the former. The process had begun in the fourth century; the earliest known mention of Romulus occurs in a fragment of the Sicilian historian Alcimus,⁽¹¹⁾ and the first recognisable reference to the story of the twins occurs in Lycophron (Alexandra 1232 f. = F.Gr.Hist. 840 F.16), whose source was a writer of the fourth century.⁽¹²⁾

By the end of the third century, when Fabius Pictor wrote his history, there was a lot of material available in Greek sources about the origins of the city. Indeed, we are told that Pictor's version of the foundation of Rome was not substantially different from an account which had already been made known to the Greeks by Diocles of Peparethus. We cannot know whether Diocles really was the source of Fabius, but Plutarch or his source evidently thought he was.⁽¹³⁾ It is clear that these Greek writers were only interested in the legendary origins of the city and did not care to study the tedious

details of her subsequent history - and certainly not the early centuries of the Republic. This is quite understandable in the case of learned Greek antiquarians who concerned themselves with genealogies and foundations of cities, among whom we may perhaps include Diocles of Peparethus and other obscure writers who are recorded as having opinions on the beginnings of the Eternal City, such as Promathion⁽¹⁴⁾ (F.Gr.Hist. no.817), Galitas (?) (no.818), Cleinias (no.819), and so on.

But we find the same reluctance to write about the archaic period of the Roman Republic in the historical writers of the third century B.C. Dionysius (I.6.1) tells us that the first Greek historians to write about the early history ("ἀρχαιολογία") of Rome was Hieronymus of Cardia, who inserted an account of the origins of the city into his work on the Diadochi and Epigoni - doubtless in the part which contained the adventures of Pyrrhus in Italy. Timaeus also wrote about the origins of Rome in his universal history, and we are told, again by Dionysius (ibid.), that he composed a separate work on the war between Pyrrhus and the Romans. For these historians the most important events involving Rome were undoubtedly those of recent times in which she had become an important power in the affairs of the Mediterranean; the accounts of the origins of the city probably took the form of

excursuses whose function will have been to introduce a new and hitherto unheard-of element into the history of the Hellenistic world - along the lines outlined by Polybius in I.12. 8-9.⁽¹⁵⁾ The same pattern was doubtless repeated in the work of Callias, the historian of Agathocles (F.Gr.Hist. 564 F.5).

Greek writers in general, therefore, had written about the earliest period of the city's history (and, of course, about the contemporary period, beginning with the Pyrrhic War), but not about the archaic age after the origins of the city. It is true that some episodes of this archaic history had come to the notice of Greek writers, but they were either isolated events totally devoid of historical context, like the attempted colonisation of Corsica, mentioned by Theophrastus,⁽¹⁶⁾ or events of wide significance, such as the capture of Rome by the Gauls - certainly the first event of Roman history which the Greeks thought worth recording at the time of (or soon after) the event itself.⁽¹⁷⁾

In view of this evidence it seems reasonable to suppose that the earliest Roman historians were able to write fully detailed accounts of the origins of the city because an abundance of legendary material was available, and was consolidated in numerous Greek sources. But after the foundation of the city and prior to the writer's

own age, a long period intervened which was treated summarily because no comparable stock of material existed. Historians were compelled to rely on native records, which were extremely meagre. This is Gabba's main contention, which I believe should be fully accepted.⁽¹⁸⁾

To return to Cato, we can see that the absence of any account in the Origines of the first two and a half centuries of the Republic could be interpreted as no more than an extreme version of an established tendency - i.e., Cato simply left out the period which his predecessors had treated merely in outline.⁽¹⁹⁾ There is no doubt a good deal of truth in this: omission of the early Republic should be seen in the light of the fact that no full account of the period had as yet been written by any historians - "καλήν ἱστορίαν", as Dionysius says, "ἐγκαταλείθεισαν ὑπὸ τῶν πρεσβυτέρων ἀνηγηρόνευτον" (I.6.3).

Greek writers of the third and second centuries B.C. had not been concerned with it, and the surviving evidence suggests that the earliest Roman work to contain an extended narrative of the age between the overthrow of the Tarquins and the war against Pyrrhus was the voluminous Annales of Gnaeus Gellius, who wrote towards the end of the second century B.C.⁽²⁰⁾ Very little is known about Gn. Gellius except that his work was on a very large

scale indeed; and from the available evidence it is clear that he devoted a lot of space to the early Republic.⁽²¹⁾ This account will have consisted mostly of vacuous accretions around the sparse tradition derived from earlier works. The surviving texts of Livy and Dionysius of Halicarnassus show that the factual content of the traditional account of the early Republic was very meagre, and that the size of the late annalistic accounts was due simply to rhetorical embellishment.

It seems that the earliest researchers (primarily Fabius Pictor), who put together the basic narrative, had very little evidence to draw on when dealing with the earliest period of the Roman Republic. The primary material available to them must have consisted mostly of the following:- a list, or lists, of magistrates, going back to the beginning of the Republic; texts of laws and treaties; early monuments and buildings, sometimes inscribed; popular oral traditions; the traditions of the aristocratic families (mostly oral for the early period, but perhaps including some written material); and whatever could be obtained from the chronicle of the pontifices.⁽²²⁾ It is difficult to assess the relative importance in the formation of the tradition of the various types of evidence listed here; but from the point of view of constructing a regular and connected narrative,

which could be easily related to the list of annual magistrates (which must have been indispensable), it seems that the most productive sources of information would have been the records of the pontifices and the traditions of the great families,⁽²³⁾ whose members had played a part in all the great events, and whose names were represented in the list of magistrates.

If this is anywhere near the truth - and it would be unlikely if these two types of evidence were not a major primary source for the early Republic - then certain interesting consequences would seem to follow regarding Cato's Origines.

Fabius Pictor and Cincius Alimentus dealt with the early history of Republican Rome in a summary fashion because they were constrained by a lack of sufficient evidence;⁽²⁴⁾ but Cato omitted the period altogether out of deliberate choice, and it is surely reasonable to assume that he was moved to do so (at least in part) because of the rather special character of the sources that were available.

We know that Cato eschewed the sort of historical writing that concentrated on the activities of a handful of important individuals - and tended rather to stress the achievements of the Roman people as a whole. The most obvious sign of this tendency is the fact that he did not mention the names of any of the generals in the

wars recounted in the later books of the Origines (a feature which will be discussed more fully below). Suffice it to say for the present that what we know in general of Cato's attitude to the established Roman aristocracy suggests in itself that the sort of account which relied heavily on the records of the leading noble families would not have appealed to him; and it is surely not fanciful, in view of the aristocratic character and gentile bias of Fabius Pictor's history,⁽²⁵⁾ to suggest that by totally omitting the period in which these features were most clearly evident, Cato was indulging in a veiled polemical attack against his illustrious predecessor.

Of course, polemic was by no means foreign to Cato's nature, and "die ihm angeborene Oppositionslust"⁽²⁶⁾ manifested itself, along with his censorious manner, in the pages of his Origines. We have already made reference to his sarcastic comment on the apologetic remarks with which Postumius Albinus introduced his history - and in the present context it is most interesting to note that a similar contempt for the methods of his predecessors is manifested in Cato's description of the chronicle of the pontifices:

"Non lubet scribere quod in tabula apud pontificem maximum est, quotiens annona cara quotiens lunae aut solis lumine caligo aut quid obstiterit." (Origines F.77).

It is not hard to see that Cato is here making fun of the trivial items which the tabula of the Pontifex Maximus contained; it is equally obvious that he would not have done so if the records of the Pontifex had not been a recognised source of information for, and had not been widely used by, earlier historians. Mommsen had already observed that if Roman historians had taken material for the earlier period of the Republic from this source, Cato's explicit rejection of the information it contained must furnish a prima facie explanation of why no account of the period appeared in the Origines,⁽²⁷⁾

A brief comment here on the question of the "carmina" (banquet songs). Cicero writes (Brutus 19,75):

"atque utinam extarent illa carmina, quae multis saeculis ante suam aetatem in epulis esse cantitata a singulis convivis de clarorum virorum laudibus in Originibus scriptum reliquit Cato".⁽²⁸⁾

Cicero's words indicate clearly that Cato had no direct acquaintance with the songs, but simply that he knew of their former existence. It is perhaps worth remembering that Cato omitted from his Origines the very period with which the "Carmina" are supposed to have been concerned, namely the early period of the Republic. "The kings of Rome (except the first and

last) are perhaps less 'poetic' than is the first century of the republic", writes A. Momigliano, in a list of alleged 'ballad subjects' (art.cit. (n.102) p.85). It seems altogether imprabable that Cato used the "carmina" as a source for his Origines; on the contrary, if Cicero's statement is drawn from a discussion of sources in the Origines (which is by no means certain), then it is far more likely that Cato also was lamenting their disappearance.

We may conclude this section by stating that Cato's omission of the period between the downfall of the kings and the outbreak of the Punic Wars may be due in part to a dislike of the character of the available evidence; rejecting the archives of the noble families as narrow and arrogant, and the pontifical records as utterly trivial, Cato decided to dispense with the period altogether, and to write on Italian Origins instead.

.....

(ii) The idea of "Italia" in Cato's Origines

It is not enough, however, to restrict ourselves solely to the problem of the missing history of Rome in the early Republic; it is necessary also for us to consider carefully the more positive question: why did Cato concern himself in the second and third books of

of his work with the origins of the cities of non-Roman Italy? As I have pointed out, there seems little doubt that Cato intended his survey of Italian origins to take the place of a narrative of the events of the early Roman Republic; it is conceivable, therefore, that the motives which led him to make a study of the former subject may also have influenced his decision to pass over the latter.

I am confident in the belief (which is shared by the majority of our ancient authorities) that the two books on the origins of non-Roman Italy constitute the most important feature of the work. We cannot know precisely why Cato centred so much of his attention on this subject, because we have no relevant evidence beyond the bare fact that he did so.

But one thing is fairly clear. For Cato, Roman history meant the history not only of the city of Rome, but of Italy as well. This conclusion is in a sense self-evident, as long as we realise that the sections on the origins of the communities of non-Roman Italy in books 2 and 3 were not in any way subordinate to a more conventional theme (namely the history of the Romans and their wars in Italy), but were parallel to the account of the origins of Rome in the first book; if, that is to say, the formal pattern of the "origo" of Rome in book 1 was reproduced, many times but on a smaller scale, in the

accounts on the origins of all the other cities of Italy.⁽²⁹⁾ I believe that it is fundamentally wrong to imagine, as e.g. Niebuhr, Schwegler, Seeck, Gutschmid and others have done, that sections on the origins of the Italian peoples were in some way interwoven into an account of the history of Rome, as it was normally understood; or that the second and third books contained an account of the conquest of Italy by Rome, in which small digressions on the origins of the Italian communities were patronisingly inserted into the narrative at each stage in the development and growth of Roman power. This is unlikely, not only because there is no evidence of any kind to support it, but also because the positive evidence of Cato's methods of research, as far as they can be discerned in the surviving fragments of books 2 and 3, are a very powerful argument for the view that he examined the origins of the Italian communities as a subject worth studying in its own right.

I do not believe that Cato intended his account of the early history of Rome in book 1 to be differentiated formally from the description of the origins of the other Italian cities in books 2 and 3; rather, his field of study was the whole of Italy, of which the city of Rome formed only one part - albeit the most important part. Naturally Rome was treated first,

and occupied more space than any other single city; nonetheless, the point must be emphasised that, in terms of the work's formal structure, the story of the origins of Rome in the first book is no more than the first in a long series of parallel accounts, occupying three books in all, of the origins of all the communities of Italy.

This interpretation is to some extent confirmed by the fragments of the first book, which began with the most primitive period of the history of Italy, not of Rome:

"Primo Italiam tenuisse quosdam qui appellabantur Aborigines". (F.5 P.).(30)

It is likely that we should also locate in this context the fragment on the ~~e~~ymology of the name "Italia", (which I quote in full, as it does not appear in any of the standard editions of the fragments of the Origines):-

"Italia ob vini copiam Oenotria appellata est.
Italiam Cato appellatam sit ab Italo rege.

Timaeus (F.Gr.Hist. 566 F.42) quod in ea boum quondam fuerit multitudo, Graecos autem antiquos solitos esse vocare tauros 'italos', a quibus videntur dicti vituli".(31)

Earlier historians had concentrated exclusively on

the history of the city of Rome: to write about the origins of Italy was entirely unprecedented, and in doing so Cato once more demonstrated a strong hostility to the type of historical writing that had been established by Fabius Pictor.

To Cato it may have appeared an unjustifiable arrogance on the part of a Roman historian to confine himself to the city of Rome alone; to do so was to ignore the enormous contribution of the allied communities of Italy to Rome's victories in the Punic and subsequent wars. If the content of the second and third books of the Origines implies that Cato recognised and acknowledged this contribution of the 'socii' to the success of Rome, then truly he showed "ungewöhnliche Einsicht in das Wesen der römischen Geschichte" (E. Norden, Röm. Lit. p.25).

Many scholars have believed that the incorporation of a survey of Italian origins into the Origines reveals a political conception of the author. What this means is not entirely clear; the suggestion that in his historical work Cato championed the cause of the Italians in a political struggle against Rome⁽³²⁾ is unwarranted; the Origines are not anti-Roman.⁽³³⁾

Nevertheless, it is not likely that Cato's study of the origins of non-Roman Italy was merely the product of antiquarian curiosity⁽³⁴⁾ with no wider implications.

On the contrary, a feeling for the unity of Italy was very strongly rooted in Cato: he clearly believed that Italy was unified by a characteristic culture and way of life: "Italiae disciplina ac vita....quam Cato in Originibus....commemorat". (F.76)⁽³⁵⁾ Moreover, the fragments of the Origines constitute the earliest evidence we possess for the notion that "Italia" extended as far as the Alps. True, the statement "Alpes quae secundum Catonem....muri vice tuebantur Italiam" (F.35) does not necessarily mean that he regarded the Alps as the frontier of Italy; but F.39 makes it clear that he did: "De magnitudine Gallicarum succidiarum Cato scribit his verbis: 'in Italia Insubres terna atque quaterna milia aulia succidia...' etc. (= Varro, de re rust. II.4.11).

It was primarily F. Klingner who lent his authority to the view that Cato grasped the concept of the fundamental unity of culture and sentiment which bound together peoples who, before the Roman conquest, were politically, ethnically, and linguistically diverse:

"Catos Anlage und im Grunde schon sein Titel Origines besagten 'es sind viele stränge die im römischen Staate zusammenlaufen. Man soll Italien nicht totsichweigen'". (Klingner, Römische Geisteswelt, p.59).

This interpretation, which has been widely approved,⁽³⁶⁾

is not only a reasonable proposition as it stands - it can also be supported by two further characteristically Catonian attitudes which seem to point in the same direction:

1. At the beginning of the second book of Cicero's De Republica, Scipio credits Cato with the idea that the Roman constitution was superior to those of other cities (he means, of course, those of Greece, as he goes on to say) because it was created not by one man, but by many and over a long period of time:

"Is (Cato) dicere solebat ob hanc causam praestare nostrae civitatis statum ceteris civitatibus, quod in illis singuli fuissent fere, qui suam quisque rem publicam constituissent legibus atque institutibus suis, ut Cretam Minos, Lacedaemoniorum Lycurgus, Atheniensium, quae persaepe commutata esset, tum Theseus tum Draco tum Solo tum Clisthenes tum multi alii, postremo exsanguem iam et iacentem doctus vir Phalereus sustentasset Demetrius, nostra autem res publica non unius esset ingenio, sed multorum, nec una hominis vita, sed aliquot constituta saeculis et aetatibus". (Cic. de Republica 2.2).

The Greek examples of course are Cicero's, but the main idea is fundamentally Catonian. Cato was a great believer in the collective institutions of

the Roman State, and throughout his life he fought against the tendency of leading aristocrats like Scipio and Flaminius to free themselves from the restrictions of a system of annual collegiate magistracies. In c.152 B.C. Cato spoke in favour of a law which proposed to forbid the tenure of a second consulship.⁽³⁷⁾

2. In his account of the wars in Origines 4-7, Cato apparently did not refer to any of the military commanders by name. That he should have omitted the names of Scipio, Flaminius, and Paullus while narrating the events in which they played such a large part seems incredible, but the uncompromising Cato did omit them, as we learn from Nepos and Pliny (n.h.8.11 = Origines F.88), and especially from the evidence of the fragments themselves (e.g. 82,83,86,87 etc.). F. Bömer has recently revived the theory, which goes back at least to Eduard Meyer, that the practice of omitting names of military commanders from historical narrative was a feature common to all the earliest Roman historians. The reason for this peculiarity is said to be that the archaic pontifical chronicle enumerated the events of each year without attaching to the events the names of the individual magistrates concerned in them. The names of all the magistrates were simply recorded at the beginning of the year, or, conceivably, at the time of their election.

It is supposed that the earliest annalists, including Pictor, merely adopted the practice of the chronicle.⁽³⁸⁾

Even if this is true in general, which seems unlikely, it cannot be the explanation of the omission of names in Cato; leaving aside the (nonetheless important) fact that Cato scoffed at the pontifical records (F.77), the explanation is worthless, as De Sanctis pointed out,⁽³⁹⁾ because it applies only to the early period, with which Cato was not concerned. In any case it will not have been necessary to consult the pontifical chronicle for information about the chief participants in important events since the beginning of the Punic Wars. Moreover, as E. Badian reminds us (Latin historians, (cit.p.468) p.8), the principle applies also to enemy generals, such as Hannibal, whom Cato referred to as "dictator Carthaginensium" (F.86 P).

It is altogether more likely that the omission of names of military leaders is a further indication of Cato's hostility to the growth of personal power in Rome, and to the 'cult of personality' in contemporary literature (cf. also his remark about the Hellenistic monarchs in Plutarch, Cato Maior ch.8.8). In part, Cato was expressing his belief that Roman magistrates acted not as individuals but in their capacity as representatives of the Roman people; he therefore referred to the great military

leaders of his generation not by their personal names but simply as "the consul", or whatever title was appropriate. But this highly unusual procedure was also a reaction against the method of earlier historians, who had concentrated solely on members of the great aristocratic gentes, and not on the achievements of the Roman people as a whole. As far as he was concerned, the credit for Roman victories was due less to the commanders than to the bravery of the army.⁽⁴⁰⁾

There is one fragment (83) which tells the story of a military tribune who, in a battle in the First Punic War, performed an act of heroism which Cato likened to that of Leonidas. This man, Cato says, received small praise for his actions. He tells the story in detail, but, in accordance with his practice, does not mention the name of the tribune, because his purpose was not to rescue the memory of the man from oblivion, but the example of the deed itself, which could be set against the act of Leonidas as a monument to the Roman spirit ("ingenium populi Romani").⁽⁴¹⁾ For Cato, the function of a general was no different from that of a private soldier: both had a duty to serve the state; and again, a brave soldier, or even a brave elephant (F.88), was as worthy of record as a successful general.

"sed quid duces et principes nominem?" asks Cicero, "cum legiones scribat Cato saepe alacris in eum locum profectas, unde redituras se non arbitrarentur". (42)

In Cato's opinion the strength of the Roman constitution derived from the fact that it was a gradual and collective product and not the creation of one or two individuals; similarly, the history of Rome was not the combination of biographies of a few aristocratic persons, but the story of the "gesta populi Romani" (F.1). In much the same way he thought of the history of Italy not as the record of a single city, however important, but as the combined history of a large number of different cities and peoples - Latins, Etruscans, Greeks, Celts, Ligurians, Sabellians, and so on. (43)

Cato may well have thought that the Italian confederacy was all the stronger precisely because its constituent elements were so diverse; just as the Roman constitution was superior to those of the Greek cities because it was the result of collective experience, and not the artificial product of one mind... "... non unius esset ingenio, sed multorum... constituta".

It was G. De Sanctis who first noticed the possibility that Cato's political conception of Italian unity might be connected with the fact that he omitted the history of the early Republic from his Origines. In the Storia

dei Romani (IV.2.1 p.63) he argued that Cato's "Italian Idea" would obviously have been accompanied by a certain reluctance to narrate the history of a period in which Italy was divided into separate and politically independent units, and in which Rome and the peoples of Italy were engaged in incessant wars with one another.⁽⁴⁴⁾

The narrative history of the wars in Origines 4-7 begins, in book 4, with the First Punic War. There may be no significance in this other than that Cato thought it to be a good starting point for a history of the recent past - just as Polybius felt that he could not give a satisfactory account of the Second Punic War without a preliminary narrative of the First.⁽⁴⁵⁾ But it is worth noting that the First Punic War, a turning point in Roman history, was especially significant in that it was the first war which the Romans fought outside the Italian peninsula, and the first in which none of the Italian peoples was engaged against them.

That the narrative should have begun at this point, and not, say, with the Pyrrhic War, accords well with the theory of De Sanctis that Cato was unwilling to relate the wars in which Rome was fighting against the Italians, and preferred to write about those in which they fought side by side.⁽⁴⁶⁾ After the fall of Tarentum in 271 B.C.,

Roman history and Italian history became identical, at least as far as external affairs were concerned; since the later books of the Origines seem to be largely concerned with external wars, it can be said that in effect they contain the combined history of Rome and Italy.

Before the Punic Wars, however, the history of the several Italian communities was diverse and complex, and could not be traced back along a single line; Cato rejected the practice adopted by other Roman historians of following the fortunes of the city of Rome alone, and recording the gradual subjection of one after another of the Italian communities. Instead he gave a systematically organised survey of their origins, treating each one separately and in turn. Thus, the earlier history of Rome and Italy before the Punic Wars was not given in a full and continuous narrative in the first three books of the Origines - but, as Mommsen justly pointed out, Cato's account of the origins of Italy "furnished a sort of substitute for the missing history of Rome from the expulsion of king Tarquinius down to the Pyrrhic War, by presenting in its own way the main result of that history - the union of Italy under the hegemony of Rome". (Römische Geschichte I, p.928, trans. Dickson).

(iii) Notes to Chapter II

- (1) Cf. O. Skutsch, The Annals of Quintus Ennius, "Studia Enniana", London 1968, 9 f.
- (2) Cf. J. Vahlen, Ennianae Poesis Reliquiae, CLXX-CLXXIV.
- (3) The chronological relationship between Cassius Hemina and Cato is something of a puzzle. The fact that the fourth book of Hemina's work was entitled Bellum Punicum Posterior (F.31 P) seems to indicate that it was written before the outbreak of the Third Punic War, i.e., before the composition of the last books of Cato's Origines. Moreover, Pliny calls him "vetustissimus auctor annalium". On the other hand, F.39 P. refers to an event of 146 B.C. It seems unlikely that he wrote before Cato; on the contrary, the fragments show clearly that he was influenced by the Origines. He was interested in etymologies (F.1-4 P), in religious and institutional origins (F.4,6,7,11, 12,13,14,18,20), in the origins of political and social customs (15,21,26), in chronology (F.8), and in the foundations of cities (2,3). All these features, especially the last, are reminiscent of Cato, whose influence on Cassius cannot really be questioned. (F. Leo, Röm. Lit., p.329). The comparison of Origines F.57 with Hemina F.29 seems to put the matter beyond doubt. He also accepted Cato's date (751 B.C.) for the foundation of Rome (F.20). Cf. F. Klingner, "Röm. Geisteswelt" p.76. On Cassius Hemina in general see H. Peter, HRR I, CLXV-CLXXIII; C. Cichorius R.E. 'Cassius (No.47)' 1723-5; E. Badian, The Early Historians 31 n.49.
- (4) E. Gabba Considerazioni sulla tradizione letteraria sulle origini della Repubblica, "Les Origines de la république romaine". Fondation Hardt, Entretiens XIII, 1966, 136-7.
- (5) But notice the valuable discussion of K. J. Beloch, Römische Geschichte, 1926, 95 ff.

- (6) Dion. Hal. I.6.2. Dionysius unquestionably means that Fabius and Cincius sketched over the archaic period, because his purpose in making this remark was to justify his own account of the "archaeologia" of Rome, as a period that had not yet been satisfactorily treated in a Greek historical work.
- (7) Cf. the important paper of E. J. Bickermann, Origines Gentium, "Class.Phil." 47, 1952, 65-81.
- (8) Dion. Hal. I.72.2 = F.Gr.Hist. 4 F.84 (Hellanicus); F.Gr.Hist. 5 F.3 (Damastes).
- (9) Cf. E. Bickermann, Origines Gentium (cit.n.7.), 66.
- (10) F. Bömer, Rom und Troia, 1951; K. Schauenburg, Aeneas und Rom, "Gymnasium" 67, 1960, 176-90; A. Alföldi, Die trojanischen Urahnen der Römer, 1957; id. Early Rome and the Latins, 1965, esp. 250 ff; most recently G. K. Galinsky, Aeneas, Sicily and Rome, Princeton 1969.
- (11) Festus s.v. Romam p.326 L. = F.Gr.Hist. 560 F.4.
- (12) J. Perret, (Les Origines de la legende troyenne de Rome, 1942, 349 ff.), pointed out that Lycophron's source could not be Timaeus, as was commonly supposed; but his own suggestion, that it was Fabius Pictor (!), is out of the question. (See A. Momigliano, Terzo Contributo, 682 f.). Cf. E. Manni, "Kokalos", 7, 1961, 3-14.
- (13) Plutarch, Rom. 3: evidence for Diocles assembled in F.Gr.Hist. no.820: bibliography and discussion in A. Momigliano, Secondo Contributo, 403.
- (14) On Promathion notice the eccentric views of S. Mazzarino in "Studi Romani" 8, 1960, 389 ff., and Il Pensiero Storico Classico, 1966, I, 190 ff., II, 64 ff. Cf. below pp.262-266.
- (15) "διόπερ οὐ χρὴ θαυμάζειν οὐδ' ἐν τοῖς ἐξῆς, εἴν που προσανατρέχωνεν τοῖς χρόνοις περὶ τῶν ἐπιφανιστάτων πολιτευμάτων. τοῦτο γὰρ ποιήσομεν χάριν τοῦ λαμβάνειν ἀρχὰς ταύτας, ἐξ ὧν ἔσται σαφὲς κατανοεῖν ἐκ τίνων ἕκαστοι καὶ πότε καὶ πῶς ὄρηθόντες εἰς ταύτας παρεγένοντο τὰς διαθέσεις, ἐν αἷς ἐπάρχουσι νῦν."

- (16) Hist. Plant. 5.8.2. This perhaps refers to an event of the fourth century (M. Sordi, I rapporti romano-etruschi e l'origine della "civitas sine suffragio", 1960, 94 f.; F. Cassola, I gruppi politici romani nel III secolo a.c., 1962, 32-3). Such an expedition by Roman ships seems unlikely before 311 B.C, when the duoviri navales were established (Liv. 9.30.4; J. H. Thiel, A History of Roman Sea Power before the Second Punic War, 1954, 19). On the other hand, Theophrastus dates the event by a rather vague " ", which, if anything, suggests a somewhat earlier date. A date before the middle of the fourth century is possible if the ships Theophrastus was referring to were not in fact Roman, but Etruscan (Sordi, loc.cit.); Thiel suggested the possibility that we are dealing with an event of the sixth century (op.cit. p.20) - a suggestion which has now been developed by S. Mazzarino, Il Pensiero Storico Classico, I, 190 ff. See below : p.264.
- (17) Theopompus F.Gr.Hist. 115 F.317; Plutarch, Camill. 22.2., citing Heraclides Ponticus; Aristotle Fr.586 Rose. Cf. M. Sordi, op.cit. (n.90), 25 ff.
- (18) E. Gabba, Tradizione letteraria... esp. 165 ff.
- (19) E. Badian, The Early Historians... p.11.
- (20) The date of Cn. Gellius can only be approximately fixed. He wrote after 146 B.C. because he recorded the Secular Games of that year (F.28 P) but probably before Coelius Antipater, because Cicero writes "omnes hoc historici, Fabii, Gellii, sed proxime Coelius..." (de Div. I.26,55 = Cn. Gellius F.21 P) which, for what it is worth, implies that Coelius was the most recent of those named; and Coelius was a contemporary of Gaius Gracchus, as he tells us himself (Coelius F.50 P).
- (21) One grammarian claims to quote from the 97th book (F.29 P). This is usually taken to be an exaggeration, but it is not inconsistent with the evidence of other fragments. F. 26 P., which seems to relate to the year 216 B.C. (cf. Peter's note ad loc.), is said to come from book 33, and an event of 389 B.C. is quoted from the fifteenth book (F.25 P). Gellius' treatment of the early centuries of Rome thus appears to have been much

more ample than any earlier account known to us. Among discussions of Cn. Gellius see especially K. J. Beloch, Römische Geschichte 103 f; F. Münzer, R.E. 'Gellius (no.4)', 998 ff; H. Peter, HRR I CCIV ff; A. Rosenberg, Einleitung u. Quellenkunde... 131 ff; E. Badian, The Early Historians... 11 f.

- (22) On the primary sources for Roman history before the Punic Wars see e.g., L. Pareti, Storia di Roma I, 1952, p.1 ff; A. Momigliano in "Journ.Rom.Stud." 53, 1963, 96 ff. (= Terzo Contributo 547 ff.), etc.
- (23) One cannot say precisely how big a part was played by popular oral tradition, as opposed to the traditions of the great families, but it seems likely that the stories preserved in this way will have been for the most part isolated from any firm historical context, and anecdotal in character.
- (24) The point that Pictor had no choice in the matter is emphasised by E. Gabba Tradizione letteraria... p.138; P. Fraccaro, "Athenaeum" 30, 1952, 246.
- (25) S. Mazzarino Il Pensiero Storico Classico II, 105-6.
- (26) H. Peter, Wahrheit und Kunst, 282.
- (27) e.g. T. Mommsen Römische Geschichte I (12 aufl)928.
- (28) Cicero, Brutus 19.75: Tusc. 4.2.3 = Cato, Origines F.118, - see Peter's note for further references. For criticism of the ballad theory in general see P. Fraccaro, "Athenaeum" 30, 1952, 242 ff; id., "Journ.Rom.Stud." 47, 1957, 59 ff; A. Momigliano, "Journ.Rom.Stud." 47, 1957, 104 ff. (= Secondo Contributo 69 ff.).
- (29) e.g. F. Leo, Röm. Lit. 293 ff; R. Helm, R.E. 'Porcius', 157 f.
- (30) Cf. F. Klingner, Italien: "Römische Geisteswelt", 18 ff.

- (31) Schol. Isidor. Etym. 14.4.18 (J. Whatmough, Scholia in Isidori Etymologiae Vallicelliana, Paris 1926, p.39). I owe this reference to A. Mazzarino, "Helikon" 8, 1968, 444 n.2.
- (32) Thus e.g. Mommsen, Römische Geschichte I, 928.
- (33) D. Kienast, Cato der Zensor, 108; D. Timpe Le "Origini" di Catone... 19.
- (34) Thus E. V. Marmorale, Cato Maior, Bari 1949, 224 ff; contra, D. Kienast op.cit. 107; D. Timpe art.cit. 20.
- (35) D. Timpe, art.cit., p.8 n.16, believes that the hypothesis of Cato's Italian idea is "molto esagerata", on the grounds that there is no evidence for it, except "il fatto che i primi libri delle Origines comprendono tutta l'Italia". What better evidence could there be?
- (36) H. Haffter, Cato der ältere in Politik und Kultur seiner Zeit, in "Römische Politik und römische Politiker" p.161; cf. D. Kienast, Cato der Zensor, 108 ff.
- (37) In the speech Ne quis iterum consul fieret, F.185-6 Malc; cf. Plutarch, Cat.Mai. 8.6.; see e.g. H. H. Scullard, Roman Politics 220-150 B.C., Oxford 1951, 234; Kienast op.cit. 92.
- (38) Ed. Meyer, "Rh.Mus." 37, 1882, 612 ff; H. Peter, HRR. I. CXXLI-II; F. Leo, Röm. Lit. 296; F. Bömer, Naevius und Fabius Pictor, "Symb.Osl." 29, 1952, 39 n.4.
- (39) G. De Sanctis, Storia dei Romani, IV.2.1.63; cf. D. Kienast, Cato der Zensor, 110.
- (40) Cf. F. Klingner, Cato Censorius... "Römische Geisteswelt", 59-60.
- (41) D. Kienast, op.cit. 110.
- (42) Tusc. 1.42.101; cf. Cat.Mai.de Sen., 20.75; and see Peter's note on F.83.
- (43) F. Klingner, Cato Censorius... 59 ff., D. Kienast, op.cit., 108-9.

- (44) "E fors' anche tutto cio era stato da lui omessa a ragione veduta, perche non amava scrivere intorno a contese civili o a lotte contro gli Italici e, dopo le origini di Roma, non voleva illustrare se non le imprese compiute da Roma col concorso degli Italici operanti sotto la sua egemonia". Storia dei Romani, IV.2.1.63.
- (45) e.g. Polyb. I.3.7; F. Leo, Röm. Lit., 294, argues for the same scheme in Cato. See also the remarks of G. Nenci in La testimonianza di Catone sulla "decessio de foedere" saguntina, "Studi Annibalici", 76 ff.
- (46) Cf. P. A. Brunt, Italian aims at the time of the Social War, "Journ. Rom. Stud." 55, 1965, 100n. The whole article (pp.90-109) is an important study of the concept of Italian unity.

CHAPTER III *

(i) The Structure of the First Book of the Origines

In the foregoing chapters I have argued that in spite of an evident diversity of subject matter, the several sections of the Origines have a kind of unity imposed on them by the imprint of the author's powerful personality (cf. Altheim, Epochen d. röm. Gesch., II.310). The Origines can without doubt be divided into sections containing material of very different kinds; but we have seen that there is no clear objective evidence that the form in which Cato's historical work circulated in the time of Cicero and Cornelius Nepos - i.e. a composition of seven books under the title Origines - did not accord with the intentions of the author himself.

Now that this important fact has been established, we can begin to analyse the form and content of the work in greater detail. The Origines falls into three parts: (a) the first book, on monarchic Rome, (b) the second and third books on non-Roman Italy, and (c) the last four books on contemporary history. I intend to take sections (a) and (c) first, in this chapter, and postpone a discussion of (b) until later. The reason

(* For notes to Chapter III see below, pp. 106-108.

for this will become apparent in due course.

We begin, theⁿ, with an examination of the character of the first book of the Origines. We have argued that the first three books treated the origins of all the communities of Italy, and that Rome, the most important city, was dealt with in book I. The internal evidence is consistent with this statement, in so far as the fragments relate to the arrival of Aeneas and the Trojans in Latium (F.4), the formation of the Latins from a fusion of Trojans and native Aborigines (F.5), the adventures of Aeneas and Ascanius in Italy (F.8-14), and the foundation of Alba (F.14). Whether or not the notorious dynasty of Alban kings appeared in Cato is uncertain; ⁽¹⁾ certainly he was aware of the chronological difficulty inherent in the view ⁽²⁾ that Romulus was the son or grandson of Aeneas (F.17). As for the story of Romulus and Remus and the founding of the city, Dionysius of Halicarnassus mentions Cato, along with Cincius Alimentus and Calpurnius Piso Frugi, as a follower of Fabius Pictor (F.15). So far his treatment is exactly what one would expect, although in some details his version of these events is unusual. But what follows is more of a puzzle. Cornelius Nepos says that the first book contained "res gestas regum populi Romani", and this is supported by the evidence of the fragments

which refer to events of the reigns of Romulus (F.20), Tullus Hostilius (F.22), Servius Tullius (F.23) and Tarquinius Superbus (F.24). (3)

But to give an historical account of the monarchic period, and then to leave out the subsequent history of the early Republic, is a rather strange procedure, and is not immediately justified as an essential part of the "origins of Rome" as we would normally understand it. One might have expected him to break off the narrative after Romulus, or conceivably Numa, but not to carry on for the first 250 years or so of the city's history. Cato was certainly no lover of monarchy -

"τοῦτο τὸ ζῶον ὁ βασιλεὺς σαρκολάγρον ἐστίν" (Plutarch, Cato Maior ch.8.8)... "nemo hoc rex ausus est facere..." (Orat. in Q. Thermum F.58 Malc)⁽⁴⁾ and it is clear that some explanation is needed of why he recounted the whole of the age of the kings in the first book of the Origines.

In our former discussion of the disproportionate allocation of space in the works of the early Roman historians, we saw that Fabius Pictor and Cincius Alimentus gave only a summary treatment of the period after the Ktisis of the city (Dion. Hal. 1.6.2). It is an important question whether the Ktisis of Rome in Dionysius means the founding of the city by Romulus, or whether it also includes the rest of the monarchic period as well; certainly there is no doubt about the

meaning of the word in the statement "τετάρτῳ δὲ μηνί μετὰ τὴν κτίσιν, ὡς Φάβιος ἱστορεῖ, τὸ περὶ τὴν ἀρχαίην ἐπολήθη τῶν γυναικῶν (sc. τῶν βαβύλων)" (Fabius Pictor F.7 Peter = F.Gr.Hist. 809 F.5).

And it seems to me most improbable that the account of the "κτίσις Ῥώμης" by Diocles of Peparethos (whether or not that phrase indicates the title of a work by Diocles, as Jacoby F.Gr.Hist. 820 F.1. assumed, is not clear) went on beyond the founding of the city to relate the events that occurred under the kings.

There is no clear evidence, as far as I know, that Fabius Pictor dealt more fully with the age of the kings than with the early Republic, in spite of numerous assertions that he did. When A. Alföldi argued that much of the traditional account of the regal period was deliberately fabricated by Fabius Pictor, critics rightly pointed out that hardly any elements of the received tradition could be shown to go back to Pictor, because in fact we do not know how he treated the regal period.⁽⁵⁾

And there is no evidence that the Greek writers who touched on the legends of the origins of Rome were interested in the subsequent monarchic period; it is true that Timaeus knew of Servius Tullius, but that he gave a full account of the age of the kings is most unlikely.⁽⁶⁾ Thus, there is no reason to suppose that

the age of the kings was not included in the period

" μετὰ τὴν κτίσιν τῆς πόλεως " which Fabius and Cincius treated "κεφαλαιωδῶς".

But whatever may be the correct interpretation of this statement of Dionysius, I do not believe that the content of the first book of Cato's Origines should be explained solely on the assumption that earlier historians had given a more detailed account of the monarchy than the early Republic.

The true answer, I believe, lies elsewhere. The clue to the character of Book 1 is indicated by certain remarks of Cicero, in the opening pages of the second book of the De Republica. We have already observed that Scipio, the main speaker in the dialogue, quotes Cato as the advocate of a theory of the gradual growth of the Roman state. The discourse on the development of the Roman constitution which Scipio delivers in book 2 of the Republic is distinctly stated to be an illustration of the truth of Cato's saying, that the res publica was created over a long period of time and by many men; the importance of this idea for Cicero, as an expression of the underlying purpose of his account of early Roman history in book 2, is demonstrated by the fact that Laelius takes it up again later in the book:

"Nunc fit illud Catonis certius, nec temporis

unius nec hominis esse constit^tutionem rei publicae;
perspicuum est enim, quanta in singulos reges rerum
bonarum et utilium fiat accessio." (De re publica, 2.21.37).

Cicero's view, that the character of the Roman state could be best illustrated by a historical sketch of its formative development, undoubtedly goes back to Cato. There can be no uncertainty about this, because Cicero openly admits it:

"quam ob rem, ut ille solebat, ita nunc mea repetet oratio populi originem: libenter enim etiam verbo utor Catonis". (II.1.3).⁽⁷⁾

This last phrase is clearly a reference to the Origines: the suggestion that these ideas are derived from conversations of Cato ("is dicere solebat" etc.) is of course due merely to the convention of the dialogue form.⁽⁸⁾

But the most suggestive piece of evidence in all this is the fact that Scipio's historical excursus, although fragmentary, can be seen to be concerned predominantly with the monarchic period, and is therefore seemingly co-extensive with the account of Roman history in the first book of Cato's Origines. (The fact that Cicero's account in Republic bk.2 extends slightly beyond the end of the regal period is significant and will be discussed shortly).

I have already argued that book 1 of the Origines contains the "origo" of Rome: and this is precisely how Cicero, following Cato, describes his sketch of early Roman history. There seems little doubt, as F. Leo pointed out, that the interpretation Cicero puts on the phrase "origo populi Romani" is derived from Cato - that is to say, it was Cato who gave him the idea that the "origo" of the Roman people ought to illustrate "nostram rem publicam et nascentem et crescentem et adultam et iam firmam atque robustam" (de re republica II.1.3).⁽⁹⁾

The explanation of the content of the first book of the Origines is therefore to be found in Cato's view that the "origo populi Romani" was unusual in being a gradual process; it was not enough to trace the antecedents of the Roman people, and to describe the foundation of the city by Romulus: Rome was not artificially created, like a Greek city, but was a natural, organic, growth: "nostra autem res publica non unius esset ingenio sed multorum, nec una hominis vita sed aliquot constituta saeculis et aetatibus". Consequently an account of its origins could only be given in the form of an historical narrative of the period in which its characteristic laws and institutions came into being, and in which it grew to maturity.

For Cicero this period ended, not with the overthrow of the monarchy, but with the decemvirate and the Valerio-Horatian Laws.⁽¹⁰⁾ This fact must be connected with the statement of Polybius that the Roman constitution had continued to be excellent since the year 449 B.C.⁽¹¹⁾ It seems unlikely that Polybius had arrived at this conclusion on his own and from an independent examination of the primary evidence; the idea that the res publica reached its maturity at this time must have been based on a conception of Roman history that had originated among the Romans themselves. Thus, although it may be true that Cicero was following the example of Polybius in ending his account of the history of the development of the res publica at the decemvirate, it is probable that the chronology of Polybius himself reflects that of his source.⁽¹²⁾ It is impossible to say whether Polybius was following Fabius Pictor, or Cato's Origines, or some other historian, or whether his source of information was simply the general opinion of the educated Romans of his time.

There can, however, be no doubt that certain characteristic political ideas are common to both Cato and Polybius: and while some (such as the description of Carthage as a mixed constitution - Polyb. VI.51.1 f., Cato Origines F.80 P) could just as easily imply the

independent use of a common source (e.g. Aristotle, Politics 1272 b 24 ff.), there are also some clear points of direct contact between the two. In the particular case of Carthage, it cannot be certain, unfortunately, that Cato's reference to its constitution was part of a comparison with that of Rome, as in Polybius, although the similarity of Origines F.79 P. to Polybius VI.52.4. is very suggestive. It has often been thought that Cato regarded the Roman res publica as a mixed constitution, but there is no definite proof of this.⁽¹³⁾

However, Cato's view that the Roman state was created "non unius ingenio... sed multorum" etc. is reproduced more or less precisely in Polybius VI.10.12 f.⁽¹⁴⁾ This evidence, which could be multiplied, clearly indicates that Polybius and Cato must have influenced one another directly. One should not ask whether Cato borrowed from Polybius (as Gelzer thought), or Polybius from Cato (as Laqueur believed);⁽¹⁵⁾ the exchange of ideas is much more easily explained by the view of D. Kienast, that they were personally acquainted and had conversed with one another on matters of political philosophy.⁽¹⁶⁾

To sum up: we have evidence of contact between Cato and Polybius, and of the strong influence of Cato's Origines on Cicero, particularly on the form of the excursus on early Roman history in the second book of

the Republic; and we have seen that Cato's account of the 'origo' of the Roman people must have contained a historical sketch of the creation of the major social, political and religious institutions of the Republic. Cicero and Polybius believed that the mature res publica was fully established with the Valerio-Horatian legislation;⁽¹⁷⁾ and it is surely an important question whether this terminal date was based on Cato's Origines, which certainly provided the model for Cicero's historical excursus, and which undoubtedly influenced Polybius' conception of the development of the Roman state.

Even if there were no evidence in its favour, I believe that the opinion would be justified that Cato did not terminate his account of the "origo populi Romani" in book I precisely on the occasion of the expulsion of Tarquin, but extended it to include the first few decades of the Republic.⁽¹⁸⁾ I say "even if there were no evidence", because it happens that there is some evidence, the importance of which has hitherto been underestimated, which seems to put the matter beyond doubt.

Among the fragments of the first book of the Origines is the following (F.25 P):-

"Cato in I originum: Nam de omni Tusculana civitate soli Lucii Mamilii beneficium gratum fuit".

There is no real difficulty about the incident referred to in this fragment. Tradition records that in 460 B.C. a band of rebels, led by a Sabine named Appius Herdonius, made a successful night attack on the Capitol in Rome. When the news reached nearby Tusculum, at this time an ally of Rome, the dictator of the town, Lucius Mamilius, persuaded the local senate to send a contingent of troops to help the Romans: with their aid the Romans were able to recapture the Capitol and restore the situation. We are told that they were duly grateful. (Livy III, 15-18: Dion.Hal.X.16).

H. Peter, in his note on Origines F.25, wanted to know "cur tum solius Mamilii beneficium gratum fuerit", in view of the fact that thanks were offered to all the people of Tusculum ("Tusculanis gratiae actae" - Liv. III.8.10). A fair point: but if he had read on a bit further Peter would have found, under the year 458 B.C., the entry "Eo die L. Mamilio Tusculano adprobantibus cunctis civitas data est". (Liv.III.29.6).

This must be what Cato was referring to in Origines F.25 P. Attempts to relate the fragment to Octavius Mamilius, the son-in-law of Tarquinius Superbus, and thus to place it within the context of the monarchic age, are designed simply to save the reputation of Cornelius Nepos: but the latter's statement "primus

(liber Originum) continet res gestas regum populi Romani" should not be taken as anything more than a rough guide, in view of the other shortcomings which we have indicated in his description of the Origines.

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(ii) The Organisation of the later books

As to the form of the last four books of the Origines, the main question is whether Cato arranged his material chronologically or in some other way. It appears from Nepos that the order of events in these books was chronological, a supposition supported by the fact that the trial of Servius Galba ended the work and was at the same time the most recent event recorded in it. But many scholars have thought that this impression is false, and that Cato did not follow a strict chronological order.

Nepos says that the work was written "capitulatim". This does not, however, mean that Cato organised his material by subject matter under headings ("capita"), as some have thought;⁽¹⁹⁾ as F. Leo demonstrated, "capitulatim" does not mean "abschnittsweise", but rather "nach den Hauptsachen", and refers to an account which outlined only the most important facts ("capita").⁽²⁰⁾ It seems that Nepos gave a literal translation of the Greek "κεφαλαιωδῶς", which is used several times by

Polybius to mean "in a summary fashion" (I.13.1; I.5.4; II.1.4; III.5.9; VI.58.1; XII.25.7; X.217 etc.). And as we have seen, Dionysius of Halicarnassus uses the same word, in contrast to "ἐκκριβῶς", to describe the manner in which Fabius and Cincius treated the period after the Ktisis of Rome (I.6.2). And again Dionysius says that the account of the Pentekontaetia in the first book of Thucydides was written "κεφαλαιωδῶς", which should leave us in little doubt about its meaning. (21)

The remark of Nepos ("haec omnia capitulatim sunt dicta") seems to tell us that Cato confined himself to recounting what he considered to be the main events ("capita") of the period he was dealing with, just as Thucydides did in the Pentekontaetia, or Polybius in his summary of the successive Gallic invasions of Italy in II.18 f.; this latter account is probably derived from Fabius Pictor, (22) and is written "κεφαλαιωδῶς", in the sense that it narrates in succession a number of episodes of similar type, with an indication of the length of time that elapsed between each of them. Exactly the same sort of narrative is suggested by the fragments of the later books of the Origines. Hence: "Deinde duovicesimo anno post dimissum bellum, quod quattuor et viginti annos fuit, Carthaginienses sextum de foedere decessere" (F.84 P) - here the suggestion

is that the period between the end of the First Punic War and the beginning of the Second was passed over rapidly, and was represented only by an account of those events which were relevant to the relations between Rome and Carthage.⁽²³⁾ Whether the other events of the period, such as the Gallic and Illyrian Wars, were also treated here, or in another part of the work, or were left out altogether, cannot be known; we do not have enough evidence to determine the criteria on which Cato selected the events which he treated in his account of the wars.

In the narrative of the First Punic War the episode of the military tribune Caedicius (F.83) was related in detail, and the inference must be that the account of the Punic Wars consisted of a number of selected episodes like this one. Another example is the altercation between Hannibal and Maharbal (F.86-7 P), which again may have been a self-contained anecdote ending with a moral ("vincere scis, Hannibal..." etc.). The alarming possibility that the whole account of the Punic Wars was made up of moralising and improving tales seems, on reflection, to be unlikely, although such stories must have occupied a considerable amount of space.

The only other topics which we know were dealt with at length all concern the author himself: that is,

the campaign he conducted in Spain as consul (F.92), and the battle against King Antiochus at Thermopylae in which Cato took part (F.130).⁽²⁴⁾ Otherwise we know that at least two of his speeches were given verbatim - the Pro Rhodiensibus (F.95 P) and the contra Galbam (F.106-109 P). To reconstruct any sort of general picture out of these few fixed points is extremely difficult.

The fact that Cato related only those events which he considered relevant, and left out unimportant details, is consistent with the ^estatement that he did not want to record trifling matters like high corn prices and eclipses such as the tabula of the Pontifex Maximus contained (F.77 P).

This remark about the tabula of course confirms the impression given by the fragments, and by the statement that events were narrated "capitulatim" - the impression, namely, that Cato rejected the annalistic method of arranging events year by year under the heading of the current magistrates. It is true that Cato's comment is directed specifically at the content of the tabula, but it undoubtedly implied also that he was hostile to the type of historical writing that the pontifical chronicle had inspired, and which recorded the routine details of each year:⁽²⁵⁾ to this extent, the content is inseparable from the method of presentation.

Rather, Cato's narrative method must have been episodic, and the dating of events fixed by means of intervals - i.e. an indication of the number of years before or after some fixed point - as F.84 implies.

But the rejection of the annalistic chronological method does not necessarily mean that events were not related in chronological order - nor does the fact that they were written "capitulatim". Again we may refer to the account of the Pentekontaetia in Thucydides.

No conclusive evidence suggests that the organisation of the later books of the Origines was other than roughly chronological, as Nepos suggests: but what we know about the allocation of material to the individual books presents difficulties: book 4 extended at least as far as the Battle of Cannae (F.86-7), and book 5 down to 167 B.C. at the earliest (F.95): this would mean that the two books together covered a period of about one hundred years, leaving a mere eighteen years at the most to the last two books. If Nepos is to be believed, the fifth book contained at least some of the Hannibalic War, and, if arranged chronologically, will have gone on to deal with the first two Macedonian Wars, the war against Antiochus, the Third Macedonian War and other events connected with it, as well as wars that were going on at the same time in Illyria, Gaul and Spain.

Cato himself played an important part in the events of this period, and we are told that he emphasised his own activities in the Origines, besides including his own speeches: at least one of these, the Pro Rhodiensibus, was incorporated in book 5.

The period between 216 and 167 B.C. covers so many memorable events that scholars have been inclined to argue that they could not all have been included in the fifth book of the Origines. The material, they believe, cannot have been related chronologically, and must have been arranged in a systematic scheme. We noted earlier the view of A. von Gutschmid that Cato related connected groups of wars separately and in turn, so that the last four books contained, respectively, the two Punic wars, all the Macedonian wars, the war against Antiochus and related events in the East, and all the Spanish wars - and in that order.

But the most satisfactory scheme of this type was proposed by Fraccaro.⁽²⁶⁾ He believed that the fourth book contained the First Punic War and part of the Second, as far as Cannae: the rest of the Hannibalic War occupied the first part of the fifth book. The substantial unity of events during the Punic Wars enabled Cato to hold to a narrative in chronological order which resembled a conventional account. But after

Zama this unity was not so evident, and the events of East and West followed separate, independent, courses. Fraccaro suggests that Cato's account followed the same pattern. He went on in the fifth book to deal with events in Illyria, Greece and the East generally, down to 167 B.C. In the sixth book, he turned to the West, and dealt with the wars against the Ligurians and Gauls, and all the numerous political problems which those wars created at Rome. In the seventh book, he came to the wars in Spain, and narrated in succession all the campaigns which the Romans had fought there down to the massacre of the Lusitanians by Galba, at the end of Cato's life.

But Fraccaro's reconstruction seems rather artificial. It implies a work written in accordance with a contrived and clearly formulated plan, and I doubt if the Origines really was such a work. It seems far more likely that the trial of Servius Galba appeared in the seventh and last book because it was the most recent event recorded in the work, and not because Cato chanced to be writing about Spain in that book.

A more satisfactory solution altogether is that the last two books were on a larger scale than the two preceding, and were of a different character. It is quite conceivable that the fourth and fifth books

contained an account of the century from 264 to c.167 B.C. Obviously the manner of exposition was brief (Sallust, Historiae I F.4. M), and the account made no claim to being comprehensive: on the contrary it was written "capitulatim", and there is no way of knowing which particular events Cato selected as worthy of record. The difficulty outlined above about the content of book 5 raises two problems:

1. It is normally assumed that the fourth book ended with the Battle of Cannae and "Hannibal ante portas".⁽²⁷⁾ In fact this is a compromise solution, based on a reconciliation of Nepos' statement - "in quinto (libro bellum Poenicum est) secundum" - with the disturbing fact that all the fragments referring to the Hannibalic War come from the fourth book. Is Nepos entirely, or partly, wrong? There are several possible explanations of Nepos' mistake - e.g. that his summary of the contents of the Origines is based on a brief glance at the opening pages of each book;⁽²⁸⁾ or that he somehow knew merely that the First and Second Punic Wars occupied books 4 and 5, and naively assumed that one book was assigned to each war; whereas in fact the Hannibalic War will have been treated more fully, prefaced by a brief summary of the First Punic War, in the manner of Polybius.⁽²⁹⁾ It should be admitted,

however, that there is no solid evidence of where the break between the fourth and fifth books occurred: Nepos is shown to be in error by the direct evidence of the fragments, and his statements on this point ought perhaps to be ignored altogether.⁽³⁰⁾

2. The period 216-167 B.C. included many memorable events; but there is no guarantee that Cato attempted to touch on all of them. Even the assumption that the later books of the Origines were confined to external wars may be erroneous, as it is based mainly on the impression given by Nepos' talk about "bella". One suggestion is that these later books were for the most part autobiographical ("haud sane detractor laudum suarum", says Livy of Cato: 34.15.9 cf. Plutarch, Cato Maior 14) and that Cato dealt only with those events in which he himself had played some part. R. Meister, who adhered to this theory, argued that the divisions between the books coincided with the major stages in Cato's life: hence, the fifth book will have begun with the year 214 B.C., in which Cato, at the age of seventeen, saw his first military service as "tribunus militum", and for the first time entered the pages of history.⁽³¹⁾ I make no comment on these reconstructions, which are all pure conjecture: my point is to illustrate the extent of our ignorance in these matters.

It does seem clear, however, that the scale of books 6 and 7 was larger than that of 4 and 5. The reason for this must surely be that when dealing with the period down to c.167 B.C. Cato could look back into the past and give a finished assessment of events;⁽³²⁾ but the last two books, on the events of the very latest years of Cato's life, contained contemporary history in the strictest sense - i.e., he must have recorded events more or less as they occurred, and went on adding to the account right up to the last few days or months of his life. (Cic. Brut. 89). These last two books were undoubtedly a gradual composition. As F. Leo suggests, it was perhaps the last two, not the last four, books which were left unfinished, and which had to be edited and added to the rest after Cato's death.⁽³³⁾

(iii) Notes to Chapter III

- (1) If the words of Servius ad Aen. 6.760 (= Origines F.11) "postea Albani omnes reges Silvii dicti sunt" really represent the view of Cato, the Alban king list will have appeared in some form in the Origines. Cf. R. M. Ogilvie, A Commentary on Livy I-V, 1965, p.43 f.
- (2) Apart from various Greek writers such as Alcimus (F.Gr.Hist. 560 F.4), Callias (F.Gr.Hist. 564 F.5), Eratosthenes (F.Gr.Hist. 241 F.45), and the source of Lycophron (Alex. 1232 ff.), etc., we know that Naevius and Ennius believed Romulus was the grandson of Aeneas (Serv. auct. ad Aen. 1.273). Cf. the remarks of O. Skutsch, Studia Enniana, 1968, p.12 f., and W. Strzelecki, Naevius and Roman annalists, "Riv.Fil.Class." 91, 1963, p.452 ff.
- (3) A. Mazzarino, in "Helikon" 8, 1968, pp.444-6, offers the conjecture that F.27 - "vehes ligni" - might refer to the war of Tarquinius Priscus against the Sabines recounted in Livy I.37.1 and Dion. Hal. III.35-6.
- (4) The Plutarch passage refers to Eumenes of Pergamum; in the speech it is uncertain whether Cato was thinking of the kings of Rome or of the contemporary Hellenistic monarchs. Cf. H. H. Scullard, Roman Politics, p.258.
- (5) A. Alföldi, Early Rome and the Latins, Michigan 1965, passim, esp. 101-175; for criticism see e.g. E. Gabba, Tradizione letteraria... 139 ff; A. Momigliano "Journ.Rom.Stud." 57, 1967, 211 ff.
- (6) F. Jacoby, F.Gr.Hist. 566 F.61 and Kommentar. The reforms of Servius Tullius constitute a well established part of the earliest tradition, and were recounted in detail by Fabius Pictor. (Liv. I.44.2; Dion. Hal. 4.15.1 = Fabius Pictor F.9,10, Peter, = F.Gr.Hist. 809 F.8,9). See the discussions of A. Momigliano in Terzo Contributo, 649-656; E. Gabba, "Athenaeum" 39, 1961, 98 ff., esp. 111; id., Tradizione letteraria..., 142.

- (7) On this question see esp. F. Leo Miscella Ciceroniana (1892) in "Ausgewählte Kleine Schriften" I. 319 ff; F. Taeger, Die Archaeologie des Polybios, 1922, 11 ff; E. Villa Il "de re publica" come fonte per la conoscenza delle idee politiche di Catone il Censore, "Il Mondo Classico" 1949, 68-70; cf. D. Kienast, Cato der Zensor, 110 f; D. Timpe, Le "Origini" di Catone... 21.
- (8) N.B. also Scipio's words at the very beginning of book 2 "Catonis hoc senis est, quem, ut scitis, unice dilexi maximeque sum admiratus..." etc. Cf. A. E. Astin Scipio Aemilianus and Cato Censorius, "Latomus" 15, 1956, 159 ff.
- (9) Cf. F. Leo Miscella Ciceroniana (cit.n.127) 317; D. Timpe, Le "Origini" di Catone... 21 etc.
- (10) Cf. K.von Fritz, The Theory of the Mixed Constitution in Antiquity, 1954, 135 ff.
- (11) Polyb. VI.11.1; Ed. Meyer, "Rh.Mus" 37, 1882, 622 f; K.von Fritz, op.cit. p.467 ff; F. W. Walbank, A Historical Commentary on Polybius I, 1957, note ad loc; F. W. Walbank and C. O. Brink, "Class. Quart." N.S.4., 1954, 113-4; F. Taeger, Archaeologie des Polybios, 100.
- (12) F. W. Walbank, Commentary, I. p.674; M. Gelzer, "Kleine Schriften" III, 97 n.18.
- (13) e.g. D. Kienast, Cato der Zensor p.110.
- (14) Cf. F. W. Walbank, Commentary, I. ad.loc.
- (15) R. Laqueur, Polybios, 1912, 248-9.
- (16) D. Kienast, Cato der Zensor, 110 f; A. E. Astin, art.cit. (n.8).
- (17) It is worth noting that Diodorus 12.25 brought the struggle between the orders to a definitive conclusion in 450 B.C. Cf. the remarks of M. Gelzer, "Gnomon" 1956, p.84, (= "Kleine Schriften" III, 196).
- (18) Cf. D. Timpe, Le "Origini" di Catone... 20.
- (19) e.g. Gutschmid, art.cit. p.524; Schanz-Hosius, Röm. Lit., 187.

- (20) F. Leo, Röm. Lit. 294 n.4; cf. L. Alfonsi, in "Parola del Passato" 9, 1954, 174-5; R. Meister, Zu römischen Historikern... p.4.
- (21) Cf. M. Gelzer, "Kleine Schriften" III, 105.
- (22) M. Gelzer, "Kl.Schr." III, 96-7; cf. K. Hanell, Zur Problematik der älteren römischen Geschichtsschreibung, in "Histoire et historiens dans l'antiquité", Fondation Hardt, Entretiens IV, 1956, 169.
- (23) Cf. G. Nenci in "Studi Annibalici", Cortona 1964, 71-81.
- (24) On Cato's account of himself, see P. Fraccaro, "Opuscula" I, 194 ff; H. Traenkle Catos Origines im Geschichtswerk des Livius, "Festschrift Karl Büchner", 1970, 274 ff.
- (25) Cato's statement calls to mind the remarks of Sempronius Asellio, who differentiated between "annales" and "historiae" according to their approach to the material, rather than to the method of presentation. On Sempronius Asellio F.1-2 P., see e.g., M. Gelzer, "Kl.Schr." III, 94 ff.
- (26) P. Fraccaro, "Opuscula" I, 193 ff.
- (27) D. Timpe, Le "Origini" di Catone... 24.
- (28) H. Peter, Wahrheit und Kunst. 285 n.1.
- (29) G. Nenci, in "Studi Annibalici", Cortona 1964, 77.
- (30) F. Leo, Röm. Lit. 295 n.
- (31) R. Meister, Zu römischen Historikern p.4.
- (32) Cf. P. Fraccaro, "Opuscula" I, 197.
- (33) On the Oratio contra Galbam, which Cato inserted into the Origines, together with a general account of the trial and acquittal of Galba, see N. Scrivero in "Giorn.Ital.Fil" 14, 1961, 63-68.

CHAPTER IV.*

(i) The Content of books 2 and 3

The second and third books constitute, as I have argued, the most interesting and important feature of the Origines. We have seen that by studying the communities of non-Roman Italy, Cato freed himself from the restrictions of the narrow 'Romano-centric' approach which had characterised all earlier historical writing produced by the Romans. He seems thereby to have expressed the view (whether he did so explicitly we cannot say) that the history of the city of Rome was not sufficient on its own to constitute an intelligible field of independent study.⁽¹⁾ Many of the most characteristic features of Roman life could not be adequately explained in purely Roman terms: "Sabinorum etiam mores populum Romanum secutum idem Cato dicit" (F.51). Rome was evidently part of a larger cultural unit. "Italia" could be seen as a koiné in which all the constituent elements shared in common a specifically Italian way of life. "Italiae disciplina et vita...quam Cato in Originibus... commemorat" (F.76).

These remarks about the general significance of the second and third books of the Origines lead on directly to the more specific question of their subject matter.

(*) For notes to Chapter IV see below, pp. 129-136.

It is a curious fact that in the past scholars have been concerned mainly with that part of the Origines for which there is least evidence - that is, with the last four books. The importance of the historical evidence furnished by many of the fragments of the second and third books is widely acknowledged, but they have rarely been systematically analysed all together, although it should be clear enough that a better knowledge of the general character of these books would greatly increase our understanding of the information contained in the individual fragments.

What, then, did the second and third books of the Origines contain? External testimony offers little help beyond the general remark of Cornelius Nepos, that they treated the subject "unde quaeque civitas orta sit Italica" - a statement which is supported by the evidence of other ancient writers.⁽²⁾ But if we are to form a more precise picture of what Cato had to say about non-Roman Italy, we are obliged to fall back on the evidence of the fragments themselves.

We are confronted here by the awkward fact that, with one significant exception, the available fragments are all brief extracts cited either by grammarians for points of language, or by antiquarians for single specific pieces of information - usually with no clear indication

of the original context.⁽³⁾ A second difficulty is that we cannot be certain of the order in which the fragments should be arranged.⁽⁴⁾ How Cato organised his survey of Italy is a matter for speculation. As we have argued (in Chapter I), the notion that digressions on origins were incorporated into the framework of a chronological narrative is unlikely, although it remains a theoretical possibility; it is more probable that the account was arranged systematically, and in some sort of geographical order.⁽⁵⁾ In any case it seems fairly certain that Cato dealt with each of the Italian peoples separately and in turn. This at least is the prima facie implication of F.73:

"Haut eos, inquit, eo postremum scribo, quin populi et boni et strenui sient",⁽⁶⁾

and this conclusion seems to be confirmed by the important account of the origins of the Sabines which is attributed to Cato by Dionysius of Halicarnassus (II.49. 2 ff. = Cato, Origines F.50), the exceptional fragment I referred to. Apart from its relatively substantial length, this fragment is important because it gives a complete and self contained discussion of Sabine origins in a condensed form; it is evidently a paraphrase of what must have been a much more extensive and complex account in Cato's Origines.⁽⁷⁾ The passage consists of the

following elements: (1) An explanation of the name 'Sabines'. (2) A statement of the original provenance of the Sabines. (3) An account of their arrival in the territory of Reate, and the colonisation of the land which they occupied in historical times. (4) A calculation of the size of this area, and of its distance from the sea. (5) An additional story about some Spartan exiles who came to Italy in the time of Lycurgus and settled among the Sabines, and (6) the consequent explanation of the harshness of the Sabine way of life.⁽⁸⁾

That this account is fairly representative of Cato's method in general is confirmed by the evidence of the other fragments. First, it is clear that he did not restrict himself exclusively to the question of origins; the description of the size and location of the Sabine territory, and the remarks about the discipline and warlike temperament of the Sabines, are an illustration of the way in which topographical and ethnographical matters were incorporated in the Origines. Many of the fragments are purely descriptive in the sense that they do not relate to the origins of peoples in the remote past, but to the conditions which obtained in Italy in the author's own age. A description of the contemporary situation was relevant to the question of origins in as much as inferences about the provenance of tribes could be drawn

from their current way of life,⁽⁹⁾ but it is evident that many of the fragments cannot be satisfactorily accounted for in this way. Cato's interests were clearly wide-ranging, and his account included much information of purely geographical or ethnographical significance. These kinds of material appeared in the Origines not because they had any direct bearing on the question of Italian origins as such, but evidently because Cato believed them to be worth studying for their own sake. The same applies to the admiranda of Italy and Spain (Nepos, Cat. 3.3). Nothing else can explain why he was moved to write about the fatness of Gallic pigs, for example, or the agility of goats (F.39 and F.52). Many of the fragments deal with matters of this kind - e.g. poppies and silphium (F.35 and 74), field mice, black goats, and white hares (120, 134 and Paradox. Palat. 21 p.360 Giannini, cited above, ch.I, n.23), the richness of the wine harvest in the Ager Gallicus (F.43), the location of the lacus Larius (Lake Como) and its extent (F.38). He enumerated 34 towns of the Euganei and 112 tribes of the Boii (41 and 44); he remarked on the law of succession at Arpinum, and a curious custom of the Libii (61, 33); but apart from his interest in local laws, customs and institutions,⁽¹⁰⁾ we know that he gave brief and pithy descriptions of the national characteristics

of the peoples of Italy - Ligurians, Celts, Sabines (32, 34 and 50-1) and the unidentified peoples of whom he wrote: "quin populi et boni et strenui sient" (F.73).⁽¹¹⁾

An analysis of the fragments, and a classification of the various kinds of information contained in them, can also indicate the essential features of Cato's method of dealing with the question of origins - in the strict sense of provenance of peoples and foundations of cities.

It is important to note, first of all, that he was not content merely to record the name of a founder, or to give a bare, mechanical statement of the facts about the origins of a tribe;⁽¹²⁾ rather, he attempted to reconstruct a detailed historical account of the (sometimes very complex) circumstances in which tribes migrated and cities were founded. To illustrate this point, I refer once more to the passage about the origin of the Sabines.

The fragment begins with an explanation of the name Sabini - "ἐπι Σάβου"⁽¹³⁾ That Cato was interested in the etymology of place names is widely attested.⁽¹⁴⁾ He sought to explain the names of tribes and places in various ways - for example by reference to local topographical features;⁽¹⁵⁾ but in the great majority of cases he was able to advance evidence for the derivation of names from eponymous heroes. The derivation of the name Sabines from Sabus has numerous parallels in the other fragments,⁽¹⁶⁾

and it is clear that attempts to draw inferences about the origins of peoples from the evidence of proper names was an important and regular feature of Cato's method.

But Sabus, the eponym of the Sabines, was not a purely nominal person invented by Cato. He believed, doubtless on the authority of indigenous tradition, that Sabus was the son of a local god named Sancus. There is no reason to suppose that he was ever moved to fabricate the name of an eponymous founder simply by arguing backwards from the name of a place.⁽¹⁷⁾ We know that he (18) supplied a brief genealogy for most of his founder-heroes, and was sometimes able to recount old and intricate traditions about them.⁽¹⁹⁾

The account of the Sabines' invasion of the territory which they occupied in historical times develops in stages, representing a well defined sequence of events. We are told that first of all they left their homeland (at Testruna, near Amiternum), and made an attack on the area around Reate.⁽²⁰⁾ They expelled the inhabitants - the Aborigines - and captured their main city, Cutiliae. From there they sent out colonies, and founded cities, including Cures.⁽²¹⁾ There follows the story of the Lacedaemonian " *σύνοικοι* ", who settled among the Sabines after leaving the Peloponnese at the time of Lycurgus.

Cato's approach can be described as historical partly

because his account was in narrative form, and presented the evolution of the Sabine people as a historical process (cf. the Catonian definition of the "origo populi Romani" in Cic. de re publ. II.1). But it is even more significant that he attempted to place the origins of the Italian communities in some sort of historical setting. The most obvious indication of this is the fact that he gave precise dates for the foundations of some towns,⁽²²⁾ and approximate dates for others.⁽²³⁾ Moreover, it can be seen that although he dealt with each of the Italian peoples in turn, he did not narrate the events of their separate histories in complete isolation, but in relation to the general prehistory of the Italian peninsula as a whole. I emphasise this point because it is not to be supposed that the recording of legends about the foundations of cities and so on was done for its own sake, as mere story-telling or antiquarianism.

We can in fact reconstruct from the fragments a broad outline of the events which gave rise to the founding of cities and the evolution of the tribes which inhabited Italy in historical times. The manner in which Cato reported the origins of the individual Italian communities makes it necessary to presuppose that he had a reasonably ordered conception of these wider developments.

In many areas of Italy he found traces of the Aborigines,

the first inhabitants of the peninsula (F.5), who had come from Achaea many generations before the Trojan War (F.6). They settled in Latium (F.5) and in the territory later occupied by the Volsci (F.7), as well as in the neighbourhood of Reate, before they were expelled by the Sabines (F.50). The arrival of Arcadian immigrants under Evander, which caused the spread of the Aeolic dialect among the natives (F.19), led to the foundation of Tibur by Catillus, who according to Cato (F.56) was the commander of Evander's fleet. After the Trojan War many of the Greek heroes settled in Italy, particularly in the South, where Philoctetes founded Peteliae (F.70) and where the native Aurunci⁽²⁴⁾ were dispossessed by the Greeks (F.71). Among the Trojan refugees who found their way to Italy were the Veneti ("Troiana stirpe ortos auctor est Cato" - F.42), and of course the followers of Aeneas. In Latium the Aborigines joined with the Trojans to become the Latins (F.5); Aeneas' son Ascanius founded Alba (F.11, 13), while another Trojan, Polites, founded Politorium (F.53).

Apart from these Greek and Trojan immigrants,⁽²⁵⁾ another important group to arrive from overseas were the Etruscans. We cannot be entirely certain that Cato subscribed to the 'oriental' theory of Etruscan origins, but the mention of Tyrrhenus in F.45 surely makes it probable that he did. Tarchon, the eponymous hero of Tarquinii

and founder of the Etruscan dodecapolis according to Etruscan tradition, is mentioned by Cato as the son of Tyrrhenus, and as the founder of Pisa (ibid.). This suggests a reconciliation of the Etruscan tradition with the Greek version;⁽²⁶⁾ and it is therefore likely that Cato's account was similar to that of Strabo, in which we are told that Tyrrhenus led the Lydians to Italy, named the country after himself, and appointed Tarchon as founder of the cities (V.2.2 p.219 C.). Unfortunately we are not informed about the relative chronology of the Etruscan migration in Cato's account; but the integration of the ancient tradition of Mezentius into the Aeneas legend (F.9,10,11,12) implies that the Etruscans were thought to be already established in Italy at the time of the fall of Troy.

Apart from this catalogue of legendary migrations (which do not, of course, reflect any kind of historical actuality,⁽²⁷⁾ but are merely the product of Greek theorizing) we know that Cato was aware of important developments which took place in historical times. No serious study of the origins of non-Roman Italy could fail to take account of the three "grands faits de l'evolution italique"⁽²⁸⁾ - the Etruscan conquest, the Sabellian expansion, and the Celtic invasion.

The fragments of Cato's Origines preserve the earliest

and most reliable literary evidence we possess of the existence of an Etruscan "empire" in Italy: "in Tuscorum iure paene omnis Italia fuerat" (F.72). Unfortunately we know very little about the account Cato gave of the Etruscan colonies in the Po valley and in Campania: but there can be no doubt of the fact that he wrote about them. The passage of Velleius Paterculus which reports Cato's date for the Etruscan foundation of Capua (Vellei, I.7.2 = Cato, Origines F.69) constitutes historical information of the first importance. We know also that the period of Etruscan rule was regarded by Cato as an important stage in the evolution of areas which had once formed part of the Etruscan "empire", such as the ager Volscus (F.62).⁽²⁹⁾

It is also reasonable to suppose that Cato's account referred to the expansion of the Oscan speaking peoples from the highlands of central Italy in the fifth century B.C., a movement which led to their occupation of a large part of Southern Italy.⁽³⁰⁾ According to tradition, each of the individual Sabellian tribes came into being as a result of a "ver sacrum",⁽³¹⁾ beginning with the migration of the direct descendants of the Sabines. We are told that the Samnites⁽³²⁾ left their homeland after a "ver sacrum" vowed by the Sabines during a war against the Umbrians (Strabo V.4.12 p.250 C.). The Hirpini, Lucani, Picentes,

and the rest, originated in the same way.⁽³³⁾ Unfortunately, we do not possess any fragments of Cato's treatment of these events,⁽³⁴⁾ but there can be little doubt that they were recorded in the Origines, especially as tales involving sacred springs formed a characteristic element of an ancient and indigenous tradition which seems in origin at least, to have been peculiar to the Sabellian peoples. If we are to believe J. Heurgon, these origin legends were cherished particularly among the Sabines, whose local traditions were well known to Cato.⁽³⁵⁾

Finally, we can assume that the Origines contained an account of the coming of the Celts. We know that Cato wrote at some length about the inhabitants of Cisalpine Gaul, which he regarded as a part of Italy (F.39,85, and above, ch.II, p.71), and it goes without saying that he must have described their arrival and their overthrow of the Etruscan empire in the valley of the Po. We possess fragments dealing with the geography (F.38) and agricultural economy (F.39,43 etc.) of the area, and with the customs of individual Gallic tribes (F.33 etc.); and one famous excerpt characterises the Gauls in general:-

"Pleraque Gallia duas res industriosissime persequitur, rem militarem et argute loqui" (F.34).

We know also that he tried to determine the relationship between the various tribal sub-divisions of the

North-Italian Celts (F.37,40,44 etc.); but there is unfortunately no way of telling from the evidence of the fragments how Cato treated the controversial question of the first appearance of the Celts in Italy. As is well known, the extant literary sources reveal two conflicting traditions about this event, both of which stand side by side in Livy. One of these versions dated the Celtic invasion of Northern Italy to the time of Tarquin the Elder - i.e. the late seventh or early sixth century B.C.⁽³⁶⁾ - while the other places the earliest incursions at the end of the fifth century (Liv. V.34-35.3). It might be said that since Cato recounted the anecdote of Arruns and Lucumo of Clusium (F.36), he must have subscribed to the latter view; but, for reasons which will become apparent (see pp. 124-127), this is not a compelling argument.

In conclusion, three general points may be made.

(1) We may take it as unlikely that Cato conformed to any very rigid scheme of arrangement in the second and third books of his Origines. Obviously his treatment of the origins of the cities of Etruria, Campania or Magna Graecia, will have been different from his account of the Oscan-speaking tribal communities of the highlands of central and Southern Italy, or of the Celtic and Ligurian tribes living in the foothills of the Alps. In

the former case he will have been dealing with sophisticated, semi-hellenised foundation legends and historical traditions associated with urban communities of long standing (e.g. F.45,56,58,59,69,71,72); but in his account of Transpadana, for example, he will have concentrated more on the identification of separate tribal groups (F.41,44, etc.) and on ethnic distinctions and relationships between various peoples (e.g. 37,40 etc.).

(2) It is important to recognise the great diversity of the material offered in the fragments of Origines books 2 and 3. It is misleading to suggest that Cato was interested only in the foundations of cities. Although legends about foundations and origins were undoubtedly an important element of these books, it is clear that they included much else besides. Indeed, one of the most striking features of Cato's account of non-Roman Italy, as revealed by the surviving fragments, is the fact that it combined with a study of origins a descriptive account of the land, its geographical and (important for Cato) agricultural peculiarities, and of the customs and institutions of the inhabitants.

(3) Cato's treatment of the communities of Italy was historical in that he tried as far as possible to place the early history of each in a wider context, and thus to produce an integrated account of the origins of "Italia", which could be related to the contemporary

situation that he was attempting to describe. In this way it becomes clear why Cato began his narrative history in book 4 of the Origines with an account of the First Punic War, because it was the first war in which the Romans fought at the head of a united Italy.

(ii) Appendix on F.36 and F.72

Some mention must be made of two unusual fragments which do not seem to fit into the general pattern suggested by the rest of the evidence. They will be discussed together in this appendix, as they both seem to me to present the same kind of problem.

Let us consider first the implications of F.36:-

"In secunda quoque origine M. Cato non longe secus hac particula usus est: neque satis, inquit, habuit, quod eam in occulto vitiaverit, quin eius famam prostituerat".

It has long been recognised that this fragment refers to the story of Arruns of Clusium, who, according to Livy, had encouraged the Gauls to invade Italy.⁽³⁷⁾ The view that the tale was created "ex nihilo" by Fabius Pictor is unwarranted.⁽³⁸⁾ Indeed, it is open to doubt whether Fabius Pictor had any knowledge of it at all. Fabius is the probable source of a passage of Polybius in which the Gauls are said to have migrated into Northern Italy "περὶ τὸ κάλλος τῆς χώρας ὀφθαλμιάσαντες, ἐκ μικρᾶς προφάσεως" (Polyb. II.17.3). Whether this implies a knowledge of the story of Arruns of Clusium is uncertain. Walbank and Heurgon believe that it does; but one could equally well side with Hirschfeld and assert the contrary.⁽³⁹⁾ Polybius is speaking here not about a Gallic attack on Clusium but about the occupation of Etruscan territory in

the valley of the Po. The element common to Polybius and the Arruns tradition is the explanation of the Gallic invasion; but this motive - Celtic greed - is a commonplace,⁽⁴⁰⁾ and in Justin it is used to explain a totally different migration (Justin 43.3.4). Even in the context of the invasion of 390 B.C., the connection of this motive with Arruns of Clusium is not firmly established in the literary tradition; Pliny, whose source was probably Varro,⁽⁴¹⁾ says that the person who first introduced the Celts to the vine was a Gaul named Helicon (Plin. N.H. XII.2.5). It is possible, as J. Wolski has argued,⁽⁴²⁾ that the entire tradition about the role of Clusium in the Gallic War is a later construction, arising from a duplication of the Gallic invasions of the third century, in which Clusium played an important part (see especially Polyb. II.25).

The Arruns story appears to combine two distinct elements: (1) the seduction of Arruns's wife by Lucumo, and (2) the origins of the Gallic invasion. In itself the tale of the seduction of Arruns's wife, which Cato referred to, does not necessarily presuppose any connection with the Gallic catastrophe, and I would be inclined to argue that its assimilation into the tradition about the Celts is secondary. There is no good reason to suppose that Fabius Pictor knew the story of Arruns and Lucumo,

and we cannot be certain of the context in which Cato recounted it. There seems little doubt that it was a popular story - "un anecdote, folklorique et discrètement érotique"⁽⁴³⁾ of Etruscan origin. Its basic elements seem to be repeated in the accounts of the conflict between the sons of Demaratus, also called Arruns and Lucumo (Liv.I.34). It is possible moreover that Arruns of Clusium was the subject of a group of connected legends, which also included the adventures of the brothers Caeles and Aulus Vibenna. According to Heurgon,⁽⁴⁴⁾ Arruns is represented on a bronze mirror from Praeneste (Gerhard-Körte, Etr. Spiegel, V.127); this shows the brothers Vibenna about to ambush a certain Cacū, who is playing a lyre and singing to a boy named Artile, whom Heurgon identifies with Arruns.

The insertion of the Arruns story into the tradition about the Gallic catastrophe is almost certainly arbitrary, and perhaps arose from the association in the tradition between the Celtic invasion and the city of Clusium. It does not matter for our purpose whether this was a doublet of the events of 225 B.C., or whether it has some basis in historical fact. I should like to think that Cato was not himself responsible for the combination of two traditions which were originally separate; it seems more likely to me that a later annalist took the story of Arruns

and Lucumo from Cato's Origines and conflated it with the conventional explanation of the migration - Celtic greed and fondness for wine - an explanation which had already appeared in Fabius Pictor.

A similar situation arises out of Origines F.72:

"Cato Originum lib. III: equos respondit: oreas mihi inde, tibi cape flagellum". (Fest. s.v. "oreae", p.299 L.).

This can hardly be anything other than an excerpt from the well known story of the horse, the stag and the man, with which the poet Stesichorus warned the people of Himera against Phalaris (Arist. Rhet. II.20, 1393b8). One might reasonably ask how this fable came to be included in the third book of Cato's Origines. To me it seems in the highest degree improbable that the third book treated the origins of the cities of Sicily as well as of Italy - although H. Peter, following Bormann, seems to have inclined to the view that it did (HRR I. p.cxxxiii). Even more unlikely, in my view, is the conjecture of Jordan, that the fable was used to represent the subjection of Italy to the rule of Rome (Proleg. XLIX). In the case of the Stesichorus-Phalaris story one is immediately struck by the fact that it is set in Himera, which fits in with the tradition about Stesichorus, but not with Phalaris, who was tyrant of Acragas. It is obviously significant

that in another version known to us Phalaris is replaced by Gelon (Conon, F.Gr.Hist. 26 F.1,42). As Freeman observed (History of Sicily II p.66): "the story would equally suit a great number of cities and a great number of tyrants" - and, we might add, a great number of poets or wise men too. That Cato had heard the story in an Italian context must be the explanation of its appearance in Origines III. Jordan's comparison with Menenius Agrippa's fable (Liv. II.32.9-12) is apposite. It seems most likely that in Cato the story was attached to one of the cities of Magna Graecia, although this cannot be certain. (45)

(iii) Notes to Chapter IV

- (1) Cf. S. Mazzarino, Il Pensiero Storico Classico II, 89 f: "un fatto essenziale resta comunque la coscienza, d'intorno alla metà del 2° secolo a.C., e al decennio che la seguì, che una storia di Roma è impossibile senza la storia d' Italia, la quale ne è premessa essenziale" - apropos of Cato and Cn. Gellius.
- (2) Dion. Hal. I.11.1: "...Πόρκιος τε Κάτων ὁ τὰς γενεαλογίας τῶν ἐν Ἰταλίᾳ πόλεων ἐπισημειώτατα συναγαγών"; Fronto, Princ. Hist. p.192 Van den Hout: "Catus ita Cato < censorius a > p < a > tria oppidatim statuis ornandus, qui prima sollertiarum et Latini nominis subolem et Italicarum origines urbium et aboriginum pueritias inlustravit"; Solin.2.2.: Sed Italia tanta cura ab omnibus dicta, praecipue M. Catone, ut iam inveniri non sit, quod non veterum auctorum praeceperit diligentia"; Serv. ad Aen. VII.678: "De Italicis etiam urbibus Hyginus plenissime scripsit, et Cato in Originibus".
- (3) For example, what was Cato's purpose in quoting the inscription which recorded the consecration of the sacred precinct of Diana at Aricia (F.58)? The most obvious answer would be that he wanted to trace the origins of the cult, or of the sanctuary. But this is not the only possible explanation, and one would certainly not be justified in inferring from this fragment that one of the topics dealt with in Cato's survey of Italy was the foundation of cults. A perfectly tenable alternative would be that Cato mentioned the dedication of the shrine of Diana in the course of a narrative account of the political events in Latium at the end of the sixth century B.C. (if that is indeed the true date of the shrine; see A. Alföldi, Early Rome and the Latins 47 ff., esp. 49-51). But a third possibility, which has the advantage of being consistent with what we know of Cato's procedure in general, is that he cited the inscription in an attempt to identify the earliest Latin cities. Just as he enumerated the towns of the Euganei, or the tribes of the Boii, so he may have wanted to find out not only about the origins of the Latins in general (F.5), but also the number and identity of the communities which constituted

the nomen Latinum in archaic times. Naturally the inscription at Aricia would have provided first rate evidence in an inquiry of this kind:

"Lucum Dianium in nemore Aricino Egerius Laevius Tusculanus dedicavit dictator Latinus, hi populi communiter Tusculanus, Aricinus, Lanuvinus, Laurens, Coranus, Tiburtis, Pometinus, Ardeatis Rutulus..." (F.58).

For some more examples of fragments of uncertain context see the appendix to this chapter, pp. 124-128.

- (4) It must be remembered that the arrangements of standard editions, such as Peter's HRR, are based entirely on the more or less speculative reconstructions by their editors of the form of the original.
- (5) If the description of Italy in Origines II and III was arranged geographically, some further evidence can be obtained from those extracts which are quoted with book-numbers. The fragments which refer to the Ligurians (31-2), to the Celtic tribes of Northern Italy (33-5), to the Marrucini (53), and to Tibur (51), Aricia (58), and Arpinum (61), are all cited from book II; whereas information about the extreme Southern tip of Bruttium is given in a fragment (71) from book III. (F.72 is numbered, but it is difficult to know exactly what it is referring to; if anything, it confirms the hypothesis offered here. See above pp. 127-128). This evidence suggests that the second book dealt with the Northern half of Italy down to (and including) Latium, while the South formed the subject of book III (cf. Wagener p.8; Jordan pp. xxxv-xxxvi; Peter, HRR I, p.cxxxiii). Despite its superficial attractiveness this reconstruction is by no means entirely compelling. F.71 is the only one of those known to be from book III which can confidently be placed in any sort of geographical context, and the fact that it deals with the toe of Italy may be fortuitous. It does not matter greatly whether Cato's geographical survey went from North to South in a straight line, or round in a circle, in the manner of a periplus, or in some other direction; the important question is whether it was arranged geographically at all. It will be assumed in this thesis that it was, if only because this seems a priori to be the most likely reconstruction: but I fully recognise that there can be no certainty in the present state of our knowledge.

- (6) Cf. Jordan XXXVI; Peter CXXXIII. Unfortunately, Gellius, who preserves these few words of Cato's text, omitted to identify the "populi" concerned. If he had done so, our problem would probably be easily resolved.
- (7) A similar example is given in F.48, where Servius (Auctus) summarises Cato's account of the foundation of Capena. Here too we have an extremely compressed account of a rather intricate sequence of events which must have been recounted in full by Cato. Cf. J. Heurgon, Trois études sur le "ver sacrum", Brussels 1957, (coll. "Latomus" XXVI), p.12.
- (8) The first part of this account (Dion. Hal. II. 49.2-3) is a genuine fragment of Cato, whatever view is taken of II.49.4-5. It is an extremely vexed question whether this latter passage, which deals with the Lacedaemonian exiles who settled among the Sabines, comes from Cato's Origines (as H. Peter supposed), or whether it represents an independent version. J. Poucet has argued that it is not Catonian, but I hope to show presently that it is, and I shall treat it as such in this discussion - although in the present context it is not a matter of central importance. See J. Poucet, Les origines mythiques des Sabins, in "Etudes étrusco-italiques" (Univ. de Louvain 1963) p. 155 ff.
- (9) The best example of this is provided by the Sabine fragment, where the harsh discipline of the Sabines is adduced as evidence of their relationship to the Spartans.
- (10) Cf. also F.78,80,81,94, etc. (on Carthage and Spain).
- (11) Other fragments of the same (descriptive) type as those cited in the text include 37,46,57,60,75,76. Among discussions of the second and third books of the Origines see especially G. C. Lewis, An Inquiry into the Credibility of the Early Roman History, London 1855, vol.1 pp.128-9; A. von Gutschmid, "Kleine Schriften" 5, 521 f; H. Peter, HRR I2 cxxxiii; F. Leo, Gesch. röm. Lit. I, 293,297 etc.; R. Helm, R.E. s.v. 'Porcius' (no.9) 157 f.
- (12) Although sometimes he was unable to discover even this. Cf. F.31 and 40.

- (13) The Mss. have "ἐπὶ Σαβίνου", which Sylberg emended to "Σαβίου". Poucet, in the article referred to (above n.8), argued against Sylberg's interpretation - in my opinion wrongly, as I shall argue later. In the present discussion the exact form of the name is immaterial.
- (14) And personal names. We are told (F.9) that he derived the name of Iulus (Ascanius) from the Greek "Ἰούλος" (= "a beard"), while Caeculus, the founder of Praeneste, was so called because of his small eyes (F.59). The derivation of the name Quirinus from "κύριος" (Lyd. de Magistr. I.5, p.11 W = Cato, Origines F.19) need not be attributed to Cato, who is in fact cited only as an authority for the view that Romulus and his followers spoke Greek.
- (15) E.g. F.46: "Graviscae...quod gravem aevem sustinent"; and F.60: "Praeneste...quia is locus montibus praestet". And, as I hope to show later, the derivation of the name Campani "a campestri agro" (Liv.IV.37.1 etc.) probably goes back to Cato's Origines. Cf. F.53: "Marsus hostem occidit prius quam Paelignus, propterea Marrucini vocantur, de Marso detorsum nomen", and the explanation of the name of Feronia in the story of the Lacedaemonian exiles (F.50).
- (16) E.g. Politorium founded by Polites (F.54); Tibur named after Tiburtus, brother of the founder Catillus (F.57). Perhaps the most interesting fragment in this connection is Cato's explanation of the name of Italy itself. He apparently rejected the relatively sophisticated explanation of Timaeus (F.Gr.Hist. 566 F.42), although he must have been aware of it, and returned to the earlier view of Antiochus of Syracuse (F.Gr.Hist. 555 F.5), that Italia took its name from a King Italus. Cf. above, Ch.II p. 69 and n.31.
- (17) We shall see when we come to consider his views on the origin of the Ligurians that the construction of gratuitous hypotheses was not characteristic of Cato's method.
- (18) E.g. F.45: Tarchon, son of Tyrrhenus; F.59: Caeculus, son of Vulcan, etc.
- (19) See especially F.59 (Caeculus).

- (20) Dionysius makes no mention of the circumstances which gave rise to the migration, nor does he say when it occurred; but he does tell us that it happened "at that time" ("τότε ") - which is meaningless in the account as we have it. It is a reasonable assumption, however, that "τότε" refers to something which was recorded in the original version of Cato, but omitted in Dionysius' shortened summary of it. For the speculation that the Sabines migrated after a "ver sacrum", see Heurgon, op.cit. (n.7) p.5 f., who connects it with the account in Dion. Hal. I.16 (from Varro).
- (21) It seems to me probable that Cato enumerated these early colonies in full. Dionysius specifies Cures because he was primarily interested in this city as the home of Titus Tatius (see II.48.1). A full list of cities would be consistent with the method revealed by F.41 and 44 - and F.58 if my interpretation of it is correct. (see above, p.129, n.3).
- (22) Ameria (F.49), Capua (F.69).
- (23) E.g. F.21: "Antenna etiam veterior est quam Roma"; cf. F.13, and F.7 (the arrival of the Aborigines "πολλῶν γενεῶν πρότερον τοῦ πόλεως τοῦ Τρωικοῦ ").
- (24) According to Livy the Aurunci were a small tribe living in the area situated between Latium and Campania and bounded by the rivers Liris and Volturnus (cf. the name Suessa Aurunca). It seems rather strange therefore that Cato should have located the Aurunci in the extreme South of the peninsula before the arrival of the Greeks. This may be connected with the fact that the name "Aurunci" is probably a rhotacised form of "Ausones" (the two people are identified by Servius ad Aen. VII.727). According to Antiochus of Syracuse (F.Gr.Hist. 555 F.7) the Ausones were identical with the Opici (Osci), who had once occupied a large part of Southern Italy before the Sabellian invasions: Thuc. VI.2.4; Diod. V.7.5; Dion. Hal. I.72.3; Strabo V.4.3 p.242 C; cf. Beloch, Campanien², 3 ff; J. Heurgon, Capone préromaine, p.50; E. T. Salmon, The Samnites p.28 ff.

- (25) Add the Argives who are said to have founded Falisca Etruscorum (F.47), and the Greek-speaking Teutanes, who had occupied Pisa before the arrival of the Etruscans (F.45).
- (26) Of course Cato himself was not responsible for the reconciliation of these rival versions. A similar compromise is already presupposed in Lycophron's Alexandra 1245 ff., where, however, Tyrrhenus and Tarchon are presented as the twin sons of Telephus, not as father and son.
- (27) Except in so far as Greek colonisation and (less certainly) the arrival of the Etruscans are real enough facts in themselves.
- (28) L. Homo, L'Italie Primitive, Paris 1925, p.32 (Eng. trans. V. Gordon Childe, London 1927, p.24).
- (29) We can infer from this fragment and from F.7 that Cato traced the history of this area with its successive occupations by Aborigines, Etruscans and Volscians.
- (30) On the Sabellian movement in general see e.g. E. Pais, Storia Critica di Roma II.385-6, IV.343-52; L. Homo, Primitive Italy p.136 ff; J. Heurgon, Capoue préromaine p.82 f; E. T. Salmon, The Samnites, p.34 ff.
- (31) The known instances are listed and discussed in W. Eisenhut's important article "ver sacrum", in R.E. VIII A, 919 f; cf. J. Heurgon, Trois études sur le "ver sacrum" p.6 f; E. T. Salmon, op.cit., p.35 f.
- (32) For definitions of the terms 'Samnite', 'Sabellic', 'Sabellian', and so on, see Salmon, op.cit. p.33.
- (33) Strabo V.4.12 p.250 C; Festus p.93 L (Hirpini); Serv. ad Aen. XI.785 (Lucani); Plin. N.H. X.40; Strabo V.4.2 p.240 C; Festus p.235 L (Picentes), etc.
- (34) Except for F.53 (cited above, n.12), which is rather obscure, but which is probably a reference to a "ver sacrum", as Schwegler (Röm. Gesch. I, 242 n.1) realised. (cf. Eisenhut R.E. VIII A, 920; Nissen, Ital. Landeskde. I.518). The account of the "ver sacrum" in Solin.2.7 (on the founding of Tibur)

was attributed to Cato by Peter (F.56), but this seems unjustified (cf. Eisenhut, art.cit. 921, who throws doubt on the whole passage). Cato is quoted in support of the view that Tibur was founded by Catillus the Arcadian. But the following sentence, in which the "ver sacrum" is described, speaks of Catillus as a son of Amphiaraus, and thus follows the more widespread tradition that Tibur was an Argive, not an Arcadian, foundation. (Verg. Aen. VII.670, and Serv. ad loc.; Plin., N.H. XVI.273; Horace, Carm. I.18.2, II.6.5 etc.). The story of the "ver sacrum" therefore seems to derive from the source (a certain Sextius) whom Solinus quotes for the Argive version, in contrast to Cato. On the Etruscan "ver sacrum" hinted at in Origines F.48, see J. Heurgon, Trois études sur le "ver sacrum", p.11 ff.

- (35) Cf. F.50; J. Heurgon, "Rev.Et.Latines", 45, 1967, 576; id., Trois études... p.11 ff; cf. below Ch. X.
- (36) Cf. Dion. Hal. VII.3.2, where the Etruscans are said to have been driven out by invading Gauls in the sixth century B.C.; this reference seems to be a reflection of the same tradition. Cf. H. Homeyer, "Historia" 9, 1960, 349 f; A. Alföldi, Early Rome and the Latins p.68 n.2.
- (37) Liv. V.33.3; Dion. Hal. XIII.14; Plut. Camill. XV. 3-6; Zonar. VII.23. A. Wagener demonstrated (by comparing Origines F.36 with Dion. Hal. XIII.14) that Cato was referring to the seduction of Arruns's wife by Lucumo - an interpretation which is now generally accepted (see Peter's note on F.36; Ogilvie, Comm. on Livy I-V, ad loc.) against Mommsen, who thought that the Arruns-Lucumo story was a late annalistic fabrication (Röm.Forsch. II.301-2). The hypothesis I am advancing would to some extent reconcile the two points of view because although I do not think that the story itself is a late invention, I do believe that its insertion into the tradition about the Celts was the work of a late annalist. This would answer the objections of Mommsen, which are not negligible.
- (38) A. Alföldi, Early Rome and the Latins, 157 f; the notion that Pictor invented all the Roman

stories involving women is without foundation. See A. Momigliano, "Journ.Rom.Stud." 57, 1967, 211 f. Alföldi cannot bring himself to imagine that any of these tales could have been handed down by native oral tradition - "it would be more correct, I think, if we were not to call these stories 'legends' any longer" - op.cit. p.151 n.1. This leads to the extraordinary remark on the Arruns-anecdote: "We cannot exclude the possibility that the latter (Pictor) did not invent this frivolous story, but found it in some Greek author" (p.158). Neither alternative is in the least likely.

- (39) F. W. Walbank, Commentary on Polybius I. p.182 ad loc.; J. Heurgon, La vie quotidienne chez les Etrusques p.310; O. Hirschfeld, "Kleine Schriften", p.17.
- (40) H. Homeyer, "Historia" 9, 1960, 346 and 358 n.66. This is an important paper, particularly in view of its contention that part of Livy's excursus on the Etruscans, in V.33. 7-11, derives via Varro from Cato's Origines.
- (41) O. Hirschfeld, "Kl.Schr." pp.17-18; Homeyer, art.cit. p.346 n.6.
- (42) J. Wolski, "Historia" 5, 1956, pp.35-9.
- (43) J. Bayet, Tite Live, Histoire romaine V, Appendix 5, p.156; cf. also the remarks of J. Gagé in "Rev.Hist.Rel." 143, 1953, p.176 ff.
- (44) J. Heurgon, La vie quotidienne chez les Etrusques, pp.274, 283, etc.
- (45) On the traditions about Stesichorus, see now M. L. West, "Class.Quart." N.S. 21, 1971, p.302 ff., but without any reference to the Cato fragment.

CHAPTER V *

(i) The "Ktiseis": Cato's literary models ?

The two books on non-Roman Italy were clearly an unconventional feature in a Roman historical work, and this fact raises the question of whether there was any earlier literature which Cato might have drawn upon. Leaving aside for the moment the problem of Cato's sources of factual information, we may ask ourselves first whether there was any literary precedent for a study of origins.

In this connection scholars have frequently drawn attention to the group of Greek writers of the Hellenistic period who concerned themselves with foundations of cities, and composed works entitled "κτίσεις". It has been argued that Cato modelled his account of Italy on these works, and that his Origines represents a deliberate attempt to establish a Greek genre in Latin;⁽¹⁾ the title itself is said to be a direct translation of "κτίσεις".

But in spite of the widespread currency of this view, very few concrete arguments have been adduced in its support. It seems to me to be very difficult to establish with any certainty what sort of relationship there was, if any, between Cato's Origines and the Greek Ktiseis, because so very little is known about the dis-

* For notes to Chapter V see below, pp. 153 - 155.

tinguishing characteristics of the latter. Indeed, it must be admitted that we know hardly anything at all about the Ktiseis.

As far as I am aware, there has never been an adequate discussion of this subject.⁽²⁾ One major drawback is that F. Jacoby did not live to complete the fourth part of his great work (here referred to as F.Gr.Hist.), in which the Ktiseis were to have been collected.⁽³⁾ This means, first, that the fragments and testimonia have not been systematically assembled,⁽⁴⁾ and secondly that we do not have the benefit of Jacoby's views on the Ktiseis in any coherent form. We are compelled to rely on scattered remarks in his various writings - remarks which sometimes contradict one another (see below, notes 9 and 11), and reveal that he had only considered the Ktiseis in passing, and had never concentrated his full attention on them directly.

In the following pages I have attempted to give a brief account of the origins and development of the Ktiseis as an independent literary genre. The direct evidence is thin and extremely fragmentary, and I have not discussed it in full in this chapter; instead I have added an excursus at the end of the thesis in which the main facts about the Ktiseis are set out in detail (Chapter XI, below pp. 373-407).

I regard it as self evident that in examining the nature of the Ktiseis as an independent genre one may legitimately use only the fragments of works which included the words "κτίσις" or "κτίσεις" in their titles. This rule applies even more strongly if one is going to draw inferences about Cato's Origines from the character of his supposed models.

The extent of our ignorance needs to be stressed from the start. For many of the known writers of Ktiseis we possess only one or two meagre and uninformative fragments, and for others we have no fragments at all, but only bare references to the fact that they wrote "κτίσεις", or "κτίσεις πόλεων", or something similar. Some of these latter examples are known to us only from lists of books quoted by the Suda, and the suspicion must be strong that often we are not dealing with separate works at all, but rather with subsections of larger and more general works. We know nothing whatever about the Ktiseis ascribed to Cadmus, Hippys, Charon, Philochorus and Callimachus, and not much more about those of Xenophanes, Zopyrus, Menecrates of Elea, Polemon of Ilion and Demosthenes of Bithynia, while Diocles of Peparethus is clearly a rather special problem. This leaves us with very little to go on. But some deductions can be drawn from the information available to us, scanty though it is.

Unless the surviving evidence is entirely misleading, three stages can be outlined in the development of the Ktiseis down to the first century B.C. These are: first, the beginnings of Ktiseis-literature in the poetry of the archaic period, secondly, the prose Ktiseis of the fifth century B.C., and thirdly, the Ktiseis in verse and prose which were produced in the Hellenistic age.

It seems clear that stories about foundations of cities formed an important part of the very earliest Greek literature. Interest in this subject in epic poetry may be compared to the reconstruction of genealogies, a practice which was originally intended to establish a direct link between the ruling families in the Greek cities of the archaic period and the heroes of the epic. In the same way, the need was felt to give the cities and their founders a place in the traditional history of the heroic age. Elaboration of local traditions about the mythical origins of cities eventually led to the formation of local epics - independent poems which had their starting point in the wider world of gods and heroes but then followed an individual course. The earliest traces of this development are already present in Homer. The locus classicus is the account of the colonisation of Rhodes by Tlepolemus, described in the second book of the Iliad (653-70)⁽⁵⁾.

The earliest explicit evidence we have for a complete

poem on the subject of a city's foundation is the Foundation of Colophon attributed to Xenophanes. It is clear, however, that earlier elegaic poets had dealt with similar subjects: for example, Mimnermus had written on the founding of Colophon in his poem Nanno, which (6) apparently touched on the colonisation of Ionia in general. An interest in foundations is also attested for Callinus (frg.7 Bergk), and Semonides of Amorgos, who is said to have written on the early history of Samos. (7) In the fifth century Panyassis of Halicarnassus composed an epic poem on the foundations of the cities of Ionia, if we are to believe a citation in the Suda. (8)

It is often said that the Ktiseis as a special literary type originated in poetry, (9) but this conclusion cannot be absolutely certain because of the doubt that attaches to the citation of Xenophanes' "Κολοφώνος Κτίσις" (see below p.373f), the only specific example in early poetry of a work with such a title. The other fragments we have mentioned indicate only that the archaic poets were interested in foundations, (10) but not necessarily that a special category of Ktiseis existed at that time. As F. Jacoby said in another context: "Poetical Κτίσεις (11) become certain and numerous only in the Hellenistic period." It is certainly possible, therefore, that we are dealing with a form which originated in prose, and only later

appeared in verse.

The development of prose Ktiseis in the fifth century began with Hippys of Rhegium, if the early date assigned to him by our sources is accepted (see below, pp. 393-400), and Ion of Chios, whose account of the colonisation of his native island is in any case the first work of its kind that is more than a name to us. One of the fragments of Ion's work is extensive enough to give us some idea of what it originally contained. Beginning with an account of the birth on the island of the eponymous hero - Chios, the son of Posidon - the fragment goes on to describe the various migrations and related events which occurred during the reigns of his successors. Thus a complex picture is built up of the mixed origins and gradual formation of the island's population. The passage is worth quoting in full:-

"Ion the tragic poet says in his history that Poseidon came to the island, which was then uninhabited, and there he loved a nymph, and when she was in labour snow ("Χιων") fell on the ground, and therefore Poseidon named the boy Chios. He also states that Poseidon loved yet another nymph, by whom he had two sons, Agelus and Melas, and that in the course of time Oenopion sailed with some ships from Crete to Chios, followed by his sons Talus, Euanthes, Melas, Salagus, and Athamas. Carians, too, came to the island

in the reign of Oenopion, and also Abantes from Euboea. Oenopion and his sons were succeeded on the throne by Amphiclus, who came from Histiaea in Euboea at the command of the Delphic oracle. In the third generation after Amphiclus, Hector, who had also made himself king, waged war on those Abantes and Carians who dwelt in the island; and some of them he slew in battle, and the rest he obliged to capitulate and withdraw. When the Chians had rest from war, Hector bethought him that they ought to join with the Ionians in the sacrifice at Panionium; and he received from the Ionian confederacy a tripod as a meed of valour. Such is the account which I find given of the Chians by Ion. He does not, however, say why the Chians are reckoned among the Ionians".⁽¹²⁾

The fragment appears to be a short summary of the whole of Ion's work. This emerges from the fact that it begins at the beginning, when the island was uninhabited, and goes down to the time when Chios was incorporated in the Ionian confederacy. The supposition is confirmed by the phrase " τοσαῦτα εἰρηκότα ἐς Χίου Ἴωνα εὕρισκον". We can see that Ion's account conflated a number of diverse local traditions, incorporating into the main narrative persons whose names were presumably associated with local monuments, cults, etc. The duplication of Melas suggests that he was trying to reconcile several different versions.

The general impression is of an honest attempt at historical reconstruction in the manner of Herodotus or Hellanicus. Ion's work appears to be a local history of Chios restricted in its scope to the earliest period. This is probably what Schwartz had in mind when he interpreted the Ktiseis attributed to Charon of Lampsacus as a subsection of the "Ἔργα Λαμψακηνῶν " (see below, p. 378). It is worth noting that the fifth century authors of Ktiseis, Ion, Charon, Hellanicus and probably Hippys, were all leading figures in the history of serious prose literature in the fifth century. In the case of Hellanicus it is probable that the "Κτίσεις Ἑθνῶν καὶ Πόλεων" represent an attempt to construct an integrated and coherent account of mythical events such as the colonisation of Ionia, and in this way the Ktiseis of the fifth century take their place beside genealogies and chronography as an important element of the scientific study of Greek prehistory.

A marked change seems to occur, however, in the subsequent period. In the fourth and third centuries B.C. the subject of foundations continued to be a major pre-occupation of Greek historians, but it was absorbed into the voluminous general histories which began to be written after the middle of the fourth century by such historians as Ephorus and Timaeus. These men did not compose separate works on Ktiseis, but incorporated their researches of

foundation stories (along with other topics such as ethnography, geography, and genealogies) ~~led~~ into the wider framework of universal history. The writing of Ktiseis did not die out, but it clearly became a specialised category of prose literature, moving perceptibly further and further away from the main stream of Greek historiography, and coming more and more under the influence of learned antiquarians, philologists, and sensational or romantic writers. (13)

If we ask what sort of authors wrote Ktiseis in the Hellenistic period, we find two main groups: on the one hand there were obscure men of whom almost nothing else is known from external testimony, such as Dionysius of Chalcis, Menecrates of Elea, Zopyrus, Diocles of Peparethus and Demosthenes of Bithynia; and on the other hand authors such as Apollonius Rhodius, Callimachus, and Polemon of Ilium, who were poets, scholars, antiquarians - but not historians. In other words, the evidence we have seems to indicate that in the Hellenistic age the Ktiseis had become separate from historiography.

Moreover, what we know of the character of the Ktiseis that were written in this period suggests that they made little attempt at serious historical reconstruction, but rather used the foundations of cities as a starting point for the construction of romantic and novelised

stories. This is certainly the picture which emerges from the fragments of the *Ktiseis* of Dionysius of Chalcis and Apollonius of Rhodes, the only Hellenistic examples of the genre of which we know anything at all. (14)

If we ask why the writers of Ktiseis are hardly ever referred to as historical authorities in controversies about foundations, (15) the answer must be that, as I have indicated, neither the purpose nor the content of their works was historical, but rather antiquarian, poetical and romantic. For Polybius, the standard authorities on the history of the foundation of Locri were Aristotle and Timaeus. Elsewhere Polybius mentions Ephorus and Eudoxus, and again Timaeus, as examples of writers who discussed foundations in their works, but he nowhere mentions anyone who wrote special studies of Ktiseis.

What Polybius has to say about the foundations of cities is of considerable importance for us, and in my opinion has often been misunderstood. It will be worthwhile to review the relevant passages.

In IX.1 Polybius distinguishes three types of historical writing, each of which, he says, appeals to a certain type of reader. First, there is "ὁ γενεαλογικὸς τρόπος", which pleases the "φιλήκοος"; then there is "ὁ περὶ τὰς ἀποικίας καὶ κτίσεις καὶ συγγενείας", appealing to the "πολυπράγμων καὶ περιττός"; and finally, for the

" πολιτικός ", there is " ὁ περὶ τῶν πράξεων τῶν ἔθνων καὶ πόσεων καὶ δυναστῶν ".

It would be a mistake to imagine that these three " τροποὶ " represent three entirely separate categories of literature. On the contrary, it is clear that Polybius is describing three types of material which were commonly found mixed in universal histories. After listing the first two " τροποὶ " (genealogies and ktiseis), Polybius adds: " καθά που καὶ παρ' Ἐφόρῳ λέγεται". The precise sense of the Greek is obscure, but I am inclined to agree with Shuckburgh and Paton that Polybius was citing Ephorus as an example of a historian who included this kind of material in his work. Walbank's translation "as Ephorus also remarks somewhere or other " hardly fits into the general sense of the passage. For Polybius is contrasting his own practice of concentrating exclusively on political history with that of almost all other historians, who combined it with genealogies and ktiseis:

" οἱ μὲν γὰρ ἄλλοι συγγραφεῖς σχεδὸν ἅπαντες, εἰ δὲ μή γ', οἱ πλείους, πᾶσι τοῖς τῆς ἱστορίας μέρεσι χρώμενοι πολλοὺς ἐφέλκοντα πρὸς ἔντευξιν τῶν ὑπομνημάτων " (IX.1.3.).

It can hardly be argued, therefore, that Polybius was distinguishing three separate groups of authors; and to suggest that he was contrasting ktiseis and genealogies with historiography as such, (16) is surely inadmissible.

It is clear that Polybius was not in fact referring to specialised works on foundations (i.e. the Ktiseis of Polemon, Callimachus, and the rest), but rather to the "κοινὰ ἱστορία" of Ephorus, Timaeus, and others, (17) which included sections on foundations. It has been suggested that one of the works Polybius had in mind when he wrote this passage was none other than Cato's Origines. But Cato's Origines is in fact an example of a historical work which does not fit into any one of the categories outlined by Polybius, but rather combines elements of all three. It could be said that the early books of the Origines were an example of "ὁ περὶ τὰς ἀποικίας καὶ κτίσεως καὶ συρρεθείας (τρόπος)"; but it is to be noted that Dionysius of Halicarnassus describes Cato's work as "γενεαλογίαι" (I.11.1). Moreover, in the later books of the Origines we find what several scholars have thought to be an attempt to write "pragmatic history" in the Polybian sense. (18)

The argument that Cato's Origines depend on the Ktiseis is based partly on the view that the first three books were originally separate from the remaining four; but this notion is unfounded, as we have seen (Chapter I), and in any case fails to take account of the fact that the combination of contemporary history with a study of foundations, migrations and so on, was the general rule

in the historical writing of the Hellenistic period.

In view of the foregoing remarks there is no need for us to assume that the form of Cato's work owed anything to the Ktiseis. It seems to me most probable that the idea of prefacing an account of recent history with a survey of the origins of Italy goes back directly to Timaeus and other writers who devoted parts of their works to origins. Timaeus' first five books seem to have contained a general description of the geography and ethnography of the West;⁽¹⁹⁾ they dealt with traditions about foundations and origins, as well as describing the land and the customs of the inhabitants. This type of writing, which goes under the general name of " Χωρογραφία ",⁽²⁰⁾ is precisely what we find in the second and third books of the Origines.

Of course some features of the Ktiseis are found also in Cato's work; quite apart from foundation stories, we may note for example that the Ktiseis show a marked fondness for the discussion of etymologies.⁽²¹⁾ But these resemblances seem less impressive when it is pointed out that foundation legends and etymologies were not unique characteristics of the Ktiseis, but were common to Hellenistic erudition in general, and to historiography in particular. Moreover, a comparison of the Ktiseis and Cato's Origines will reveal a number of important differences.

As I pointed out in the conclusion of the last chapter, the second and third books of the Origines represent a serious attempt to study the historical origins of the political and cultural situation which prevailed in Italy at the time of the author. The Ktiseis, by contrast, contained erudite information and romantic tales recounted for their own sake; the main focus of interest was firmly fixed in the most distant (mythical) past.

The second and third books of the Origines were not confined solely to foundation stories; as I argued in the previous chapter, Cato's account of Italy ranged much more widely than the title Origines and Nepos' phrase "unde quaeque civitas orta sit Italica" might suggest. But there is no reason to suppose that the Ktiseis contained anything other than stories more or less closely associated with the foundations of cities. There is not the slightest shred of evidence for the view (quite widely held, it seems) that the Ktiseis not only recounted the actual colonisation of places, but also included at least some of their later history. (22)

It can be said in conclusion that none of the many difficulties raised by the Origines is in any way solved or made easier by the hypothesis that Cato modelled his work on the Ktiseis. It seems to me that the hypothesis was proposed in the first place as a result of two basic

misconceptions. First, it is based on a misunderstanding of the nature of Cato's Origines. There is no doubt that Cato was to some extent following a Greek tradition and adopting Greek methods when he inserted an account of the origins of the Italian communities into his historical work; and there is also no doubt that many Greek historians wrote about foundations, as Polybius informs us. But the one distinctive feature of the Ktiseis, as far as we can see, is that they concentrated on foundations, and on foundations alone. Thus, the notion that Cato followed the Ktiseis does not become necessary, or even likely, unless it can be shown that he too composed a work that was concerned exclusively with origins. But - I repeat - the idea that the first three books of Cato's work originally stood alone as a work on origins is not acceptable.

Secondly, the hypothesis implies a view of the Ktiseis which cannot be substantiated by the known facts. Although Hellenistic historians like Timaeus were interested in foundations and origins, there is no reason to suppose that specialised works of Ktiseis formed a major element in Hellenistic historiography, as is sometimes asserted. This may have been the case in the fifth century B.C., when Ion and Hellanicus wrote their Ktiseis, but, as far as we can see, in the Hellenistic period Ktiseis refers to one of two things: (a) a category of romantic and mildly

erotic poems, known to us through the fragments of Apollonius Rhodius, and (b) a remote backwater of antiquarian studies, a long way from the main current of historical writing, and explored only by obscure writers such as Zopyrus, Dionysius of Chalcis, and Polemon.

There remains the supposed similarity of the titles. In fact the verbal equation Origines = Ktiseis is only approximate. The word "origo" has a definite causal connotation,⁽²³⁾ which is present, for example, in the Catonian conception of the "origo populi Romani".⁽²⁴⁾ "κτίσις", on the other hand, is never used in an abstract sense. It implies no more than the straightforward act of founding a city, or the colonisation of a tract of territory (as in the "κτίσις Ἰταλίας" of Hippys, or in Ion's account of the ktiseis of Chios, which makes no reference to a city, but simply describes the settlement of people on the island). It is possible that Cato had the Ktiseis in mind when he called his work Origines, but we are not compelled to believe this; he could quite easily have thought up the title for himself. The fact that the form and content of the second and third books of the Origines bear little resemblance to the Ktiseis makes it unlikely that the title should have been derived from them.

(v) Notes to Chapter V

- (1) See e.g. A. Schwegler, Röm.Gesch. I, 81; H. Peter, Wahrheit und Kunst, p.283 and n.2; W.von Christ, Gesch.griech.Lit. II, p.213; F. Leo, Gesch.röm.Lit. I, pp.290,293; A. Rosenberg, Einleitung und Quellenkunde, pp.163-4; G.De Sanctis, Storia dei Romani, IV.2.1, 61-2; B. Schmid, Studien zu griech. Ktisissagen (cit., below n.2), p.189 n.1; M. Gelzer, Nochmals über den Anfang der röm.Geschichtsschreibung ("Hermes" 82, 1954), in "Kleine Schriften" III, 1966, 107; R. Helm, R.E. s.v. "Porcius" (no.9), 161; D. Kienast, Cato der Zensor, p.107; E. Badian, The Early Historians, in "Latin Historians" (ed. T. A. Dorey), p.8; D. Timpe, Le "origini" di Catone..., "Atti Accad. Patav.", 1971, p.15 ff.
- (2) The account of B. Schmid, Studien zu griechischen Ktisissagen, Diss. Freiburg/Schweiz 1947, contains some useful material, but is not directly concerned with our problem, viz. the development of a specialised category of literature dealing exclusively with κτίσεις. His survey of the writers of prose Ktiseis is rather perfunctory, and occupies only four pages (90-93). He is chiefly interested in the character of foundation stories in Greek literature generally. The same applies to the recent dissertation of L. Gierth, Griechische Gründungsgeschichten als Zeugnisse historischen Denkens vor dem Einsetzen der Geschichtsschreibung (1971). Note also the discussions of a celebrated fragment of Callimachus by G. De Sanctis, Gli ecisti di Messina e Callimaco ("Atti Accad. Torino, LXIII, 1928), in "Scritti Minori" I, 1966, 43 ff; and W. Ehlers, Die Gründung von Zankle in den Aitia des Kallimachos, Diss, Berlin, 1933.
- (3) See e.g. Abhandlungen zur griechischen Geschichtsschreibung, Leiden 1956, p.359.
- (4) K. O. Müller arranged his edition of the fragments (F.H.G.) in chronological order of authors, with no attempt at systematic classification.

- (5) For discussion of this passage see E. Norden, Die germanische Urgeschichte in Tacitus Germania, 1923, p.16; B. Schmid, Studien z. griech. Ktisissagen, p.4 ff.
- (6) Mimnermus frgs.9-10 Bergk. Cf. C. M. Bowra, Early Greek Elegists, London 1938, p.28 ff.
- (7) F.Gr.Hist. 534 T.1: "Ἀρχαιολογία τῶν Σαρίων". But NB the reservations of Jacoby, Atthis, p.363 n.62.
- (8) Suda s.v. Πανύασις : Ἐγραφε δε..... Ἴωνικά, ἐν πενταμέτρῳ, ἔστι δὲ τὰ περὶ Κόδρου καὶ Νηλέα καὶ τὰς Ἴωνικὰς ἀποικίαις, εἰς ἔπη γ'. No certain fragments of this epic have survived, and its authenticity has been challenged: Jacoby, loc.cit. (n.7). For a general discussion of Panyassis: B. Schmid, Studien z. griech. Ktisissagen, 36-43; G. L. Huxley, Greek Epic Poetry, London 1969, p.186 f.
- (9) e.g. F. Jacoby, F.Gr.Hist. III B Suppl.2, p.2 n.13.
- (10) We may perhaps add Archilochus frg.145 Bergk, which refers (in whatever context) to the founding of Syracuse.
- (11) Atthis p.364 n.62; but contrast the reference cited above (n.9), where he tells us that the Ktiseis were a poetical "which was widespread in the fifth century".
- (12) F.Gr.Hist.392 F.1 (= frg.16 Blumenthal) = Pausanias VII.4.8-10, translated by Sir J. G. Frazer.
- (13) Cf. F. Jacoby Ueber die Entwicklung der griechischen Historiographie ("Klio" 9, 1909), in Abhandlungen p.62.
- (14) Cf. B. Schmid, Studien z. griech. Ktisissagen, 88-9; D. Timpe, Le "origini" di Catone, 16 ff.
- (15) Only in the exceptional case of Rome are the Ktiseis cited as authorities - Dionysius of Chalcis (F.Gr.Hist.840 F.10) and Diocles of Peparethus (F.Gr.Hist.820 F.1). This is because later scholars of the Roman period attempted to unearth every possible Greek version of the foundation of Rome that had been written before Fabius Pictor.

- (16) This is the view of D. Timpe, Le "origini" di Catone, 16 n.44.
- (17) Cf. Polyb. XII.26d.4 (Timaeus); XXXIV.1 (Ephorus and Eudoxus).
- (18) E.g. M. Gelzer, Der Anfang römischer Geschichtsschreibung, ("Hermes" 69, 1934), in "Kleine Schriften" III, 94 f.
- (19) J. Geffcken, Timaios' Geographie des Westens Berlin, 1892 ("Philolog. Untersuch." 13); cf. F. Jacoby, F.Gr.Hist. III B Kommentar 542 ff.
- (20) E. Norden, Die germanische Urgeschichte in Tacitus Germania, Berlin 1923, 10 ff.
- (21) E.g. Hellanicus F.Gr.Hist. 4 F.71; Ion of Chios, F.Gr.Hist. 392 F.1; Menecrates of Elea, Müller FHG II, p.342, frg.2; Dionysius of Chalcis, Müller FHG IV, p.393, frgs. 1,4,10,12 etc.; Apollonius Rhodius, J. U. Powell, Coll.Alex. (1925) p.5 f., frgs.5-9; Demosthenes of Bithynia, F.Gr.Hist. 699 F.10 (and F.16?). Cf. F. Jacoby, F.Gr.Hist. I (Kommentar) p.454.
- (22) E.g. E. Badian, The Early Historians p.8; D. Timpe, Le "origini" di Catone... p.17, and n.53 (with further references). On the meaning of the phrase "Κρίσις Πόλεως" in Dionysius of Halicarnassus etc., see above, Chapter III, pp.
- (23) Similarly, our word "origins", a concept which has a curious fascination for historians (who speak of the origins of wars etc.), perhaps because it means at once "beginnings" and "causes". On this ambiguity cf. the comments of Marc Bloch, The Historian's Craft (Trans. by Peter Putnam), Manchester 1954, p.29 f.
- (24) As defined by Cicero, de re publ. II.3. Cf. F. Leo, Miscella Ciceroniana in "Ausgewählte Kleine Schriften" I, 319 ff; L. Alfonsi, "Parola del Passato" 9, 1954, p.172. Cf. above, Chapter III, pp. 91.

CHAPTER VI

(i) The Sources of the second and third books of the Origines:Introduction

In the last chapter we concluded that Cato's account of Italian origins was not modelled on a special category of Greek literature concerned exclusively with foundations of cities (the Ktiseis), but rather that it was inspired by the practice of Greek historians of the Hellenistic period who had included in their works systematically organised accounts of origins and foundations, and geographical and ethnographical descriptions of peoples and places. The starting point for our discussion of these matters was the observation that the study of non-Roman Italy was something entirely new in Roman historical literature, and that in consequence the form of Cato's work cannot have been based on Roman models.

The same argument obviously applies a fortiori to the question of the sources of the second and third books of the Origines. It is a fact that the Roman annalists confined their attention to the history of the city of Rome alone, and passed over the independent

(1) For notes to Chapter VI see below, pp. 173-177.

histories of the other Italian communities. Cato was a conspicuous exception to this rule, and it follows that his account of Italian origins cannot have been based to any great extent on information drawn from earlier Roman writers.

But the peoples of Italy had not been ignored by Greek researchers. We know that many Greek writers, beginning perhaps with Hecataeus of Miletus,⁽¹⁾ had touched on the subject, and that some had written at considerable length about the origins and early history of the Italian communities - particularly the historians of the West, such as Antiochus of Syracuse and Timaeus. And it is perhaps in this context that some of the writers of Ktiseis become relevant, since both Hippys of Rhegium and Polemon of Ilium are known to have dealt specifically with the foundations of Italy.⁽²⁾

Cato must almost certainly have read some of these works. We learn from the fragments of the Origines that he attributed a Greek origin to many of the communities of Italy - and not only to the cities of Magna Graecia (F.70,71), but also to some of the peoples and places of the hinterland, such as Tibur (F.56), Politorium (F.55), Falisca Etruscorum (F.47), the Sabines (F.50,51), etc. The stories recounted in these extracts must go back ultimately to the Greeks, and in some cases Cato

may have used Greek literary sources directly.

But the Greek books on Italy that will have been available to Cato are known to us only through a handful of meagre fragments, and it is therefore very unlikely that we should ever be able to identify precisely the source of any particular fragment of the Origines. That Cato had read Timaeus is extremely probable on general grounds, but explicit proof is lacking. Polybius remarked that some writers had been deceived by Timaeus' account of foundations of cities (xii.26d.2). R. Laqueur suggested that he was making a veiled reference to Cato, whom he could not, of course, criticise directly;⁽³⁾ but this is hardly demonstrable. L. Moretti's attempt to show that certain fragments of the Origines are based directly on Timaeus unfortunately contravenes all the established rules of source-criticism.⁽⁴⁾

The fragments of Cato's work which report hellenised versions of the origins of Italian peoples do not necessarily imply that he was drawing on information taken at first hand from Greek sources - although it is often asserted that they do.⁽⁵⁾ It is remarkable, but nonetheless a fact, that in very many cases known to us, barbarian peoples were happy to perpetuate hellenocentric accounts of their own past which Greek researchers imposed on them.⁽⁶⁾

To cite some examples from Italy: the "tradition" that the Sabines were related to the Spartans was fabricated by the Greeks of Tarentum in the fourth century B.C.⁽⁷⁾ But this version of their origins was accepted by the Sabines themselves, who repeated it as an integral part of their national tradition.⁽⁸⁾

Similarly the Etruscans, probably at a fairly early date, had become firm believers in the story that they had migrated to Italy from the East under Tyrrhenus the Lydian. But this was clearly a Greek reconstruction, which conflicted with the native account - i.e. that the Etruscan cities had been founded by indigenous heroes such as Tarchon. Whatever the truth about the origin of the Etruscans, the information we have about their own national traditions undoubtedly implies that they were indigenous to Italy, as Pareti showed nearly half a century ago.⁽⁹⁾ The versions in which Tarchon, the founder of Tarquinii, is presented as the son or brother of Tyrrhenus, are obviously clumsy attempts to reconcile the Greek account with the native tradition.

But the best example of the process of assimilation of Greek conjectures is the development of the legend of the foundation of Rome. As we have noted (above, chapter II, p.57f), the Greek legend of Aeneas was established in Etruria and Latium at a very early date

(whether he was already thought of as a founder-hero in the sixth or fifth century is unfortunately not certain). By the middle of the fourth century at the latest, Aeneas was firmly associated with the origins of Rome, and had been incorporated into the national story of the foundation - originally as the father or grandfather of Romulus, later as a more remote ancestor.

Obviously Cato's account of the Aeneas story (F.4-14) is not taken from a Greek source, but is made up of traditions which in the second century B.C. were well established among the Latins; in the same way, when he spoke of Tarchon as the son of Tyrrhenus (F.45), and about the influence of Lacedaemonian immigrants on the Sabines' way of life (F.50-51), he was probably following local tradition rather than the version of Timaeus, or some other Greek author.⁽¹⁰⁾ The appearance of Greek elements in foundation stories recorded by Cato does not necessarily mean, therefore, that he was using Greek sources; it is equally possible that he had obtained his information from the inhabitants themselves, who had allowed their native tradition to be contaminated by the influence of Greek erudition.⁽¹¹⁾

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(ii) Limitations of scope and method in Greek accounts of Italy

Although it is scarcely conceivable that Cato had not read Timaeus and other Greek writers, it would certainly be wrong to imagine that he based his account of the origins of Italy entirely or even mainly on second-hand literary sources. The grounds for this assertion are twofold:-

(a) For much of the material presented in the second and third books of the Origines Greek and Roman literary sources simply will not have existed. It is one of the fundamental contentions of this thesis that Cato was the first historian of Italy as we understand it. To some extent this conclusion is an inevitable consequence of the view (above, chapter II, pp. 71 ff.) that Cato was the first to define "Italia" as the whole peninsula as far as the Alps. But I would argue further that he was the first to study the early history and ethnology of this area comprehensively and in detail.

Earlier Greek accounts had not been like this. They had naturally had their centre of gravity in the cities of Magna Graecia, ⁽¹²⁾ and even those historians (especially Timaeus) who had shown a genuine interest in the barbarian populations of Italy seem nevertheless to have concentrated mainly on those peoples who had

had dealings with the Greeks and who had played a significant role in the wider history of the Greek world. For all practical purposes this means the immediate (Oscan) neighbours of the Greek cities, the Etruscans, and, after the middle of the fourth century, the Romans.

The case of Rome is instructive. It was only in the second half of the fourth century, when the Romans began to get the upper hand in their struggle against the Samnites, that Greek writers began to give them serious attention.⁽¹³⁾ Before that date the city had only been incidentally alluded to by Antiochus, Hellanicus, and Damastes;⁽¹⁴⁾ it seems to be a reasonable inference that other Italian towns, which did not enjoy the meteoric success of Rome, continued to be as little noticed by the Greeks as Rome had been before the second half of the fourth century B.C. And there were presumably some more remote areas of Italy about which the Greek learned world was almost entirely ignorant.

The fragmentary evidence we possess suggests that Greek writers knew little about areas such as Cisalpine Gaul until the Roman conquest;⁽¹⁵⁾ the fragments which do survive about this area and its inhabitants are characterised by an almost total lack of genuine factual knowledge.

Nothing is more certain, therefore, than that Greek literary sources cannot have been a major source for Cato's description of the more remote parts of Italy such as Cisalpina. Greek speculations about the Hyperboreans, mythmaking about the River Eridanus, and jejune inferences like that of Timaeus on the origin of the Galatai⁽¹⁶⁾ should be compared with the precise and empirical details about the Celts in the fragments of Cato's Origines (e.g. F.37,38,40,41,44, etc.). This instructive comparison leads us on to the second point which seems to bear out our argument that Cato's account of non-Roman Italy was not based to any great extent on Greek literary sources.

(b) The hypothesis that is being offered in these pages is simply that Cato was a pioneer in the field of Italian prehistory, and that earlier researchers had not done his work for him. Just as there was no Herodotus before Herodotus⁽¹⁷⁾, so it could be said that there was no Cato before Cato.

But the contention that Cato's knowledge of the origins and early history of the communities of non-Roman Italy was derived from original research among primary sources is not only founded on the negative argument that he could not have obtained it from anywhere else; it also appears to be borne out by the

direct evidence of the fragments themselves. We can easily see from this evidence that Cato's method was different from that of earlier (Greek) authors who had touched on the subject of pre-Roman Italy.

Their evaluation of the evidence about the origins of barbarian peoples in Italy, and the inferences they drew from it, were determined largely by a priori considerations: stated simply, these amounted to a more or less general attempt to reconstruct the prehistory of the world by integrating the origins of all peoples, Greeks and barbarian^s, into a unified and ordered system centred around the myths and legends of the Greek heroic age. (18) Given the basic assumption that a rationalised interpretation of Greek legends provided the key to the earliest history of mankind in general, it was no difficult matter for Greek scholars to unearth "evidence" which confirmed their theories. Timaeus found "proof" of the Trojan ancestry of the Romans in the annual sacrifice of the 'October Horse' in the Campus Martius. (19)

Local traditions which conflicted with Greek hypotheses had either to be rejected altogether, or reconciled with the Greek version in some way. Thus Tarchon was affiliated to Tyrrhenus, Romulus to Aeneas, and so on.

But it is clear from the fragments of the Origines that Cato was not troubled by preconceived notions of

this kind. His approach was empirical and pragmatic, and the great majority of the fragments show that his account of the origins of the Italian communities was based on a faithful record of material which was available in the communities themselves. We know, for example, that he copied an archaic inscription recording the dedication of the precinct of Diana at Aricia as a cult centre of the Latin league (F.58).

Cato^t evidently felt the attractions of learned original research; our sources stress the diligence and care with which he collected material for his Origines,⁽²⁰⁾ and the fragments themselves undoubtedly show that his account was based on a close and direct acquaintance with his subject matter. He was no "arm-chair historian" like Timaeus, who had had no personal experience of political or military affairs, and who wrote in distant Athens about places he had never seen.⁽²¹⁾

The descriptive parts of the Origines - the fragments dealing with ethnography, with geographical and topographical matters, and with "admiranda" - are clearly based on the evidence of the author's personal experience.⁽²²⁾

Very many of the surviving fragments of the second and third books of the Origines are of this purely descriptive type,⁽²³⁾ and in fact there are very few of these extracts for which there is even a prima facie case for saying

that Cato was using secondary sources of any kind.

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(iii) Cato's use of local tradition

As for the historical and mythical traditions recorded by Cato, in many cases these seem by their very nature to be based on indigenous popular legends. The stories of Arruns of Clusium (F.36), of Sabus the eponymous hero of the Sabines (F.50-51), and of Caeculus of Praeneste (F.59) contain obvious folk-tale elements, and are so far from the sophisticated and rationalised conjectures of erudite scholars⁽²⁴⁾ that the use of local tradition seems certain.⁽²⁵⁾

But by far the strongest evidence for this point of view is furnished by those fragments in which explicit mention is made of the traditions or beliefs of the inhabitants of the area concerned, as in F.45,50,71, and particularly F.31, where Cato confesses his ignorance of the origin of the Ligurians:-

"Cato Originum, cum de Liguribus loqueretur: Sed ipsi unde oriundi sunt, exacta memoria, inlitterati mendacesque sunt et vera minus meminere". (Serv. auct. ad Aen. 11,715).

This small fragment reveals a great deal about Cato's methods in general. Three points emerge clearly:

First, the statement surely bears out the validity of the hypothesis which we have already seen to be probable on general grounds, namely that Cato investigated local traditions and attempted to discover what the peoples of Italy believed about their own past.

Secondly, the fact that he admitted his failure to learn anything precise about the origin of the Ligurians is clear evidence, not only of the faith Cato had in the value of local traditions generally, as opposed to his own conjectures, but also of his fundamental honesty; there are other instances where he admits his own ignorance (F.40,45), and we can understand why Dionysius, after rehearsing the views of Fabius Pictor and Vennonius on the number of tribes established by Servius Tullius, describes Cato as "more trustworthy than either of these"; for Cato, says Dionysius, did not specify the number of tribes: "Κάτων μέντοι τούτων ἀρροτέρων" (Fabius and Vennonius) "ἀξιόπιστότερος ὢν οὐχ ὀρίσσει τῶν κοινῶν τὸν ἀριθμὸν"⁽²⁶⁾. Obviously Dionysius was making the same inference from Cato's refusal to number the tribes as we can make from his statement about the Ligurians - i.e. that a historian who recognises the limitations of his evidence and admits his own ignorance is bound to be generally trust-

worthy. It is improbable that Cato should have recorded anything that was not substantiated by what he took to be reliable evidence; we may therefore rule out the possibility that the dynasty of Alban kings, for example, or the story of Arruns and Lucumo, were fabricated 'ex nihilo' by Cato.⁽²⁷⁾

Thirdly, the statement "inlitterati mendacesque sunt" is not simply a gratuitous insult, as some commentators have supposed; rather, the illiteracy and mendacity of the Ligurians were adduced by Cato as an explanation of the fact that they "vera minus meminere". That they are described as "mendaces" is probably not meant as a general statement about the Ligurians,⁽²⁸⁾ but rather indicates that they had some opinions about their own origins which Cato did not consider worthy of belief. This shows that his reporting of Italian native traditions was not uncritical. It is clear also that his distrust of the Ligurians' own account was due in part to the fact that they were "inlitterati". The implication is not only that they were uneducated, but that they possessed no written records of any kind, which might have revealed something of historical value; which implies in turn that Cato would certainly have made use of documentary evidence if there had been any. That this was his usual procedure is confirmed by F.58, the verbatim citation of an inscription.

(iv) Historical documents and local historiography
in the communities of non-Roman Italy

We may conclude this discussion with some general observations. Although Cato probably consulted and occasionally used the works of Greek historians and antiquarians, it seems that his main stock of information derived from original research - i.e. from a thorough investigation of the primary evidence that was available in the local Italian communities. Clearly this evidence will have included some written documents as well as oral tradition. This much is accepted by the majority of modern scholars, many of whom suggest, for example, that Cato used local chronicles in his researches.⁽²⁹⁾ Some maintain also that there was a more developed form of historical writing in parts of Italy outside Rome and Magna Graecia, and there is certainly no reason to reject this notion on a priori grounds. L. Pareti writes:-

"È chiaro che per attuare questo suo sistema, Catone aveva dovuto, ad ogni tratto, uscire dal campo ristretto della tradizione romana, mettendo a profitto tutte le informazioni che gli potevano offrire le altre storiografie a lui accessibili: la greca, l'etrusca e l'osca".⁽³⁰⁾

We have already touched on the question of Greek accounts of Italy - but more interesting is the suggestion

that Cato may also have made use of written sources independent of the literary historiography of the Greeks. General statements such as that of Pareti are common enough, but one might reasonably ask whether anything more precise can be said about the independent local sources for the history of pre-Roman Italy which might have been consulted by Cato.

This question can be tackled directly by a close examination of the fragments in an attempt to define more exactly the nature of the sources on which they are based; but this method is severely limited by the scrappy nature of the available evidence, although some useful results can be achieved (see below pp 259 ff); it seems to me that a rather more productive method would be to carry out an independent investigation of the subject of local historical sources in non-Roman Italy, using all the evidence available to us, not just the fragments of Cato. Such a study would be worthwhile on its own, because this is a much neglected field; but it is also relevant to the present subject in that it ought to illustrate the various kinds of material which would have been at the disposal of Cato, even if we cannot prove that a particular source or group of sources was actually used by him.

Here, however, it is necessary to point out that

the evidence we have is extremely uneven; for most of the communities of Italy there are hardly any positive traces of written historical sources - the few references we have are usually isolated⁽³¹⁾ and little can be said about them. In other cases we can say no more than that a certain unfamiliar anecdote or version derives from a local source - but that its precise character cannot be determined, except perhaps on internal grounds.

But one particular area of study is more hopeful. I refer to the substantial body of evidence relating to the Etruscans. That a considerable variety of historical documents and possibly some kind of literary historiography existed in the Etruscan cities seems to be established beyond all reasonable doubt. This contention is not only attested by the solid external testimony of reliable Roman writers such as Varro; it is also confirmed by a certain amount of direct evidence in the form of monuments and inscriptions from Etruria itself. And the influence of Etruscan local tradition can often be detected in our surviving sources.

The Etruscan evidence is clearly to some extent exceptional; but this is not entirely fortuitous, since the Etruscans were civilised and literate, and heavily influenced by the Greeks, at a very early date - so that we should not be surprised if historical literature, or

at any rate written documents of historical character, were more abundant and sophisticated in Etruria than anywhere else in Italy outside Rome and Magna Graecia.

The relatively large amount of evidence for Etruria offers us the possibility of studying the independent historical tradition of at least one important people of non-Roman Italy. Let us therefore turn our attention now to an investigation of this Etruscan evidence.

(v) Notes to Chapter VI

- (1) We know that Hecataeus mentioned places on the Ligurian coast around Massilia (F.Gr.Hist. I F.53-58) and three sites in Campania (Nola, Capua, Capri: F.61-63); all the rest of the fragments on Italy deal with places in the extreme South of the peninsula (F.64-71; 80-89). This distribution is consistent with the view being advanced in this chapter, that the hinterland of Italy was little known to the Greeks until relatively late. The barbarian peoples and places mentioned by Hecataeus are those which had some contact with areas colonised by the Greeks. For a good discussion of Hecataeus' description of Italy see L. Pearson, Early Ionian Historians, pp.38-45.
- (2) See M. Gelzer, "Kleine Schriften", III, p.107, for the view that Hippys of Rhegium may have influenced Cato; R. Helm, RE s.v. 'Porcius' (no.9), 161, suggests that Cato may have used Polemon.
- (3) R. Laqueur, RE s.v. 'Timaios', 1203; cf. G. De Sanctis, Storia dei Romani IV, 2.1, p.62; but notice the more cautious view of F. W. Walbank, Historical Commentary on Polybius, II p.407, on XII.26d.2.
- (4) L. Moretti, Le "Origines" di Catone, Timeo ed Eratostene, "Riv.Fil.Class." 80, 1952, 289-302. His argument is based on a comparison of fragments of Cato with passages of Justin and Lycophron, both of whom (he claims) used Timaeus. But there is no compelling evidence that, in the instances listed by Moretti, Cato and Lycophron (or Cato and Justin) share a common source; even if they do, it is necessary to demonstrate not only that Lycophron and Justin used Timaeus, but that they used no other source except Timaeus. Against the hypothesis (J. Geffcken Timaios' Geographie des Westens, 1892, and W. Schur, "Klio" 17, 1921, 137ff.) that Lycophron's source was exclusively Timaeus see the bibliography cited above, chapter II n.12; see also M. Sordi, I rapporti romano

ceriti, 1960, p.10 ff. The problem of the sources of Trogus Pompeius is notoriously difficult, and not greatly helped, in my view, by hypotheses involving the shadowy figure of Timagenes. Any similarities there might be between Justin's account of the origins of Italian peoples (book XX) and fragments of Cato might be explained economically by the possibility that Trogus Pompeius took some of his information directly or indirectly from Cato's Origines.

- (5) e.g. L. Moretti, art.cit. p.289; D. Timpe, Le "Origini" di Catone, p.20.
- (6) E. J. Bickermann, Origines Gentium, "Class. Phil." 47, 1952, 65 ff; the phenomenon was clearly recognised already by A. Schweigler, Röm. Gesch. I.81.
- (7) Strabo, V.4.12, p.250 C.
- (8) See Dion. Hal. II.49. 4-5 (= Cato, Origines F.50); Plut. Numa 1; etc. See below pp.343-372.
- (9) L. Pareti, Le Origini etrusche, 1926, p.13 ff; the literary sources are discussed also by M. Pallottino, L'Origine degli Etruschi, 1947, p.15 ff. The distinction between genuine popular tradition and the learned inferences or conjectures of Greek scholars should be clearly understood before any use can be made of the literary evidence in a discussion of Etruscan origins. The question is, in which category - tradition or inference - should one place the view of Dionysius of Halicarnassus, that the Etruscans were autochthonous? See Pareti, op.cit.; also Storia di Roma e del mondo romano, I, p.113 ff. For the opposite point of view see J. Berard, La question des origines étrusques, "Rev.Ét.Anciennes" 51, 1949, 202-45. Good discussions of the main points can be found in H. H. Scullard, Two Halicarnassians and a Lydian, "Ancient Society and Institutions", Studies V. Ehrenberg, 1966, 225-231, and The Etruscan Cities and Rome, 1966, 34 ff. I am not convinced by the arguments of D. Musti, Tendenze nella storiografia romana e greca su Roma arcaica, "Quaderni Urbinati" 10, 1970, esp. pp7 ff., 18 ff., etc., that Dionysius's "autochthonist" point of view was the product of "anti-Etruscan" prejudice.

- (10) Indeed, in the case of the Sabines, we know that the story of the Lacedaemonian "σύνοικοι" was recorded in local histories of the Sabines (Dion. Hal. II.49.4); it is moreover extremely likely that the reference in Dionysius to "ἐπιχωροὶ ἱστορῶν" of the Sabines derives from a citation in Cato's Origines. See below, pp. 343ff., esp. 358.
- (11) Cf. E. J. Bickermann, Origines Gentium (above, n.6) p.73. For A. von Gutschmid this evidence indicated how deeply Greek traditions had penetrated into the native Italian chronicles: "Kl. Schr." V, p.520. Cf. F. Leo, Gesch. röm. Lit. I, p.298.
- (12) Certainly the "Κτίσις Ἰταλίας" of Hippys of Rhegium and the "Περὶ Ἰταλίας" of Antiochus of Syracuse will have dealt only with the extreme Southern tip of the peninsula, because at the time they were writing (the fifth century B.C.) the name "Italia" only extended as far as Lucania (Dion. Hal. I.35.1; Thuc. VI.4, VII.33). As for Polemon and other Hellenistic writers of Ktiseis, it seems likely that they confined their studies to the foundations of Greek cities in Sicily and Southern Italy. The characteristic title "Κτίσεις ... πόλεων" hardly justifies the view that they also treated the origins of barbarian peoples of the interior - with the possible exception of those towns (such as Rome, Capua and the Etruscan cities) which could be described as "πόλεις".
- (13) This is no doubt what Dionysius meant when he wrote (I.6.1) that the first Greek author to touch on the "archaeologia" of the Romans was Hieronymus of Cardia, in his book on the Epigoni - this in spite of the fact that Dionysius himself is our source for references to Rome in earlier writers such as Hellanicus (I.72.2). He meant of course that Hieronymus was the earliest author to consider the Romans worthy of attention for themselves. Of Hellanicus Bickermann writes: "Rome, a place in the far West, known from mere hearsay reports, had no distinct meaning for him. He was interested in Aeneas and not in the Romans" - Origines Gentium (cit. n.6, above) p.66. Cf. the same scholar's valuable remarks in "Riv. Fil. Class." 97, 1969, 398-9.

- (14) F. Jacoby has collected the evidence in F.Gr.Hist. 840, esp. F.7-9.
- (15) Cf. G. A. Mansuelli, I Cisalpini, Florence 1962, p.16 ff., etc.
- (16) F.Gr.Hist. 566 F.69.
- (17) A. D. Momigliano, Secondo Contributo, p.31.
- (18) For this and what follows cf. Bickermann, Origines Gentium (art.cit.).
- (19) F.Gr.Hist. 566 F.36.
- (20) Cic. Cato Maior de Sen. 11.38: "omnia antiquitatis monumenta colligo". Cf. the evidence assembled by H. Peter, HRR I, cxliii.
- (21) Polyb. XII.25h. = F.Gr.Hist. 566 F.34. But for all his "ἱστορία" (Polyb. XII.25g.4), and despite the fact that his account of the foundation of Locri was different from the tradition of the Locrians themselves (Polyb. XII.5.5), nonetheless even Timaeus occasionally recorded native traditions of Italy, for example on the Penates of Lavinium (F.Gr.Hist. 566 F.59; cf. F.36, 51 etc.).
- (22) Cf. F. Della Corte, Catone Censore, p.80 f.
- (23) See above, chapter IV, pp.113ff. and notes.
- (24) The specifically Italic character of the legend of Caeculus, and comparable myths (see below pp.260f), seems established. Cf. C. Koch, RE s.v. "Vesta", 1721 ff; H. Herter, "Rh.Mus." 76, 1927, p.421 ff; id., RE s.v. "Phallos", 1719 ff. etc. The simple story of Caeculus, the founder of Praeneste, who was conceived from a spark from the hearth, should be compared with the version of Zenodotus of Troezen, possibly a contemporary of Cato (Jacoby on F.Gr.Hist. 821), according to whom Praeneste was founded by Praenestus, a grandson of Ulysses (Zenodotus F.1 Jac. = Solin. 2.9).
- (25) In the case of the origin of the Sabines and the foundation of Praeneste we have the explicit statements of Dionysius of Halicarnassus and Solinus

(respectively) that the traditions recorded by Cato were to be found in local histories.

- (26) Dion. Hal. 4.15.1 (= Cato, Origines F.23). The text is that of A. Kiessling (Teubner 1864), on which see E. Gabba, "Athenaeum" 39, 1961, p.104 n.21.
- (27) On the story of Arruns and Lucumo see above pp. 124-127.
- (28) F.32: "Ligures autem omnes fallaces sunt" is almost certainly a less accurate version of the same statement of Cato - see Peter's note ad loc. F31 probably reproduces Cato's exact words.
- (29) e.g. A. Schwegler, Röm. Gesch. I.310; A. von Gutschmid, "Kl.Schr." V,522; A. Rosenberg, Einleitung u. Quellenkunde z. röm. Gesch. 163 ff; F. Della Corte, Catone Censore, p.80 ff. etc.
- (30) Storia di Roma e del mondo romano I. p.30; cf. id. La disunione politica degli Etruschi ("Rend.Accad.Pont." 1931) in "Studi minori di Storia Antica" I, 1958, 293-4.
- (31) A list of direct references is assembled in E. Pais, Storia Critica di Roma, I, 1.(1918), p.91 n.1; cf. G. C. Lewis, An Inquiry into the Credibility of the Early Roman History, 1853, I. 197 f; H. Nissen, Italische Landeskunde, I, 1883, p.20.

CHAPTER VII

(i) "Etruscan Histories": The evidence
of Claudius

Direct evidence about Etruscan historical literature is very limited. The original works have entirely disappeared, and very little is known about the use which was made of them by Greek and Roman writers, because no coherent antiquarian or historical account of Etruscan history has come down to us. We know, however, that such works were written in antiquity; it is sufficient to mention the Res Etruscae of Verrius Flaccus, and the emperor Claudius' Tyrrhenica. But we possess only a very few fragments of these accounts, and not many more of the works of those Roman writers such as Varro, and of course Cato, who endeavoured to examine local Italian traditions.

But there is little reason to doubt the contention that some sort of Etruscan historiography once existed. It seems to be confirmed by reliable evidence. The emperor Claudius, in a famous speech preserved on a bronze tablet from Lyons, contrasted the well known Roman story of king Servius Tullius with a tradition explicitly referred to Etruscan sources. And Varro, in a discussion of the Etruscan theory of "saecula", mentioned certain

(1) For notes to Chapter VII, see below, p. 230-242.

Etruscan histories ("Tuscae historiae") which, he said, had been written in the eighth Etruscan saeculum. Claudius and Varro are trustworthy authorities, and their statements seem to add up to conclusive proof of the existence of an Etruscan historiography. J. Heurgon, for example, has written: "Les Etrusques ont eu, certainement, une littérature historique".⁽¹⁾

This is a statement which very few scholars would call into question. But hardly any attempt has ever been made to consider its implications. Heurgon's little book (cit., n.1) contains the only competent survey known to me of the subject as a whole. Although I agree with most of what Heurgon says, it seems to me that a fuller treatment is both possible and desirable.⁽²⁾ And even Heurgon fails, in my view, to distinguish clearly enough between the various forms in which Etruscan historical traditions could have been handed down.

When Claudius speaks of Etruscan authorities, what exactly does he mean? The relevant part of his speech is as follows:-

"Between this man (sc. Tarquinius Priscus) and his son or grandson (for even in this there is disagreement among writers) came Servius Tullius, who, if we follow our own authorities, was the son of Ocresia, a prisoner of war; if (we follow) the Etruscans he was once a faithful

comrade of Caelius Vibenna, a friend in all his adventures. And being driven out after his luck had turned, he left Etruria with the remnants of Caelius's army, occupied the Caelian hill which he named after his former leader, and, changing his name (for in Etruscan his name was Mastarna), he took the name which I have mentioned (that is, Servius Tullius) and obtained the throne to the great benefit of the state".⁽³⁾

The prima facie implication of this passage is that Claudius had found the Etruscan version of the origins of Servius Tullius in an Etruscan historical book. We know that Caeles Vibenna and Mastarna figured in a popular story which was genuinely Etruscan in origin because they are portrayed in the well known painting of the François tomb at Vulci.⁽⁴⁾ But what is not confirmed by any independent Etruscan evidence is the equation of Mastarna with the Roman king Servius Tullius.⁽⁵⁾

It is possible that Servius Tullius and Mastarna really were the same person, and conceivable that the memory of this fact had been preserved by an old and authentic Etruscan tradition; but it is extremely unlikely that Etruscan tradition should have remembered something of importance to Roman history which had been completely forgotten by the Romans themselves. The Roman annalistic tradition (Claudius's "nostri") knew nothing of Mastarna, or of his supposed identity with Servius

Tullius. It is much more likely that the Etruscan story had happened to record simply that Mastarna had ruled at Rome.⁽⁶⁾ The identification of this Mastarna with Servius Tullius, whether true or not, is likely to have been a secondary reconstruction.⁽⁷⁾ "No one doubts", writes A. Alföldi, "that the equation of the venerable old king of the Roman legend with the Etruscan intruder is an arbitrary contamination".⁽⁸⁾

There are several ways in which this contamination can be explained. It is unlikely that Claudius himself tampered with the Etruscan story by inserting his own conjectures;⁽⁹⁾ it is more probable that he found the tradition in an already contaminated form in his source.

This might mean that Claudius's source was the work of a Roman writer, and that his knowledge of Etruscan sources was indirect. Although one has the impression that Claudius was presenting evidence which he thought would be unfamiliar to his audience of senators, there is no good reason to suppose that he was the first Roman scholar to unearth it; indeed, there is perhaps some evidence to the contrary.

Varro knew of a tradition, which was probably ancient and indigenous, that connected Caeles Vibenna, and his brother Aulus, with Rome:

"In Suburanae regionis parte princeps est Caelius

Mons a Caele Vibenna, Tusco duce nobili, qui cum sua manu dicitur Romulo venisse auxilio contra Tatium regem".⁽¹⁰⁾

The interesting feature of this passage is the chronology. Varro's statement that Caeles Vibenna was a contemporary of Romulus obviously conflicts with the version of Claudius, who placed Caeles Vibenna and Mastarna in the time of the Tarquins and made Mastarna the successor of Tarquin the elder. It appears that Varro either had no knowledge of the story given by Claudius or chose to reject it.

The earliest preserved reference to Mastarna in a Roman work is in a statement of Verrius Flaccus (ap. Festus p.486 L.), and even this is based on a restoration of a missing part of the text:

"Many writers say that the Tuscus Vicus was named after those Etruscans who remained in Rome after king Porsenna had abandoned his siege; and they (who remained) lived in a place which had been allotted to them. Or (it is so called) because it was occupied by the Volcentane brothers Caeles and Aulus Vibenna, who, they say, came to Rome with Mastarna against king Tarquinius"... "aut quod Volci]entes fratres Caeles et [A.] Vibenn[ae quos dicunt ad regem] Tarquinius Romam se cum Max[tarna contulisse eum incolue]rint".⁽¹¹⁾

Unfortunately we cannot be certain what the original

text really said. But the reading "Max(tarna...)" seems to me to derive some support from the fact that Verrius Flaccus dated the arrival of the Vibennae in the reign of Tarquinius. The significance of this becomes evident when we consider the difficulties in which Verrius Flaccus found himself as a result of this chronology. We learn from another passage of Festus that Verrius was unwilling to reject altogether the opinion of Varro that the Mons Caelius was named after a contemporary of Romulus. For this man to have been the same Caeles Vibenna would have been chronologically impossible, so Verrius postulated the existence of two distinct persons, both named Caeles. Caeles Vibenna, he decided, was the man who came to Rome in the time of Tarquin the elder, while Romulus was helped by "a certain Caeles from Etruria" (i.e. not the famous Vibenna):-

"Caelius Mons dictus est a Caele quodam ex Etruria qui Romulo auxilium adversum Sabinos praebuit eo quod in eo domicilium habuit". (Festus p.38 L.).

If Verrius was prepared to go to such lengths to uphold the version of Varro, one wonders why he did not accept it in toto. The only possible explanation is that he had a very good reason to connect the brothers Vibenna with Tarquin. We must therefore assume that Verrius had access to new information about the Vibennae and their relations with Rome.⁽¹²⁾ Verrius, we know, wrote a work

on Etruscan matters - Res Etruscae⁽¹³⁾ - and it seems reasonable to suppose that his new information about the Vibennae came from Etruscan sources, consulted while he was preparing his work on the Etruscans. This conclusion is supported by the independent evidence of the François tomb-painting, which shows that the original Etruscan tradition synchronised these events with the age of the Tarquins, because one of the persons shown in the picture is Cneve Tarchunies Rumach.⁽¹⁴⁾ Claudius's version of the story is rather different from that of Verrius Flaccus because the latter brought Caeles Vibenna to Rome, whereas in Claudius Mastarna arrived after the death of Caeles. This might have been the emperor's own conjecture, or possibly the interpretation of a writer intermediate between Verrius Flaccus and Claudius. If a Roman author was responsible for the equation of Mastarna and Servius Tullius, his purpose must have been to reconcile the Etruscan tradition with the Roman, and to admit Mastarna into the ranks of the Roman kings without causing too much of an upset to the traditional Roman picture of the monarchic age.⁽¹⁵⁾

But it is equally possible that the identification of the two persons appeared for the first time in the work of an Etruscan historian. It cannot be denied that the result of the contamination was rather flattering to

the Etruscans, and it could conceivably have originated because of the desire on the part of an Etruscan writer to 'etruscanise' important parts of Roman history, and in this case to claim as an Etruscan one of the best loved of the Roman kings, the creator of the most important Roman political institutions. (16)

It is worth mentioning that Claudius was in an exceptionally good position to consult Etruscan sources directly. This has been well argued by Heurgon. (17) By means of a prosopographical survey, Heurgon demonstrated that Claudius was closely connected by personal ties with the leading Etruscan aristocracy of his day; his first wife, Urgalanilla, came from a family which had maintained its national character by alliances of marriage with other families of Etruscan origin. The remarkable fact that these noble families had managed to survive for so long, and the tenacity with which they had held on to their Etruscan identity, might lead one to conjecture that they were a vehicle for the preservation and transmission of their national historical tradition. And it so happens that the conjecture can be confirmed. The "elogia Tarquiniensia", which will be discussed more fully below (pp.192-9), provide independent evidence of the fact that Etruscan noble families, even in the time of the principate, kept alive the memory of their national history, and

publicly paraded the achievements of their ancestors. In these circumstances, Claudius could hardly have ignored native sources when collecting material for his Tyrrhenica. Heurgon protested against the notion that Claudius relied solely on the reports of Roman antiquarian writers:

"Voire! Petit-gendre, gendre, beau-frère de quelques-unes des représentantes de l'aristocratie étrusque du temps, dont sa scrupuleuse endogamie dénonce la fierté ethnique et la fidélité à ses souvenirs, il n'a eu qu'à puiser directement aux sources".⁽¹⁸⁾

But the precise character of the Etruscan source, on which the equation Mastarna=Servius Tullius was based, is a matter for conjecture. It need not have been a written historical work. It could have been a book or document of some other kind; but it is equally likely that the original Etruscan source was nothing more than a firmly established and widely diffused oral tradition.

We know that the legend of the Vibennae was popular and widespread, since traces of it are found not only in the painting from the François tomb at Vulci and in the native Roman tradition,⁽¹⁹⁾ but also on funerary urns from near Chiusi,⁽²⁰⁾ and on a bronze mirror from Bolsena.⁽²¹⁾ The name "Aules V(i)pinas" appears on a fifth century red figure cup by an Etruscan artist who imitated an Attic cup of the school of Duris;⁽²²⁾ and in a temple at Veii a

votive offering was found in the form of a bucchero vase bearing the inscription "Avile Vipiiennas"; this object is dated to the middle of the sixth century B.C., and was perhaps offered by Aulus Vibenna in person. (23)

Thus, the presence of the brothers, or at least of traditions about them, is attested at Vulci, Rome, Clusium, Veii and Volsinii. It could be fairly said that the legend of the brothers Vibenna is better attested in Etruria than any other native story. It has even been suggested that Caeles and Aulus Vibenna, and other 'condottieri' of the same type, were celebrated in popular poetry; (24) that their exploits formed a widely known popular tradition can scarcely be doubted.

But it is likely that by the first century A.D. such Etruscan legends had been distorted by the influence of the Roman historical tradition. We might reasonably assume, a priori, that after the Roman conquest of Etruria the Etruscans' view of their own past developed a tendency to exaggerate the importance of events in the archaic period of their history which involved the Romans, and to stress the part they themselves had played in the formation and growth of Rome. This could explain the identification of Mastarna with Servius Tullius. There is no reason, therefore, why we should not take Claudius' phrase "si Tuscos (sequimur)" at its face value. But it does not

necessarily furnish a prima facie argument for the existence of Etruscan historical literature.

.....

(ii) "Etruscan Histories": The testimony of Varro

The evidence of Varro, however, presents us with something more substantial. The passage in question derives from Censorinus (De die natali 17,6):-

"Quare in Tuscis historiis, quae octavo eorum saeculo scriptae sunt, ut Varro testatur, et quot numero saecula ei genti data sint et transactorum singula quanta fuerint quibusve ostentis eorum exitus designati sint continetur".

This passage cannot be treated in quite the same way as the extract of the Table of Claudius, who was probably referring to oral tradition,⁽²⁵⁾ because Varro stated explicitly that the Tuscae historiae were written ("scriptae sunt"). A consideration of the purely internal indications of the passage as it stands suggests the following observations:

The author of the Tuscae historiae is not named, which is odd if a literary composition is referred to. This implies one of two things:

(i) Varro was referring not to a single book but to a group of works by several authors. This explanation

is advanced by Heurgon, who adds in its favour that Varro spoke of "Tuscae historiae" in the plural - which may not be a very compelling argument.⁽²⁶⁾ Niebuhr seems to have taken a similar view, for he postulated a series of Etruscan annalists, akin to the Roman.⁽²⁷⁾

(ii) Alternatively, the absence of a writer's name might suggest that Varro was not in fact referring to a literary work in the normal sense, but rather to a non-literary document or group of documents, similar perhaps to the Roman Fasti or Annales Maximi.⁽²⁸⁾ Any difficulty there might be in Varro's statement that they were written in the eighth Etruscan saeculum can be overcome if we assume that the documents were first collected and published in the eighth saeculum, which was roughly equivalent to the second century B.C.⁽²⁹⁾ This date, vague though it is, inevitably calls to mind the development of Roman historiography, and especially the publication of the "Annales Maximi", probably by the Pontifex Maximus Mucius Scaevola at the end of the second century.

The possibility of a connection between the Annales Maximi and the "Tuscae historiae" has sometimes been suggested, notably by Heurgon, who suggests that the one might have given rise to the other - i.e. that the "Tuscae historiae" were produced in emulation of the Roman "Annales Maximi".⁽³⁰⁾

The historical consciousness of the Romans, which awoke at the beginning of the second century B.C., and the concentration of attention by Roman historians on the history of their own city, might have impelled the other Italian peoples to undertake an examination of their own past; the possibility cannot be excluded that the Etruscans began to write local histories in order to impress on the Romans the fact that their achievements in early times were no less great than those of Rome. This kind of composition could have been inspired by Cato's Origines, written in the middle of the second century, - a work which differed from its predecessors in that it dealt with the history of the Italian peoples as well as of Rome.

The possibility that the *Tuscae historiae* were the work of a Roman, writing in Latin, who reconstructed the Etruscan past for Roman readers, seems to be ruled out by the absence of an author's name, which is surely evidence of their Etruscan origin.

But there is one further possibility. It is important to realise that the passage in question is not strictly speaking a "fragment" of Varro at all, because it does not reproduce Varro's exact words. There must be a chance that Censorinus misunderstood what he had read in Varro. It is quite clear that Varro referred to a written

composition, but it may be that he was actually speaking about some other kind of work; he may, for example, have written "libri Etrusci" - which Censorinus understood to be historical because of the nature of the passage quoted. Varro may have derived his knowledge of the Etruscan saecula from those books whose existence is well attested, namely the sacred books containing the body of doctrine known as the "Etrusca disciplina".

Clearly none of these hypothetical reconstructions can be confirmed for the simple reason that no direct evidence exists whatsoever apart from the single passage under discussion. However, it seems to me that whatever view one takes of the Tuscae historiae, a publication of non-literary documents, a literary reconstruction of Etruscan history from a variety of sources, or a misquoted reference to the books of the "Etrusca disciplina" - one fundamental question is in any case bound to present itself.

Whatever may have been the precise nature of the Tuscae historiae, it is clearly necessary to ask what sort of primary historical evidence they were based on. An answer to this question might enable us to form a more precise picture of their character, because the form and content of a historical work are clearly dependent to some extent on the nature of the sources which the historian has at his disposal. The question can be put thus: if an

Etruscan scholar were to try to write a local history, where would he be able to find the necessary raw materials? This question still has meaning even if we take the most sceptical view possible of Varro's mention of "Tuscae historiae". Varro (and Claudius) may or may not mean that Etruscan history was established in a literary tradition; in any case we would want to know whether there existed in the Etruscan cities any systematic documentary records containing detailed and authentic historical information.

.....

(iii) Family tradition in the Etruscan cities

Evidence for written records of a historical nature, going back to a very early date, is provided, in the first place, by the so-called "elogia Tarquiniensia". These inscriptions were discovered shortly after the war during excavations at Tarquinia, and since then they have been discussed many times.⁽³¹⁾ Written in Latin, they were at once recognised as "elogia", set up in honour of famous persons from the past history of the city.

The style of lettering suggests that the inscriptions are of early imperial date, and this is supported by the fact that they are similar in character to the elogia set up at Rome in the Forum of Augustus in 2 B.C.⁽³²⁾ Elogia

of the same period have been found elsewhere in Italy, notably at Arezzo and Pompeii.

The distinctive feature of the *elogia Tarquiniensia* is that they celebrate persons and events of the history of Tarquinia, in contrast to the series of famous Romans, from Aeneas to the end of the Republic, honoured in Rome and other Italian cities. It is clear that at Tarquinia feelings of local patriotism were still strong even at the time of the early Roman Empire.

As far as we are concerned,⁽³³⁾ the significant implications of the *elogia* of Tarquinia can be summarised as follows: first, some of the inscriptions refer to events which took place at a very early date.⁽³⁴⁾ One of them⁽³⁵⁾ mentions a king of Caere, which indicates that the person who is the subject of the *elogium* must have lived at a time when there were still kings at Caere; the date of the fall of the monarchy at Caere is not known, but it is probably to be placed during the general constitutional crisis which affected the Etruscan cities at the end of the sixth century B.C., and which is symbolised in our tradition by the expulsion of the Tarquins from Rome.⁽³⁶⁾ At any rate we know that most of the Etruscan cities were governed by republican constitutions by the end of the fifth century, because Livy says that their hatred of monarchy was one of the reasons why the other

Etruscan cities did not send aid to Veii, where the monarchy had been restored in 403 B.C.⁽³⁷⁾ Another elogium⁽³⁸⁾ concerns a Tarquinian magistrate who "primus... Etruscorum mare...traiecit". However this sentence is to be restored⁽³⁹⁾, and whatever may be the historical implications of "primus",⁽⁴⁰⁾ this maritime expedition must surely have taken place at a relatively early date, perhaps in the late fifth or fourth century B.C.⁽⁴¹⁾

Secondly, the elogia contain detailed information about the careers of the persons they honour. Let us consider first the career of the subject of no.77, which we know to be early: the person or persons who composed the text in the first century A.D. knew not only that this man had had dealings with the king of Caere and had fought a war against Arretium; they also knew the number of fortresses he had captured from the Latins (or Arretines).⁽⁴²⁾ The last line, where the restorations must be essentially correct, reads:

"[de La]tinis novem o[ppida cepit]"

or "[de Arre]tinis novem o[ppida cepit]"

Detailed information of this kind can hardly be attributed solely to oral tradition, and the general character of the elogia seems to suggest that they were based on written sources. This impression emerges even more strongly from the best preserved and most informative

of the *elogia* - no.48.⁽⁴³⁾ This concerns a man who had twice held the office of praetor,⁽⁴⁴⁾ who had led an expedition to Sicily, and who had been awarded a golden crown. The interesting point about this text, as Pallottino has emphasised,⁽⁴⁵⁾ is that the praetor is not credited with any positive achievement. We are told simply that he commanded an army and led an expedition to Sicily. No battle is mentioned, nor is there room in the lacunae for such a mention to be restored.⁽⁴⁶⁾ It is extremely unlikely, for that reason, that a victory was referred to in the last line of the text.⁽⁴⁷⁾ It seems, then, that the *elogium* is dealing with a praetor who did nothing in his first, and little of any real significance in his second period of office. And yet, despite the passage of several centuries,⁽⁴⁸⁾ the compilers of the *elogium* in the first century A.D. were able to enumerate the details of his career. The inevitable conclusion is that this information was recorded in a written document or collection of documents, as there is no other way in which such unmemorable events could have been handed down. Moreover, the relative insignificance of the achievements attributed to this Etruscan magistrate constitutes a telling argument against the only possible alternative - that the *elogia* were simply the product of free invention.

It can be said, therefore, that the *elogia Tarquiniensia* must imply the existence of written documents surviving

from a very early period. These documents must have recorded the names of magistrates and at least something about what they did. It is tempting to think in terms of a local chronicle, originally in the form of a list of magistrates with additional notes attached to the names, comparable to the Roman Fasti (perhaps later to be established in a literary form, as the Tuscae historiae?). A chronicle of this type could undoubtedly have furnished the necessary material for the compilers of the *elogia Tarquiniensia*. We shall see, moreover, that there is some independent evidence from Tarquinia for a list of eponymous magistrates, the necessary precondition of any type of yearly chronicle.

There is, however, a second possibility. Some evidence has recently come to light which suggests that the major source of the *elogia Tarquiniensia* may have been private archives of individual aristocratic families rather than an official public record. This conclusion is based on what is now known of the motives which led the citizens of Tarquinia to set up the *elogia* in the first place.

In a recent article,⁽⁵⁰⁾ M. Torelli published a new fragment of the most important of the *elogia* (no.48). This fragment revealed part of the name of the person who was the subject of the *elogium*; it is now beyond doubt that he was a member of the gens *Spurinna*.

Pallottino had already observed⁽⁵¹⁾ that several of

the fragments published by Romanelli bore the name of members of this gens,⁽⁵²⁾ and he had suggested that they too might have been fragments of *elogia*. Torelli's discovery seems to confirm this hypothesis, and one is led on to imagine a series of *elogia* enumerating the achievements of members of this family.⁽⁵³⁾ The significance of the *Spurinnae* is that they were an important family at the time of the early empire: one of them, *Vestricius Spurinna*, was a man of consular rank whose career is relatively well known.⁽⁵⁴⁾ If we add to this the fact that another *elogium* from *Tarquinius* bears the name of a *Caesennius*,⁽⁵⁵⁾ another influential family during the Principate, several members of which reached the consulship,⁽⁵⁶⁾ we can begin to see the emergence of a pattern. For this evidence implies that the publication of the *elogia* was not simply the product of sentimental recollections of past glories by the people of *Tarquinius*,⁽⁵⁷⁾ nor merely the result of research undertaken by antiquarian scholars.⁽⁵⁸⁾ Rather, it seems that the *elogia* were set up in honour of the ancestors of the leading *Tarquinius* gentes of the time - more specifically, of those families which formed part of the new senatorial aristocracy of the early Roman Empire.

These Etruscan senators, who were relatively new arrivals on the Roman scene, clearly felt a desire to

distinguish themselves from other men who owed their position ^{to promotion} from a more humble level. It seems not unreasonable to suppose that the elogia Tarquiniensia were set up by men who wanted to show the Romans that they were not "new men" in the ordinary sense: they wished to emphasise their ancient and noble family origins, and to show that their ancestors were no less illustrious than those of the old Roman nobility. (59)

The possibility that the leading Tarquinian aristocrats of the day undertook the publication of the elogia strongly suggests that the information they contain came from the private archives of those families. In Rome, family archives consisted of portraits of a nobleman's ancestors which were regularly produced at family funerals. (60) Accompanying these portraits were documents in which a record was kept of their magistracies and achievements, as Pliny tells us (n.h. xxxv. 6-7):-

"tabulina codicibus implebantur, et monimentis rerum in magistratu gestarum".

A. Gellius also tells us that he himself was able to consult the records of the gens Porcia when he was checking up on the ramifications of the family of Cato the Censor:-

..."cum et laudationes funebres et librum commentarium de familia Porcia legeremus" (61).

The "laudatio funebris" was an encomiastic speech delivered at a family funeral - a ceremony which Polybius describes for us.⁽⁶²⁾ The speech contained an account, according to Polybius, of "τὰς ἀρετὰς καὶ τὰς ἐπιτετευγμένους ἐν τῷ ζῆν πράξεις" of the deceased. This is borne out by a papyrus fragment, recently published by L. Koenen,⁽⁶³⁾ which contains a Greek translation of the "laudatio funebris" delivered by Augustus at the funeral of Agrippa. This document is important for the student of historiography because it shows that a "laudatio funebris" gave an account of the public career of the dead man with very much more chronological and substantial detail than Polybius' "ἀρετὰς καὶ πράξεις" might suggest. A family archive, therefore, consisted mainly of a series of former "laudationes", as Gellius informs us, whose contents must have provided a major historical source.

Obviously, records of this type could have provided the necessary material for the *elogia Tarquiniensia*; there is no reason why the noble families in the Etruscan cities should not have kept records similar to the Roman. The *elogia* in themselves show that a desire to glorify one's ancestors was characteristic of the aristocratic gentes, and they can surely be considered strong evidence for the preservation of substantial family archives.

(iv) Local chronicles in the Etruscan cities

I have already mentioned the possibility of local chronicles in the Etruscan cities, and it will be well to consider this subject further. Here again we have no direct evidence, but if we postulate, a priori, the existence of local Etruscan chronicles on the Greek pattern, it is necessary to examine the evidence for signs of conditions which the writing of local chronicles inevitably presupposes; this means, above all, a systematic method of recording the passage of time. The fundamental basis of a Greek local chronicle is a documentary list of eponymous officials, together with brief annotations of annual events, usually of a perfunctory character, which concern the office whose representatives are enumerated as eponyms - an activity which develops into the recording of annual events on an increasing scale until a regular chronicle is formed.⁽⁶⁴⁾

Unhappily we know very little about the method of measuring time in Etruria, and the evidence we have is not conclusive. But there are two important pieces of evidence which show that eponymous dating was probably used in at least some of the Etruscan cities. This had been argued by E. Vetter,⁽⁶⁵⁾ on the evidence of inscriptions from the tomba degli Scudi at Tarquinia, and has since been generally accepted.⁽⁶⁶⁾ A painting from the tomb

shows a demon writing on a diptych (two tablets joined together), and the text which appears on it reads:

"zilci Velus Hulchniesi Larth Velchas Velthurs
Aprthnals clan sacnisa thui eith suthnith acazr"⁽⁶⁷⁾

The meaning of this text is well enough established as:

"In the magistracy of Vel Hulchnie, Larth Velcha son of Velthur and Aprthni received in this tomb the funeral honours"⁽⁶⁸⁾

This is undoubtedly a clear instance of dating by means of an eponymous magistrate. It may be objected that this could be an isolated case which does not necessarily imply the widespread use - even in the city of Tarquinii - of eponymous dating. But it must be realised that the eponymous method cannot allow of isolated usage, because dating by the name of a magistrate is meaningless in chronological terms if that magistrate cannot readily be related to a yearly list of magistrates. Mere names on their own cannot indicate duration of time or relative chronology. In other words, the text quoted above must surely indicate that a list of annual eponymous magistrates existed in Tarquinia.

Further evidence of the use of eponymous dating by the Etruscans is furnished by the now famous Pyrgi inscription. The text, which is a temple dedication, mentions the ruler of Caere, Thefarie Velianas,⁽⁶⁹⁾ and

states that the temple was dedicated in his third year. It seems to indicate, therefore, that at Caere during this period (the early fifth century B.C.), events were dated by regnal (or magisterial) years. This method, if continued, inevitably develops into normal eponymous dating with the introduction of annual magistrates in place of kings (or long-term magistrates). Again, if there is evidence at Caere for the use of eponymous dating, the inevitable conclusion is that there existed a list of eponymous magistrates. The existence of a list might also suggest the existence of a chronicle, although this cannot yet be confirmed by any evidence. Nevertheless, a list of magistrates is a vitally important historical document and is in itself a primitive form of chronicle.

Equally interesting, however, are certain indications that the Etruscans had a system of dating other than the use of eponyms. The only possible alternative to the eponymous method is the use of an 'era' - that is to say, the dating of events by the number of years before or after an epochal event. In Etruria, the era is to be connected with the system of saecula. In simple terms, the saeculum was conceived of as being concurrent with the lifetime of a person; a saeculum was reckoned to end with the death of the last surviving person to have been alive at the time when the saeculum began. The Etruscan

nation was thought to have been destined to last for ten saecula.

Such a system obviously required the recording of the passage of time, and, in view of the religious character of the concept of saecula, it would seem that the business of recording time was supervised by priests. But the most interesting fact about the saecula is that the majority of our knowledge is derived from Varro, and from the very same passage in which he refers to Tuscae historiae. This obviously suggests that the Tuscae historiae were arranged within a 'saecular' framework: that is to say, their chronology took the form of an indication of which year of which saeculum saw the occurrence of a given event. Exactly comparable in this respect is the Greek system of Olympiads; and, as with the Greeks, it is quite possible that the Etruscans used two chronological systems - the 'saecular' and the eponymous - side by side.

The supposition that the Tuscae historiae were arranged according to saecula cannot be certain because the passage of Varro cannot be said to imply it directly; but the possibility is nevertheless strong. Moreover, the passage is bound to suggest that the Tuscae historiae were connected in some way with the Etruscan religion in general, and it raises the question, to be discussed below, of whether or not the recording of time by means of saecula

also involved the recording of events - i.e. the keeping of a priestly chronicle.

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(v) Priestly traditions in the Etruscan Cities

The content of the 'fragment' of Varro inevitably suggests a connection between the Tuscae historiae, whatever they were, and the Etrusca disciplina. The relationship is strongly emphasised by Heurgon, whose opinion of the Tuscae historiae, as far as I understand it, is that they were so closely dependent on the Etrusca disciplina that they can be regarded as part of the sacred literature of the Etruscans, along with the "libri rituales". He writes:-

"Les Tuscae historiae anonymes, que nous connaissons par Varron, rentrent aussi, et davantage encore, dans la catégorie de la littérature sacrée... Toute porte à croire que le texte original était en étrusque, comme d'autres libri rituales",

and again:-

"Les histoires étrusques, qui étaient venues à la connaissance de Varron...(étaient)...en étroite dépendance de l'Etrusca disciplina"⁽⁷⁰⁾

Whether or not Heurgon's remarks are valid, the question they imply is an important one. One wants to

know if the *Etrusca disciplina*, which incorporated a body of sacred texts, some of which undoubtedly contained archaic material, was instrumental in preserving historical tradition.

If the Tuscae historiae are in any sense a true parallel of the Roman Annales Maximi,⁽⁷¹⁾ then we must presume that the priests in the Etruscan cities kept an annual record. At Rome the pontifical "tabulae" were connected with the calendar, which was supervised by the pontiffs; the most plausible reconstruction of their character is that the "tabula apud pontificem maximum" was a publication of the calendar - divided into months and days - on which the festivals and ceremonies were entered, together with a record of events of a religious significance, such as eclipses and famines,⁽⁷²⁾ which required the performance of a specific religious ceremony.⁽⁷³⁾

We have no evidence that annual calendar tables were set up in Etruria, and day by day events recorded, as at Rome.⁽⁷⁴⁾ But one wonders whether the priests in the Etruscan cities kept any kind of record of their activities. The books of the *disciplina*, especially the *libri haruspicini* and the *libri fulgurales*, must have contained information about the interpretation of omens, signs and portents of various kinds, and this in itself suggests that they may also have recorded past observations of this nature.

Haruspical divination was based on a tradition recorded in sacred books and instruction passed on from father to son among the noble families. (75) What we would like to know is whether this tradition was based on precedent - i.e. whether it was a "science" depending on empirical observation and evolving out of the recording of signs and the noting of subsequent events. Cicero seems to suggest that this was the case:-

"Quae vero aut coniectura explicantur aut eventis animadversa ac notata sunt, ea genera divinandi, ut supra dixi, non naturalia, sed artificiosa dicuntur; in quo haruspices, augures coniectoresque numerantur. ... Quorum alia sunt posita in monumentis et disciplina, quod Etruscorum declarant et haruspicini et fulgurales et rituales libri, vestri etiam augurales". (76)

The possibility that the Etrusca disciplina included a corpus of recorded and classified observations makes it likely that the sacred texts also contained historical allusions. This likelihood is increased by the fact that historical traditions are known to have been preserved in this way in ancient Mesopotamia. The Babylonian omen text^s, which survive in late copies, have been shown by A. Goetze (77) to contain authentic traditions which go back to the time of the events themselves. This conclusion is confirmed by the findings of J. J. Finkelstein, (78)

whose analysis of the traditions of the Akkadian dynasty shows that the omen texts are a more reliable source of genuine information than the legends and popular traditions about the same events. The omen texts were used for the purposes of instruction, i.e. they were intended to show what kind of events could be expected if certain signs were observed.⁽⁷⁹⁾

The similarities between the religions of Etruria and the Ancient Near East have been discussed many times; and especially the matter of divination by means of extispicy seems to indicate a connection between them.⁽⁸⁰⁾ The comparison has naturally been thought to have a bearing on the controversial question of Etruscan origins, and to suggest that the antecedents of the Etruscan religion are to be sought in the East.⁽⁸¹⁾ It is not necessary, however, to discuss this vexed problem; it will be quite sufficient to mention the Babylonian and Assyrian omen texts as a parallel case, because it is obvious that a method of divination which claims for itself a rational or scientific basis must inevitably rest on a foundation of recorded precedent.

It would therefore seem likely, a priori, that the books of the *Etrusca disciplina* contained allusions to past events, and this assumption is borne out by the available evidence.

Pliny the elder, in a discussion of thunder and lightning, tells us that the Etruscan city of Volsinii was once completely burned down by a thunderbolt.⁽⁸²⁾ A few sentences later, he writes:-

"Exstat annalium memoria sacris quibusdam et precationibus vel cogi fulmina vel inpetrari. vetus fama Etruriae est inpetratum Volsinios urbem depopulatis agris subeunte monstro quod vocavere Oltam, evocatum a Porsina suo rege".⁽⁸³⁾

This passage gives us a glimpse of Etruscan history preserved by native tradition, and its survival is clearly due to the religious significance of the events, so that the ultimate source is likely to have been a priestly tradition. Pliny's immediate source is not in doubt. Among the authors listed in the index of the second book of the 'Natural History', Pliny mentions "Caecina de Etrusca disciplina, Tarquitius qui item, Julius Aquila qui item". And at the beginning of II.53 (the chapter on the origins of thunderbolts) Pliny names as his source "Tuscorum litterae", and again "Etruria...arbitratur..."

We know nothing about Julius Aquila, but Caecina and Tarquitius Priscus were well known Etruscan experts of the late Roman Republic who introduced the Etrusca disciplina to the Romans by translating the books into Latin. Tarquitius Priscus, moreover, translated into Latin a collection of supernatural events ("ostentaria Tusca"),

which were undoubtedly based on some sort of documentary record probably going back a long way into the past.⁽⁸⁴⁾ It seems to me that Pliny's phrases "Tuscorum litterae" and "Etruria...arbitratur..." refer to authentic documents in Etruscan which recorded observations of supernatural signs. We know, again on the authority of Pliny, that the sacred books of the disciplina did contain historical events, and that newly observed phenomena continued to be added until a relatively late date:-

"Factum est semel, quod equidem in Etruscae disciplinae voluminibus invenio, ingens terrarum portentum, L. Marcio Sexto Iulio coss. (= 91 B.C.) in agro Mutinensi".⁽⁸⁵⁾

But what is otherwise known of the sacred books might seem at first sight to conflict with this interpretation. We read in Cicero⁽⁸⁶⁾ that the libri haruspicini originated from a speech made by the semi-divine Tages, who sprang one day from the soil of Tarquinia and revealed to the Etruscans the art of extispicy; again, the libri fulgurales were thought to have contained the teachings of the nymph Vegeia. These stories, together with other evidence,⁽⁸⁷⁾ suggest that the books were of a purely prescriptive character and can hardly have contained a catalogue of recorded precedent, which increased with the passage of time.

It is necessary, however, to take into account the

nature of our surviving sources; writers who refer to the *Etrusca disciplina*, such as Cicero, Seneca, Pliny and John Lydus, were interested in the religious significance of what was recorded in the books, and not in any historical information they may have contained. And the fact that the *Etrusca disciplina* was held to be derived ultimately from the revelations of mythical figures such as Tages and Vegoia does not preclude the possibility that the texts also contained a record of observed happenings which bore out the validity of the original doctrine. Indeed, Cicero himself confirms this assumption, when he says that the original Tagetic text was continually added to in the light of new knowledge:-

"Tum illum (sc. Tages) plura locutum multis audientibus, qui omnia verba eius exceperint litterisque mandarint; omnem autem orationem fuisse eam, qua haruspicinae disciplina contineretur; eam postea crevisse rebus novis cognoscendis et ad eadem illa principia referendis". (88)

That these additions included a recording of events is proved, as we have seen, by the evidence of Pliny. There can be no doubt, in fact, that many of the local Etruscan traditions known to us, including the theory of *saecula* attributed by Varro to *Tuscae historiae*, are closely connected by their strong religious emphasis with the tradition of the *Etrusca disciplina*.

Cicero's source for the story of Tages was almost certainly the De Etrusca Disciplina of A. Caecina, a man with whom Cicero was personally acquainted.⁽⁸⁹⁾

Caecina's De Etrusca Disciplina certainly contained historical and legendary traditions, as is shown by a fragment of it fortunately preserved by a scholiast on Vergil:-

"Caecina...Archon, inquit, cum exercitu Appenninum transgressus primum oppidum constituit quod tum Mantuam nominavit vocatumque Tusca lingua a Dite patre est nomen. Deine undecim dedicavit Diti patri...ibi constituit annum et item locum consecravit, quo duodecim oppida condere...-nem".⁽⁹⁰⁾

Heurgon supposes that this fragment was taken from a historical work by Caecina, written in Latin but derived from native sources.⁽⁹¹⁾ However, as Heurgon himself observes, Caecina emphasised the religious aspects of Tarchon's activities, and it seems to me that this points in the direction of the De Etrusca Disciplina rather than a hypothetical historical work for which there is no supporting evidence. This conclusion is supported by the description, by Verrius Flaccus, who himself used Caecina,⁽⁹²⁾ of the libri rituales":-

"Rituales nominantur Etruscorum libri in quibus perscribitum est quo ritu condantur urbes, arae aedes

sacrentur, qua sanctitate muri, quo iure portae, quomodo tribus curiae centuriae distribuuntur, exercitus constituentur ordinantur, ceteraque eiusmodi ad bellum ac pacem pertinentia". (93)

It seems to me that the fragment of Caecina corresponds fairly precisely to the content of the libri rituales as defined by Verrius Flaccus. The story of Tarchon's foundations in Etruria Padana could have been recorded in the libri rituales themselves, but it could equally well have been recounted in Caecina's commentary on the books.

What all this evidence amounts to is that Etruscan legends and history were preserved in a religious tradition which must have relied to a great extent on the recording of events which were thought to be a consequence of signs and prophetic utterances. Whether these events were recorded for the most part in the sacred books themselves, or whether they were recorded in a comprehensive catalogue in the form of a priestly chronicle which lay outside the central body of the Etrusca disciplina cannot be known. It is also uncertain whether the Tuscae historiae mentioned by Varro were based entirely on a systematic priestly chronicle, or whether they were a literary reconstruction using a variety of sources, including religious documents.. In any case, it seems fairly certain that the Tuscae

historiae and the books of the Etrusca disciplina were closely linked.

.....

(vi) Conclusion

To sum up, it may be said that we still know nothing certain about the Tuscae historiae apart from what Varro tells us. Claudius probably never came into direct contact with historical works written in Etruscan. From Varro we can infer that they were genuinely Etruscan in origin, that they were written in the eighth saeculum, and that they contained a discussion of the saecula. Any further conclusions we might form about the Tuscae historiae must be based on what we know of the available primary sources.

In Etruria, as well as in Rome, we may presume that primary historical evidence could have survived in texts of laws, treaties and other official documents, paintings and sculptured monuments, popular poetry, accounts of Greek historians, and, of course, a rich oral tradition. But apart from these rather haphazard sources, we have been able to indicate three areas in which an Etruscan historian could have found systematic records containing authentic historical material.

(a) The evidence of the elogia Tarquiniensia implies that an ample tradition was preserved in private family

archives which still survived at the time of the Principate.

(b) A list of magistrates existed at Tarquinia and perhaps also at Caere; these will have formed an important historical source in themselves, but they also open up the intriguing possibility of local city chronicles.

(c) The Etruscans seem to have kept records of events of a religious significance. This conclusion is based on the assumption, which I think is reasonable, that the profoundly serious business of divination required a systematic body of recorded precedent, in the form not only of a record of the observed signs, but also of the events which were thought to be a consequence of the signs. Clearly, these events would often have been historically important.

(vii) Appendix: A Note on Harris:
"Rome in Etruria and Umbria" *

ROME IN ETRURIA AND UMBRIA. By W. V. Harris. Oxford at the Clarendon Press. 1971. 370 pp. Bibliog. Index. £6.50

This book traces the history of Roman policy in Etruria and Umbria from the wars of conquest to the beginning of the Principate. A historical outline forms the background for a more detailed study of certain special aspects of Etruria and Umbria under Roman domination, with chapters on the foedera, on Etruscan society, and on romanization.

The principal thesis is that while Rome's formal relationship with the Etruscan and Umbrian communities was represented by foedera (here the author develops arguments first expressed in "Historia" 14, 1965, pp. 282-92), she was able to exercise effective control by maintaining the power of the local principes in exchange for their loyalty. In this generalised form the conclusion is unexceptionable - and hardly novel, as Harris himself recognises (p. 130 n.1, 143 n.7 etc.); but he goes on to argue that the policy was especially appropriate to the situation in the Etruscan cities, where the position of the local ruling families was constantly threatened by disaffection among their slave-like subjects. The success of the Romans in

* This section contains the English text of a review which is shortly to be published in Italian in "Rivista Storica Italiana".

exploiting this situation for their own ends is illustrated, Harris believes, by the fact that the Etruscans remained consistently faithful to the cause of Rome. He also points out that there was only limited Roman colonization in Etruria, compared with other areas of Italy, and infers that the voluntary co-operation of the Etruscan principes made it unnecessary.

Harris's clear and detailed presentation of the facts is welcome - especially his reassertion of the view that the Etruscans remained loyal to Rome in the Second Punic War (pp.131-143) - but his explanation of them is not entirely convincing. We are asked to believe that the history of Romano-Etruscan relations was largely determined by Rome's exploitation of the tensions inherent in the archaic social structure of the Etruscan cities. This seems to me to be questionable for two reasons. First, one might be forgiven for seeing a paradox in the idea that precisely because of its propensity to unrest, "Etruscan society offered the conquerors a more economical method of ensuring security than colonization" (p.98). Secondly, it must be remembered that in the Gallic War of 225 B.C., and in the Hannibalic and Social Wars, the behaviour of the Etruscans was indistinguishable from that of other Italian peoples who were not hampered by the same domestic social problems. The Umbrians in particular are

almost always reported to have acted in the same way as their Etruscan neighbours in times of general crisis (NB especially Appian B.C.I. 163 and 206). This must weaken the main hypothesis, because domestic circumstances peculiar to the Etruscan cities can hardly be the sole explanation of events which involved the Umbrians and others as well as Etruscans.

Harris gives us a disappointing account of the Etruscan social system. The survey of the epigraphic evidence (pp.124-129) is confused and unhelpful. It is difficult to understand why the inscriptions referring to lautni are supposed to be "confirmation of the literary evidence" (pp.126, 129 etc.). If Harris right to reject the equation Lautn = familia (p.127), then there is no reason to distrust the prima facie evidence of CIE 1288 and 3692 - bilingual texts in which the word lautni is rendered l(ibertus). This suggests that we are dealing with a group of affranchised persons, who need not be identical with any particular group mentioned in the literary sources. The fact remains that the picture of a deep social division in Etruria rests exclusively on the reports of our literary authorities. But the inferences Harris draws from this literary evidence are too cautious, and make little significant advance on earlier studies.

Harris takes the view (rightly, in my opinion) that

the archaic social structure of the Etruscan cities survived at least until the time of the Social War (pp.202-212). Although this must have been due partly to Roman support for the local ruling classes, it is perhaps worth adding that the Etruscans seem to have been remarkably tenacious in preserving their ancient culture (cf. S. Mazzarino, "Historia"6, 1957, p.119 f.). Harris notes that before the Social War romanization proceeded slowly in Etruria by comparison with other parts of Italy, and connects this with the fact that the Romans founded relatively few colonies there; but it is also important to remember that the Etruscans possessed a highly developed urbanised culture of their own, which had flourished long before the Roman conquest; (94) this, together with the strong conservative tendency just mentioned, must have hindered the process of romanization.

The linguistic evidence, usefully assembled on pp.169-187, tells the same story. This evidence consists entirely of inscriptions, and is very difficult to interpret correctly. Harris argues that there was little widespread use of Latin - or even bilingualism - in Etruria before the first century B.C. (p.175), and that none of the bilingual inscriptions known to us can be definitely assigned to the period before the Social War (pp.176-7). The latest evidence for the use of Etruscan in inscriptions

dates from the time of Augustus (p.179). This leads to the important conclusion that, as far as the inscriptions are concerned, the transition from Etruscan to Latin occurred in the decades following the end of the Social War (p.182).

But considerable doubt must remain about precisely what kind of social change this represents. On the one hand we might argue, with Harris, that Etruscan could have survived in funerary inscriptions long after it had passed out of everyday use (pp.169, 180 etc.); but it could be said on the other hand that the transition from Etruscan to Latin may have happened much earlier among the wealthier literate classes (the only group to be represented in inscriptions) than among the majority of the population (cf. Mazzarino, art.cit. pp.98-9). Harris's arguments ^{against} this latter contention (pp.183-4) are not altogether convincing.

.....

The first chapter, on the sources, is more than an introduction. In the opinion of the present writer it is the most adventurous and interesting part of the book, and the important issues it raises require more detailed discussion.

In recent years our knowledge of Etruscan civilisation has increased considerably as a result of intensive

linguistic and archaeological research. But scholars in these fields have not yet succeeded in making the Etruscans speak to us directly and in their own words. Our information about Etruscan history and society still consists largely of scattered and one-sided accounts in Greek and Roman literature. The value of these secondary reports varies considerably. Much of what we are told is prejudiced, inaccurate, and sometimes entirely fanciful. Any modern account must be founded on a full appreciation of these limitations^s in the surviving evidence.

Harris gives us an extensive survey of the sources (pp.4-40). He asks himself not only whether the information we are given is correct, but also whether it is 'esoteric' - i.e. how much of it can be said to derive from the Etruscans themselves. He begins by treating the historical implications of the Etruscan sacred books which contained the body of doctrine known as the Etrusca disciplina. Here it seems to me that he is not fully aware of the possible extent of the historical content of these libri. He confines himself to the observation that they must inevitably have revealed something about the society in which they were written and for which they were designed. He suggests, for example, that the terms exta regalia and fulmina regalia "probably attest to ~~the~~ the period of kingship in the Etruscan cities" (p.5). Such modest inferences

seem perfectly justified, and Harris applies the method with some success to the so-called 'Prophecy of Vegoia', a Latin version of which has chanced to survive (pp.31-40). But what he does not seem to have grasped is the possibility that the Etrusca disciplina may also have been more directly historiographical - i.e. that the books may have contained a systematic record of events of religious significance.

In a reference to the Tuscae historiae which were known to Varro, Harris says that they "may have contained only portents and other matter related to the disciplina". He then goes on to dismiss Heurgon's view that the Tuscae historiae may have been influenced by the redaction of the Annales maximi at Rome (p.12). But Heurgon's comparison with the Annales seems to me to be illuminating, and suggests a possible connection between the Etrusca disciplina and the Tuscae historiae which has escaped the notice of Harris.

Annales maximi was the name given to the published version of the original annual tabulae on which the Pontifex Maximus had been accustomed to record certain events; whatever the true purpose of these tabulae, it is clear that they were not originally intended as a historical record, but were designed to suit the practical requirements of the pontifical college (cf. C. Cichorius, RE s.v. "Annales", 2249-50). That the priests in the Etruscan

cities, and in particular the haruspices, kept some sort of record of their activities (i.e. of the portents which they were required to interpret, and possibly also of the consequences of their interpretations) is a reasonable a priori assumption; and it is strengthened by the fact that the complex pseudo-science of divining the future on the basis of observed signs (ostenta) must necessarily be founded on a learned tradition made up of recorded precedent. A close parallel is provided by the Babylonian omen texts. These records of omens were used as "Lehrtexte" - i.e. they were intended to show what kind of events could be expected if a given type of omen or combination of omens were to repeat themselves.⁽⁹⁵⁾ One point which surely increases the probability of the conjectures offered here is that signs and portents can be shown to have been recorded in the books of the Etruscan discipline; at any rate the libri must be the ultimate source of the references in Pliny (Nat. Hist. II.140) to incidents involving lightning-strokes at Volsinii; and in II.199 Pliny mentions an earthquake at Mutina which he had read about "in Etruscae disciplinae voluminibus".

It is therefore possible, even if it cannot be positively proved, that the Tuscae historiae mentioned by Varro were not literary histories in the Graeco-Roman sense, but rather a documentary collection of priestly

material closely related to the disciplina, if it was not actually a part of the disciplina. Varro's evidence that the Etruscan "histories" were written in the eighth saeculum (roughly coterminous with the second century B.C.) could be taken to mean that the primary material - a record of portents etc. - was first edited and published in book form in the eighth saeculum. It is surely in this sense that the Tuscae historiae are to be compared with the Annales maximi. We may note finally that the events which were recorded in the Etrusca disciplina seem to be very similar in character to what we know of the content of the Annales maximi.

Harris has little more to say on the subject of Etruscan historiography. He quite rightly rejects the notion that the surviving reports of the war between Rome and Veii are based ultimately on an Etruscan account; and he is generally sceptical about the extent to which authentic Etruscan literature has influenced our surviving sources. But he goes much too far in this direction.⁽⁹⁶⁾ We know that in the early years of the Principate some families tenaciously and proudly held on to their Etruscan identity by ties of marriage with other families of Etruscan origin. The evidence for this phenomenon consists in the first place of what we know about the family of the first wife of the emperor Claudius (See J. Heurgon, CRAI, 1953, 92-7).

It can hardly be a coincidence that Claudius was one of the few Romans known to us who studied the Etruscans, and the only one who has preserved for us an Etruscan story with an explicit reference to Etruscan sources. The supposition that Etruscan noble families constituted a vehicle for the preservation and transmission of their national historical tradition even in the time of the Empire is surely confirmed by the evidence of the elogia Tarquiniensia; these inscriptions, as we now know, were set up on behalf of two of the most important surviving Etruscan families, the Caesennii and the Spurinnae (cf. M. Torelli, "St. Etr." 36, 1968, 467 ff.). This seems to be the most likely interpretation of the evidence, and I must admit that Harris's criticisms of it have not made any impression on me. "The suspicion must be strong", he tells us, "that the authors of the elogia elaborated these traditions out of their own imaginations" (p.30)... The Caesennii and Spurinnae were "thoroughly Roman families" (ibid)...the use of Etruscan nomenclature by the wife of Claudius's brother-in-law was "mere affectation" (p.28)... Etruscan ancestry had a "well attested snob-value" (p.30)... interest in the past of Etruria was determined by "snobbery rather than scholarship" (p.28)...etc. etc. No argument is offered for this point of view; Harris employs a method of narrating in which what needs to be proved is again

and again simply stated as a fact. And if one is to dismiss the possibility that the family of Urgulanilla might have helped Claudius in his Etruscan researches, it is surely perverse to argue, as Harris does (p.28), that Claudius nonetheless had access to Etruscan sources through some other, presumably independent, means.

In his discussion of the value of the various Greek and Roman reports about the Etruscans, Harris deals with most of the relevant evidence, but his interpretation of many individual points is unacceptable. We may refer briefly to some of them.

(a) The view that Cato the Elder used local traditions of Italy as a major source for his Origines is well founded and does not depend solely on the citation of Sabine local histories in Dion. Hal. II.49, as Harris appears to think (p.18). The statement "it is in fact unlikely that (Cato) had the advantage of much Etruscan information about Etruria" (p.19) is arbitrary. Harris does not believe that the story of Arruns and Lucumo of Clusium (Cato, Origines frg.36 P.) derives from a local Clusine source - but his scepticism is unjustified. The notion that it was an anti-Etruscan fabrication by a Roman writer was refuted some time ago (H. Homeyer, "Historia" 9, 1960, 346), and the similarity of the story to that of the sons of Demaratus (Liv. I.34) surely strengthens rather than weakens the view that it is of Etruscan origin (cf. J. Gagé,

"Rev. Hist. Rel." 143, 1953, 170-208).

(b) According to Harris (p.22), much of what Posidonius says (ap. Diod. V.40) about the Etruscans is based directly on information from Etruria; but the hypothesis has no solid foundation in the internal evidence of the passage itself. Harris thinks (p.25 f.) that the excursus on Caere in Strabo (V.2.3, p.220 C) is based on Posidonius, who in turn was dependent on a local Caeritan source. This seems unlikely as well as superfluous. M. Sordi (I rapporti romano-ceriti...p.32 f. esp. p.42 ff.), who was the first to recognise the true significance of this excursus (which deals with a Caeritan victory over the Gauls after the sack of Rome), argued that Strabo's source was a writer of the third century B.C. At any rate her view that the formation of the tradition must antedate the diffusion of the Camillus legend seems reasonable, and would clearly exclude Posidonius. Moreover, Harris based his identification on the fact that Diodorus (XIV.117.7) also mentions a Caeritan victory over the Gauls, and Diodorus "made much use of Posidonius" (p.25). But Sordi showed convincingly that although Diodorus is referring to the same event as Strabo, he sets it in a different context, and seems to be looking at it from a completely different point of view; consequently he cannot be using the same source. (97)

(c) The most startling - and in my view the most erroneous - of Harris's conclusions concerns the Etruscan studies of Dionysius of Halicarnassus. He maintains that Dionysius had read a considerable amount about the Etruscans, and that much of his information came (by whatever means) from native sources. But the fact that Dionysius knew (I.30.3) what the Etruscans called themselves ("Rasenna") is hardly convincing evidence that he had access to 'esoteric' information; and the argument that he was unusually well informed about the Etruscan social system (p.26) turns out to be circular; we read in the chapter on Etruscan society that the use of the term "penestai" (D.H. IX.5.4) to describe the Etruscan serfs may be of special significance because "Dionysius' interest in writing a work about the Etruscans is of course attested, and he may well have acquired some detailed knowledge by the time that he wrote this passage" (p.121). In any event the use of the word "penestai" shows only that Dionysius was aware of a class of persons in the Etruscan cities whose status was somewhere between that of free men and slaves. This knowledge need not be based directly on 'inside' information.

Harris believes that it was Dionysius, not Claudius, who first discovered that the Etruscan Mastarna had been a king of Rome. In Ant. Rom. IV.1 ff. Dionysius rehearses

the various Roman accounts of the origins of Servius Tullius, but in III.65, according to Harris (here adapting a suggestion of A. Alföldi), he "alludes to a different tradition", when he describes Servius Tullius as a foreign exile - "ξένος καὶ ἄπολις".

Two points can be made here. First, why did Dionysius not mention the Etruscan tradition in the place where we should expect to find it - in the discussion of the origins of Servius Tullius in IV.1 ?. It seems hardly likely that he should have deliberately suppressed his alleged discovery.⁽⁹⁸⁾ Secondly, the phrase "ξένος καὶ ἄπολις" does not diverge from the Roman account of Servius Tullius, in spite of Harris's assertion that it does (p.26 n.4); it is in fact perfectly compatible with the old Latin story in which Servius was presented as the son of Tullius and Ocrisia of Corniculum.⁽⁹⁹⁾ Taken together, these objections are surely decisive.

I have emphasised these difficulties because so little is known about the domestic history of the Etruscan states, and everything in the end depends on the value one attributes to scraps of literary evidence such as we find in Dionysius. In general Harris seems to treat Posidonius, Dionysius and Ps. Aristotle (author of the De Mir. Ausc.) as more reliable authorities than Cato, Varro, Livy, Verrius Flaccus and Etruscan antiquarians such as Nigidius Figulus (NB especially pp.123-4). In my opinion

the exact opposite of this proposition would come nearer the truth.

It is a pity that such shortcomings should have obscured the many positive merits of a book which presents a useful and well written account of an important subject.

(viii) Notes to Chapter VII

- (1) J. Heurgon, La vie quotidienne chez les Etrusques, Paris 1961, (hereafter cited as: Heurgon, VQE), p.305.
- (2) I wrote this passage before the appearance of W. V. Harris's book Rome in Etruria and Umbria (Oxford 1971), the first chapter of which deals extensively with "The Historiography of Etruria" (pp.4-31). In general I found Harris's account disappointing, and it has not caused me to change any of the opinions I have offered in the text of this chapter. I have therefore confined my discussion of Harris to an appendix at the end of the chapter (see pp.215-229).
- (3) "Huic (sc. Tarquinio Prisco) quoque et filio nepotive eius (nam et hoc inter auctores discrepat) insertus Servius Tullius, si nostros sequimur, captiva natus Ocrexis, si Tuscos, Caeli quondam Vivennae sodalis fidelissimus omnisque eius casus comes, postquam varia fortuna exactus cum omnibus reliquis Caeliani exercitus Etruria excessit, montem Caelium occupavit et a duce suo Caelio ita appellavit, mutatoque nomine (nam Tusce Mastarna ei nomen erat) ita appellatus est, ut dixi, et regnum summa cum rei publicae utilitate optinuit". The text is taken from Dessau, ILS no.212. For a full commentary on the Table of Claudius see P. Fabia, La table claudienne de Lyon, 1929.
- (4) The best photographs of the painting are still those of F. Messerschmidt, Nekropolen von Vulci, "Jahrb. d. Deutschen Arch. Inst.", Ergänzungsheft 12, 1930, plates 14-26; cf. G. Q. Giglioli, L'Arte etrusca, plates cclxvi-cclxx. For a masterly interpretation of the incident shown in the picture see A. Alföldi, Early Rome and the Latins, pp.221-228, and plates viii-xii. For the date, see now M. Cristofani, "Dialoghi di Archeologia" I, 1967, 186-219.
- (5) The bibliography on this question is very extensive. My own study is based on a reading of the following: K. O. Müller - W. Deecke, Die Etrusker I, 1877, p.110 ff; A. Schwegler, Römische

Geschichte I, 1853, p.717 ff; V. Gardthausen, Mastarna oder Servius Tullius, 1882; G. Korte, "Jahrb. d. Deutschen Arch. Inst." 13, 1897, p.57 ff; F. Munzer, "Rh. Mus." 53, 1898, p.596 ff; E. Petersen, "Jahrb. d. Deutschen Arch. Inst." 13, 1899, p.128 ff; G. De Sanctis, "Klio" 2, 1902, p.96 ff; E. Cocchia, "Atti Accad. arch., Napoli" n.s. 8, 1924, p.209 ff; P. Fabia, La table claudienne de Lyon, p.73 ff; I. G. Scott, "Mem. Am. Acad. Rome" 7, 1929, p.75 ff; L. Pareti, "Rend. Pont. Accad. Arch." 7, 1931, p.89 ff. (= Studi minori...I, pp.283 ff.); L. Pareti, "Studi Etruschi" 5, 1931, p.147 ff. (= Studi minori ...I, p.305 ff.); A. Momigliano, Claudius, 2nd ed.1960, p.11 ff; Heurgon, VQE p.63 ff; A. Alföldi, Early Rome and the Latins, p.212 ff; H. H. Scullard, The Etruscan Cities and Rome, 1967, p.122 ff; and most recently A. Hus, Vulci étrusque et étrusco-romaine, Paris 1971, p.102 ff.

- (6) That this was the hard core of authentic tradition around which everything else was constructed is shown by the fact that Mastarna, and not his leader, the more important Caeles Vibenna, became king. This can only be understood in the context of the actual historical situation in the sixth century B.C., when Rome was not yet of overriding importance as far as the Etruscans were concerned, and could safely be left in the hands of a subordinate. To later Romans and Etruscans, and even to some modern scholars, this was not only embarrassing but incomprehensible. It must surely explain why Claudius (or, more likely, his source) was moved to imagine the defeat and premature death of Caeles Vibenna; there was certainly no evidence to suggest that Caeles had disappeared from the scene when Mastarna came to Rome; on the contrary, there was an old native tradition that Caeles Vibenna had in fact come to Rome in person (see pp.182 ff.).
- (7) Cf. A. Momigliano, Claudius, p.14.
- (8) Early Rome and the Latins, p.213. For A. Alföldi arbitrary contamination automatically suggested the hand of his historical forger Fabius Pictor (pp.133-4). But whatever view is taken of Alföldi's general interpretation of Fabius Pictor, it is clear that the identi-

fication of Mastarna with Servius Tullius cannot go back to him, since the main line of the Roman tradition (Claudius' "nostri") knew nothing of Mastarna or of his alleged identity with Servius Tullius. Cf. A. Momigliano, "Journ. Rom. Stud." 57, 1967, p.211. On Alföldi's interpretation of Dion. Hal. III.65.6 and IV.1.1 see p.228 and n.98-9.

- (9) It would have been uncharacteristic of the honest and pedantic emperor who in the same sentence had observed the discrepancy between authorities on the relationship between the two Tarquins (cf. Momigliano, Claudius, p.15). The introduction of Mastarna was not necessary for the validity of his argument that Roman history could show numerous examples of foreigners being admitted to high office; the Latin tradition of Servius Tullius as "Ξένος καὶ ἄπολις" (Dion. Hal. III.65, cf. below pp.227-8), was adequate on its own for this purpose. Cf. the speech of Canuleius in Livy IV.3-4, on which Claudius' speech is modelled.
- (10) Varro, de L.L. 5.46; Serv. ad Verg. Aen. 5.560; Dion. Hal. II.36.2, 37.2.
- (11) I have given the text as restored by Garrucci. But compare Müller's text:- "...regem) Tarquinius Romam secum max(ime adduxisse colue)rint". (Festus p.356 M., 486 L.).
- (12) Cf. F. Münzer, "Rh. Mus." 53, 1898, pp.605-6.
- (13) See H. Peter, HRR II, 1906, pp.78-9; cf. Schanz-Hosius, Gesch. d. röm. Lit. II, pp.366-367 for discussion of Verrius Flaccus's Res Etruscae.
- (14) I.e. Gnaeus Tarquinius of Rome. The praenomen Gnaeus means that he is not to be identified with either of the Tarquins who were kings of Rome, and who were both called Lucius. But he must have been a member of the same family. Cf. A. Hus, Vulci étrusque et étrusco-romaine, p.104.
- (15) Cf. A. Momigliano, Claudius, p.14.
- (16) Thus F. Münzer, "Rh. Mus." 53, 1898, p.610 ff; P. Fabia, La Table claudienne de Lyon, 1929, p.76. Of course, this interpretation cannot be considered

anything more than a hypothesis; we do not have any certain knowledge of the motives which led to this manipulation. Perhaps the original Etruscan tradition recorded that Mastarna had replaced a king Tarquinius (the François tomb-painting shows the death of Tarchunies Rumach), in which case he will have been identified with Servius Tullius simply because he could not have been identified with anyone else. (cf. Momigliano, Claudius, p.14). But this need not affect the basic argument unduly. H. H. Scullard remarks: "It is sufficient to note the proud claim of Vulci to have imposed a king on Rome". (The Etruscan Cities and Rome, p.123).

- (17) La vocation étruscologique de l'empereur Claude, "C.R. Acad. Inscr.", 1953, p.92 ff.
- (18) Heurgon, art.cit. (n.17), p.97.
- (19) The Roman legend emerges not only from the derivation of the name Caelius Mons from Caeles Vibenna, but also from the popular etymology of the word "Capitolium", interpreted by the Romans as "caput Oli"; this derivation, which appeared already in Fabius Pictor (F.12 Peter), refers to an indigenous Roman legend of a Vulcentane person named Olus, who is almost certainly to be identified with Aulus Vibenna. Cf. A. Alföldi, Early Rome and the Latins, p.216 ff.
- (20) G. Q. Giglioli, L'arte etrusca, plates ccxcviii,1; ccciv,3; Brunn-Körte, Urne etrusche II p.254 ff; F. Messerschmidt, "Jahrb. d. Deutschen Arch. Inst." 45, 1930, p.79.
- (21) British Museum Catalogue; Bronzes no.633; E. Gerhard - G. Körte, Etruskische Spiegel V, plate 127 and p.166 ff; F. Messerschmidt, art.cit. (n.20), p.77.
- (22) Corpus Vasorum Antiquorum (France) n.16 (Musée Rodin pl:28-30); J. D. Beazley, Etruscan Vase Painting, p.25 ff.
- (23) M. Pallottino, Testimonia Linguae Etruscae (TLE), 1954, no.35. See Pallottino, "Studi Etruschi" 13, 1939, p.455 ff; J. Heurgon, La coupe d'Aulus Vibenna, "Mélanges J. Carcopino",

Paris 1966, 515-528. But notice the judicious remarks of E. J. Bickermann, "Riv.Fil.Class." 97, 1969, p.397.

- (24) Heurgon, VQE, p.64.
- (25) The fact that Claudius's story about Mastarna and Servius Tullius was independent of Varro might mean also that it was not recorded in the Tuscae historiae which Varro had consulted. This cannot be certain, however, because we do not know whether Varro read the Tuscae historiae right through for himself, or simply acquainted himself with that part of it which interested him. Cf. Heurgon, VQE, p.306.
- (26) Heurgon, loc.cit. The use of the plural "historiae" as the title of a single historical work is common in ancient literature from Herodotus onwards. But Heurgon's point is stronger when taken in combination with the fact that the Tuscae historiae are anonymous.
- (27) B. G. Niebuhr, History of Rome (trans. C. Thirlwall, London, 1853) I. p.384.
- (28) Cf. B. Nogara, Gli Etruschi e la loro civiltà, 1933, p.423.
- (29) The eighth Etruscan saeculum ended in 88 B.C. (Plutarch, Sulla 7), but we do not know when it began. But if we take the average of the previous three saecula (the first four, one hundred years each, are clearly suspect), we arrive at a figure of c.120 years, which cannot be far wrong. The possibility of a connection between the beginning of the eighth saeculum and the disturbances in Etruria in 208-7 B.C. has been examined by D. W. L. Van Son, "Mnemosyne" 16, 1963, p.267 ff. For practical purposes it will be sufficient to regard the eighth saeculum as roughly congruent with our second century B.C. On the saecula in general see C. O. Thulin, Die etruskische Disciplin III, 1909, p.63 ff.
- (30) Heurgon, VQE, p.307; K. O. Müller - W. Deecke, Die Etrusker, II, p.298.
- (31) First published by P. Romanelli, "Not. Scav." 1948, p.260 ff; discussion in J. Heurgon, "Mél. Arch. et Hist." 63, 1951, p.119 ff; id., "C.R.

Acad. Inscr." 1951, p.216 ff; M. Pallottino, "Studi Etruschi" 21, 1950-51, p.147 ff; U. Kahrstedt, "Symb. Osl." 30, 1953, p.68 ff; E. Vetter, "Glotta" 24, 1955, p.59 ff; F. Della Corte, "Studi Etruschi" 24, 1955-56, p.73; R. Lambrechts, Essai sur les magistratures des républiques étrusques, 1959, p.102 ff; Heurgon, VQE, p.314 ff; A. Alföldi, Early Rome and the Latins, p.212 ff; H. H. Scullard, The Etruscan Cities and Rome, p.91 ff; M. Torelli, "Studi Etruschi" 36, 1968, p.467 ff; J. Heurgon, "Arch. Class." 21, 1969, p.88 ff; W. V. Harris, Rome in Etruria and Umbria, 1971, p.28 ff.

- (32) A. Degrassi, Inscriptiones Italiae, xiii.3 ("elogia") p.1 ff.
- (33) This is not the place for a full discussion of these texts. For epigraphic details and textual restorations see especially Pallottino, "Studi Etruschi" 21, p.147 ff; E. Vetter, "Glotta" 24, p.59 ff; Heurgon, "Arch. Class." 21, p.88 ff. Dr. M. Torelli has apparently discovered some more material (apart from that published in "Studi Etruschi" 1968), which is likely to increase our knowledge quite considerably. A full treatment of the elogia must be postponed until this new evidence is published.
- (34) The idea that one of the elogia (Romanelli no.44) concerned Tarchon, the founder of the city, cannot be maintained. As Heurgon has shown ("Arch. Class." 21, 1969, p.88 ff.), the letters "...CHO...", on which this interpretation is based, could form part of one of several known Etruscan names, for example "Holchonius". The 'Tarchon theory' is finally condemned by the presence in the inscription of a word beginning with the letters "HAM...". This can hardly be anything other than the beginning of the name Hamilcar.
- (35) Romanelli, art.cit., p.266, no.77.
- (36) S. Mazzarino, Dalla monarchia allo stato repubblicano, 1945, p.95 ff.
- (37) Livy, V.1.3.
- (38) Romanelli, no.48.

- (39) Pallottino's version retains the simple sense of what is already preserved; taking "traiecit" in its transitive sense, he restores:
 "primus (trans) Etruscorum mare c(lassem) traiecit" - ("Studi Etruschi" 21, 1950-51, pp.160-3).
 The difficulty is that "Etruscorum mare" is an odd way of describing the Tyrrhenian Sea, which is normally written "Mare Tuscum". Heurgon ("Mél. Arch. et Hist." 63, 1951, p.130) proposes:
 "primus (ducum) Etruscorum mare c(um milite) traiecit", so that the originality of the enterprise is expressed in the words "primus cum milite", rather than by "primus mare traiecit". He thus avoids the chronological difficulty of "primus", which, if taken literally, would date the expedition to prehistoric times (see below, n.40).
- (40) The Etruscans are recorded in Greek sources as being active as 'pirates' around Sicily as early as the eighth century B.C. (Strabo VI.2.2. = Ephorus F.137 Jac.). Thus "primus...Etruscorum mare...traiecit" can scarcely be taken at its face value, especially in view of the fact that the subject of the elogium was 'praetor' - i.e. a republican magistrate. Hence Heurgon's suggested reading "primus c(um milite)". More satisfactory, it seems to me, is the view of Pallottino (art.cit.), followed by Della Corte ("Studi Etruschi" 24, 1955-56, 75), that "primus" is little more than a commonplace, often found in elogia, and is not to be taken very seriously.
- (41) Heurgon ("Mél. Arch. et Hist." 63, p.132 ff.), following a suggestion of J. Bayet, proposed that the praetor was leader of a band of mercenaries; we find Etruscan mercenaries fighting in Sicily for Hamilcar against the forces of Agathocles in 311. B.C. (Diod. xix.106.2); and Agathocles himself employed Etruscan mercenaries in 310 (id. xx.64.1), and again in 296 (xxi.3). F. Miltner (cited by Vetter, "Glotta" 34, p.62) refers the expedition to the year 307, when eighteen Etruscan warships went to the aid of Agathocles (Diod. xx.61.6-8). Della Corte (art.cit. p.76 ff.) favours the events of 414-3 B.C., when an Etruscan contingent was present at Syracuse on the Athenian side (Thuc. VI.88.6; VII.53.2, 57.11). A full list of occasions on which the Etruscans intervened in Sicily is given by Heurgon, art.cit., p.130 ff.

- (42) The restoration "de La)tinis" is more probable than "de Arre)tinis" because the city of Arretium is referred to in the previous line of the text, and would be unlikely to be repeated. See Heurgon, VQE, p.316; A. Alföldi, Early Rome and the Latins, p.208.
- (43) The text reads:- V.....R SPUR....
 (L)ARTIS F.
 PR. II .. MAGISTRATU AL.....
 EXERCITUM HABUIT ALTE.....
 5 SICILIAM DUKIT PRIMUS.....
 ETRUSCORUM MARE C.....
 TRAIECIT A QU.....
 AUREA OB VI.....
 (Romanelli, no.48).)
- (44) The 'interpretatio Latina' of the Etruscan title "zilath". Kahrstedt's arguments against this are not convincing. ("Symb. Osl." 30, 1953, p.68 ff.).
- (45) "Studi Etruschi" 21 (cit. n.31), pp.161 and 164.
- (46) Heurgon's restoration "...a(d Caere ?) exercitum habuit"... in lines 3-4 is now refuted by the fragment identified by Torelli (see below n.50) which shows traces of the top of a vertical stroke after the A at the end of line 3. Whatever this letter was (L?), it cannot have been a D. 8)
- (47) "ob vi(rtutem..." is better than "ob vi(ctoriam..."
- (48) The date of the events must at any rate be earlier than the incorporation of the Etruscan cities in the Roman confederacy - cf. note 41, above.
- (49) Of course it is perfectly possible in theory that the elogia Tarquiniensia were based on literary historical sources. It is worth noting in this connection that the information in the Roman elogia was taken from the literary annales (O. Hirschfeld, "Kl. Schr.", p.814 ff; A.von Premerstein, RE,V.s.v. "elogium", 2247; Mommsen, CIL I (2 ed.) p.190 f; A. Degrassi, Inscr. Ital. XIII 3, p.6 ff.).
- (50) "Studi Etruschi" 36, 1968, p.467 ff.

- (51) art.cit. (n.31, 39, etc.) p.149.
- (52) Romanelli, nos.45, 46, 47.
- (53) Torelli, art.cit. (n.31, 50, etc.) p.467.
- (54) M. Schuster, RE VIII A, s.v. "Vestricius", 1791 ff; cf. Torelli, art.cit. p.468 f.
- (55) Romanelli, no.43.
- (56) Cf. PIR² II, p.32 ff., s.v. "Caesennius".
- (57) Cf. Kahrstedt, "Symb. Osl." 30, 1953, p.69; Pallottino, art.cit. p.164.
- (58) Cf. Heurgon, "Mél. Arch. et Hist." 63, 1951, p.126; id., "C.R. Acad. Inscr.", 1953, p.92; Pallottino, art.cit. p.151.
- (59) It is surely relevant to observe that the same sort of efforts were made by Maecenas, a new name in the highest circles at Rome, but a proud descendant of the most important family in his native Etruscan city of Arretium. Horace (Sat. I.6.1.) writes:
 "Non quia, Maecenas, Lydorum quidquid Etruscos
 incoluit finis, nemo generosior est te
 nec quod avus tibi maternus fuit atque paternus
 olim qui magnis legionibus imperitarent..."
 (cf. Horace, Odes III.29.1; Propertius, El. III.9.1.)
- (60) It is interesting to note that the practice of setting up elogia is normally thought to have originated with the "imagines", and the "tituli" that went with them, which were placed in the atria of noble houses. See Mommsen, CIL I (1st. ed.) p.282; C. Hülsen, CIL I (2nd. ed.) p.189; A.von Premerstein, RE s.v. "elogium" 2443; A. Degrassi, Inscr. Ital., xiii 3, p.ix; K. J. Beloch, Römische Geschichte, p.97.
- (61) Gell., Noct. Att. XIII 20.17.
- (62) Polyb. VI. 53-4; see J. Marquardt, Das Privatleben der Römer, 1886, I. p.357-60 (= La vie privée I. p.417-21); M. Schanz - C. Hosius, Gesch. d. röm. Lit. I. p.38-40.

- (63) "Ztschr. f. Papyrologie u. Epigraphik" 5, 1970, p.217 ff.
- (64) Cf. R. Laqueur, RE XIII s.v. "Lokalchronik", 1083-1110.
- (65) "Glotta" 28, 1940, p.168.
- (66) M. Pallottino, "Studi Etruschi" 14, 1940, p.465 ff; id., "Studi Etruschi" 24, 1955-6, p.50 ff; J. Heurgon, "Historia" 6, 1957, p.85; id., VQE, p.273 f; R. Lambrechts, Essai sur les magistratures des républiques étrusques, p.97 ff.
- (67) M. Pallottino, TLE no.98.
- (68) Heurgon, VQE, p.273.
- (69) It is uncertain whether he was king or magistrate. The Phoenician text says that he was king ("melek"), but the original Etruscan has "zilath". He had been in power for three years, which means either that he had been elected for three years in succession, or that he was a long-term magistrate, analogous to the ten-year archons at Athens.
- (70) Heurgon, VQE, pp.306, 309.
- (71) See above, p.189 and note 30.
- (72) Cato, Origines F.77.
- (73) Cf. C. Cichorius, RE I s.v. "Annales", 2249-2250.
- (74) Cicero, de orat. II.52. NB the phrase "per singulos dies".
- (75) Cicero, Ep. ad fam. VI 6.3; cf. C. O. Thulin, RE VII s.v. "haruspices", col.2437.
- (76) Cicero, de div. I 33.72.
- (77) Historical allusions in Old Babylonian omen texts, "Journ. Cuneiform Studies" I, 1947, p.253 ff.
- (78) Mesopotamian Historiography, "Proc. Am. Philos. Soc." 107, 1963, p.461 ff.

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- (79) A. Goetze, art.cit. (n.77).
- (80) J. Nougayrol, Haruspicine étrusque et assyro-babylonienne, "C.R. Acad. Inscr." 1955, p.509 ff.
- (81) On this question in general see C. O. Thulin, Die etruskische Disciplin, (1906-9), Introduction pp.x-xv; P. Ducati, Le problème étrusque, 1938, esp. p.154 ff; L. Pareti, Le origine etrusche, p.182 ff; M. Pallottino, L'origine degli Etruschi, p.135 ff; F. Altheim, Der Ursprung der Etrusker, passim; id., History of Roman Religion, p.46 ff; etc.
- (82) Plin., n.h. II 53. 139.
- (83) id., n.h. II 54. 140. 5)
- (84) On Tarquitiu Priscus see J. Heurgon, "Latomus" 12, 1953, p.402 ff.
- (85) Plin., n.h. II 85, 199.
- (86) Cic., de Div. II 23.50.
- (87) e.g. Seneca, Quaest. nat. II.32 ff. 8)
- (88) Cic., de Div. II 23.50; cf. I 25, and Thulin, Die etruskische Disciplin, III, p.76 and n.1.
- (89) Cicero corresponded with Caecina (ad Fam., VI 5 ff.), and in 69 B.C. defended his father in the speech pro Caecina.
- (90) Schol. Veron. ad Verg. Aen. X. 200. The end of the sentence is obscure.
- (91) Heurgon, VQE, p.306; cf. F. Münzer, "Rh. Mus." 53, 1898, p.606.
- (92) F. Münzer, ibid.
- (93) Festus, s.v. "rituales libri", p.358 L.
- (94) It is worth pointing out that the inactivity of the Etruscans and Umbrians in the early part of the Social War may be due partly to the fact that urbanised areas of Italy (including Campania and Magna Graecia) were generally reluctant to

take up arms against Rome; the majority of the insurgents were tribal states in the poorest parts of the peninsula (cf. E. T. Salmon, Samnum and the Samnites, p.340 f.). On this contrast, and its implications concerning romanization, see A. J. Toynbee, Hannibal's Legacy, 1966, I p.269 ff.

- (95) The interesting thing about these omen texts is their close and obviously direct relation to the so-called "Chronicle of the Early Kings". Whereas it used to be thought that the omens were late fabrications based on information extracted from this chronicle (King, Chron. I p.28), scholars are now almost certain that the true relationship is the other way round. See e.g. Goetze, art.cit. (n.77). If the parallel is in any sense a true one, this would seem to have important implications for the Tuscae historiae. 6)
- (96) For instance, he argues (or implies) that haruspices such as Caecina no longer took their business seriously, but were merely concerned to maintain the "appearance of authenticity" (p.6). But the very fact that the haruspices adapted the ancient disciplina to new conditions, and constantly kept it up to date, suggests on the contrary that the haruspical tradition was still alive (cf. Cic. ad Fam. VI 6.3, and NB the remarks of S. Weinstock, "Papers Brit. Sch. Rome" n.s. 6, 1951, 122-53). Again, in saying that "authentically Etruscan books of this nature were of almost exclusively antiquarian interest by the end of the Republic" (p.8), Harris fails to take account of the fact that men like Caecina were themselves antiquarians - "men of considerable learning", as Weinstock observes, "equals in their subject of Cicero and Varro" (art.cit. p.123). 8)
- (97) Sordi also believes (op.cit. p.47 n.3) that Strabo cannot have been using a Greek source, and postulates a Roman or Etruscan historian; but her arguments are not compelling. See below, pp.274 ff.
- (98) Alföldi maintained that the tradition had been discovered and manipulated by Fabius Pictor (cf. above, pp.231-28). He may have thought (although

he does not actually say so) that Dionysius's allusion was somehow unconscious, and that he had unwittingly been influenced by Pictor's view without realising the full implications of it; but the logical result of the theory that Dionysius himself unearthed the Mastarna tradition can only be that he was fully aware of the implications of the alleged allusion in III.65, but that he chose to conceal his knowledge when he came to deal with the origins of Servius Tullius in IV.1. The absurdity of Harris's view is indicated on p.27 (and n.4), where he has to draw a distinction between "the first writer to allude to Mastarna" (i.e. Dionysius) and "the first to mention him by name" (i.e. Verrius Flaccus).

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His father Tullius had been killed in a war against Rome in which Corniculum was finally defeated and sacked by Tarquinius Priscus. Ocrisia was taken back to Rome as a prisoner and made a slave of queen Tanaquil. Ocrisia's son (Servius Tullius) could therefore be accurately described as "ξένος καὶ ἄπολις". There can be no doubt about this interpretation, because in IV.1.1 Dionysius announces that he is going to outline the details of Servius Tullius's personal background "which we at first omitted" ("ὡ κατ' ἀρχὰς παρελίπομεν"). This reference can only be to the passage where he had described Servius Tullius as a foreign exile and where he had written:

" γένος δὲ τοῦ ἀνδρὸς τοῦδε καὶ τροφῆς καὶ τύχης καὶ τὴν ἐκ τοῦ Θεοῦ γεγονένην περὶ αὐτὸν ἐπιφάνειαν, ὅταν κατὰ τοῦτο γένωμαι τὸ μέρος τοῦ λόγου, διηγήσομαι."

Dion. Hal. III.65.6

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CHAPTER VIII*

In the last chapter we tackled the problem of Etruscan historical literature directly - first by examining the implications of the available Etruscan evidence (mostly pictorial monuments and inscriptions), and secondly by studying the external testimony of Roman writers whose statements seemed to prove the existence of Etruscan historiography. But this direct approach is not the only method open to us. The influence of Etruscan native tradition on Graeco-Roman writings about the Etruscans ought occasionally to be detectable, and scholars have frequently attempted to draw inferences about Etruscan historical sources from the internal evidence of surviving texts. It is my intention in this chapter to investigate some of the passages which have been thought to contain indigenous Etruscan material.

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(i) Rome and Veii

We may begin with a discussion of the startling contention that "the whole narrative of Rome's war with Veii was already consolidated in Etruscan historical sources in the fourth century".⁽¹⁾

Livy's account of the great war between Rome and

(* For Notes to Chapter VIII see below, p.299-317.

Veii is remarkable in two ways. First, it purports to be well informed about Etruscan affairs: several meetings of the Etruscan 'league' are recorded, at which the Veientes and their allies tried to obtain aid from the other Etruscan cities in their struggle against Rome.⁽²⁾ It is hard to see how these notices could have been preserved in the national tradition of the Romans, and if they are historical the only reasonable explanation is that they derive ultimately from a non-Roman, i.e. Etruscan, source.⁽³⁾ Secondly, the narrative as it is presented by Livy has a curious internal structure, and is pervaded by an archaic religious atmosphere. This makes itself felt not only in the numerous religious episodes in the story, such as the prodigy of the Alban Lake (Liv. V.15), the stealing of the 'exta' (V.21 8-9), and the 'evocatio' of Juno from the captured city (V.22. 4-7), but also in the language and general tone of the account, with its numerous references to fate and destiny. Camillus is presented as "fatalis dux ad excidium illius urbis servandae-que patriae" (V.19.2). The particular emphasis on the parallel destinies of Rome and Veii has been interpreted as a reflection of the fatalistic determinism characteristic of the Etruscan view of the past.⁽⁴⁾ The hypothesis that an Etruscan source is ultimately responsible for these religious elements is held to be confirmed by the reference

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to the Etrusca disciplina and the "libri fatales" invoked by the Veientine haruspex in his interpretation of the prodigy of the lacus Albanus.⁽⁵⁾

It must be admitted that the general impression conveyed by these features of Livy's account is one of unity. M. Sordi writes (op.cit. p.9):- "È difficile non sentire in questo racconto, ricomposto nelle sue varie parti, un impronta unitaria: i vari episodi di esso non sono, come sembra credere Livio (V.21.8), delle favolette giustapposte, ma sono sentiti come le manifestazioni progressive di un piano misterioso." It would seem to follow that the unusual characteristics of the tradition about the Veientine war go back to a source which gave a full and coherent account of it. The hypothesis of Bayet, Sordi and others⁽⁶⁾ that this source was Etruscan cannot be dismissed out of hand⁽⁷⁾ - but it seems to me that there are a number of serious objections to it.

First let us consider the reports of meetings of the Etruscan league at the fanum Voltumnae. Obviously, the view that these notices derive from an Etruscan source only becomes necessary if they can be shown to have some basis in historical fact. But there are good reasons for supposing that they are fictitious.

It seems to me that Livy's account of these meetings presents an unrealistic picture of the function and operation

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of the Etruscan 'league'.⁽⁸⁾ It is one thing to suggest that Veii asked for help from the other cities, but quite another to imagine that they were under some constitutional or traditional obligation to provide it, and that their refusal to do so must have been due to unusual circumstances requiring explanation. But this latter idea is the a priori assumption of our sources. The Roman annalists seem to have believed that the Etruscan cities usually co-operated in their military undertakings; wars between Rome and one or other of the Etruscan cities were treated as struggles against all the Etruscans. The alleged conquest of the entire Etruscan nation by successive Roman kings is a grotesque example of this tendency.⁽⁹⁾ But in reality it is difficult to point to any single piece of authentic evidence to show the Etruscan cities acting together as a national league; on the contrary, as Pareti demonstrated, all the most reliable evidence indicates that relations between the major Etruscan cities were characterised by instability and mutual hostility throughout the historical period.⁽¹⁰⁾

The juridical accusation against the Veientes, that they started the war without consulting the league (Liv. IV.24.2, V.17.7), is obviously inconsistent with the independent evidence for the political disunity of the cities; and the idea that the 'league' maintained a position

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of neutrality during the war between Rome and Veii is contradicted by the account of Livy himself. During the siege of Veii the men of Tarquinii were hostile to Rome, and actually invaded the ager Romanus in 397 B.C. (Liv. V.16.2,4 etc.); meanwhile Caere seems to have been friendly, and allowed the Roman army to pass unhindered through its territory (V.16.5). Other cities, such as Clusium, remained neutral (V.35.4), probably because the events at Veii were no concern of theirs. The allegiances of the Etruscan cities during the war were therefore not uniform, and there is no sign of any 'national' policy, whether of neutrality or of favour to one side or the other.

In any case Livy's statement that Veii was accused of taking action without authorisation from the league is very similar to a passage of Dionysius of Halicarnassus, in which, however, the meeting is dated to the first half of the fifth century (Dion. Hal. IX.18.2); even more striking is Dionysius' account of a meeting in 479 B.C., where a decree was passed which is exactly the same as that recorded by Livy under the year 397.⁽¹¹⁾ It follows that the record of meetings at the fanum Voltumnae in Livy need not derive from a self contained Etruscan account of the siege of Veii.

The Roman annalists knew that the Etruscan communities

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met once a year at the shrine of Voltumna, and on the mistaken assumption that these sacred gatherings had been meetings of a political federation in the days of Etruscan independence, they imagined that the Etruscan 'nation' had determined its policy on these occasions, and had expressed it in a series of 'decrees'. But notices of these resolutions in our sources are no more than an annalistic fiction designed to explain references in the Roman archives to wars undertaken against individual Etruscan cities.⁽¹²⁾ As Pareti points out: "Gli annalisti, con Livio, amarono invece di attribuire un' importanza alla lega dei XII popoli fin dall' età più antica; sia pure che poi per giustificare il loro stesso racconto, che parlava di singole città etrusche, in lotta con Roma, dovessero ricorrere al coonestamento, che, per un motivo o per un altro, i XII popoli avevano rifiutato di aiutare le città belligeranti".⁽¹³⁾

The only part of Livy's account of Etruscan events that has any semblance of authenticity is the story of the Veientine king's sacrilege at the federal games (V.1.3). The story contains circumstantial details which might be genuine. But the idea that the other cities refused to help Veii because of their hatred of monarchy, and of the king himself, seems unlikely. "The two reasons given for the neutrality of the rest of Etruria - monarchy and impiety -

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are too schematic and too Roman".⁽¹⁴⁾ The anecdote itself was probably a local oral tradition which originally had no connection with the siege of Veii. Alternatively, one might postulate the existence of some sort of chronicle at Volsinii, with a list of annual 'sacerdotes'; such a document might well have contained notices of this kind, because it refers specifically to the games at the fanum Voltumnae and to the office of Sacerdos.⁽¹⁵⁾

Certainly one is not justified in assuming that the anecdote verifies the other notices of meetings at the fanum Voltumnae, or anything else in the account. There is no reason to suppose that it formed part of a larger tradition about the Veientine War; it seems rather to be an isolated fragment of an independent tradition which has been incorporated, without its true context, into the story of Veii.

As for the notion that an Etruscan source lies behind the fatalistic and religious elements of the traditional account, one can only say that it is not based on any firm evidence. Even if some Etruscan religious conceptions have found their way into the narrative, there is no reason why these should not have been introduced by the Roman annalists in an attempt to lend authenticity to the story.

The interpretation of the prodigy of the Alban Lake by an old Veientine haruspex is said to have been based

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on information derived from the "Books of Fate" and the Etruscan discipline:

"sic igitur libris fatalibus, sic disciplina Etrusca traditum est, [ut] quando aqua Albana abundasset tum si eam Romanus vite emisisset victoriam de Veientibus dari; antequam id fiat deos moenia Veientium deserturos non esse" (Liv. V.15.11).

But the story of the old man of Veii and his prophecy is obviously a fiction - in spite of ingenious attempts (16) by J. Gagé to establish "l'historicité de fond de l'épisode". Indeed, it may be doubted whether the construction of an emissarium for the Alban Lake has any historical connection with the siege of Veii. (17) The matter is complicated by the story of the Roman embassy to Delphi, which returned with an answer confirming the prophecy of the old haruspex. (18) The tradition about the golden bowl which the Romans dedicated to the god of Delphi almost certainly rests on a foundation of authentic fact. (19) Appian records that the bowl was melted down by Onamarchus in the Sacred War, but that its bronze base remained in the sanctuary of the Massiliots for all to see. (20) F. Altheim has pointed out that the story of the Liparaean pirate Timasitheus, who escorted the Roman ships, was a local family tradition and therefore probably genuine. (21)

But none of this is sufficient to verify the prodigy

or the dating of the emissarium of the Alban Lake to the time of the siege of Veii. It is probable that the golden bowl was sent by the Romans simply as a thank-offering to Apollo after their victory,⁽²²⁾ and that the connection of the Delphic oracle with the Alban prodigy was a secondary reconstruction.⁽²³⁾ It is significant that the embassy was mentioned by Diodorus (whose source was probably an annalist of the second century B.C.),⁽²⁴⁾ but not in connection with the prodigy (Diod. xiv.93).

The association of the emissarium with the siege of Veii was almost certainly a later development arising from the tradition that the city had been captured by means of a tunnel.⁽²⁵⁾ But unlike the story of the Delphic embassy the account of the haruspex and his prophecy cannot be separated from the prodigy or the construction of the emissarium. It must therefore have arisen as part of the same process of elaboration. There is no justification for the view that these embellishments appeared for the first time in an Etruscan source, or that they were added at an early stage in the development of the tradition about Veii.

It is not enough to argue that the tone of the account conforms to what we know (which is little enough) about Etruscan religion; it would still only become necessary to postulate an Etruscan source if the supposedly Etruscan

elements could be shown to be part of the earliest level of the tradition - i.e. that they date back to the third century B.C. or earlier. But there are good grounds for thinking that we are dealing with late accretions.

The story of the capture of the old man of Veii, which is central to the whole problem, takes its starting point from the fact that the Romans were unable to consult the haruspices during a war against the Etruscans: "hostibus Etruscis, per quos ea (prodigia) procurarent haruspices non erant" (V.15.1). But this statement is clearly out of place in the context of the early fourth century B.C. The practice of summoning haruspices from Etruria to advise the senate on prodigies seems to have originated at the time of the Second Punic War, and probably only became regular in the course of the second century.⁽²⁶⁾ In any case the official consultation of haruspices by the Romans can hardly have taken place until after the conquest of Etruria - i.e. after 300 B.C. at the earliest.⁽²⁷⁾ In fact the whole story is an anachronism; the interpretation of the prodigy of the Alban Lake is typical of the "responsa" which the haruspices offered when summoned to Rome in the late Republic.

The capture of the haruspex from the walls of Veii is not the only known example of a story of this kind.

There are in fact numerous parallels for the theme of the seer who must be caught before he will give away his secrets. For example Silenus, in the sixth Eclogue, reluctantly agrees to sing about the creation of the world after being tied up by the youths. Another example is Proteus, who has the power to change his shape, but reveals his secrets if captured. We may add the curious story of Picus and Faunus, who offered instructions and prophecies to King Numa after he had succeeded in catching them. (28)

But the origin of the Veientine story is indicated by a much more striking parallel, which corresponds very closely to the tale of the haruspex both in its general context and in many of its specific details. This is the tradition of Helenus, the son of Priam, who "knew the oracles that protected Troy". (29) Helenus was captured by Odysseus and Diomedes, and compelled to reveal the secrets concerning the fall of Troy - the "fata Troiana", as Servius says (ad Aen. II.166). Helenus does not appear in Homer, but the story was well established in Greek tradition by the time of Sophocles, who refers to it in the *Philoctetes*. (30)

To me this undoubtedly shows that the entire narrative of the haruspex and his prophecy was a late construction of literary origin - because it is clearly a symptom of

an obvious and deliberate attempt by the Roman annalists to assimilate the siege of Veii to the Greek legend of the Trojan War. (31)

The siege and capture of Veii was an event of the first importance in the history of Italy, and undoubtedly made a tremendous impression on the popular memory of the Romans. But precisely because of its importance in the Roman historical tradition, the story of the siege naturally attracted to itself a lot of other traditions which originally had no connection with it (such as the building of an emissarium for the lacus Albanus), as well as a large number of fictitious embellishments. These latter include some at least of the religious episodes in the account, such as the prophecy of the old man of Veii. And we may place under the same head the absurd tale of the stealing of the 'exta': Livy tells us (V.21.8) that in the course of a sacrifice by the king of Veii an haruspex pronounced that whoever cut out the entrails would be victorious in the war. This was overheard by the Roman soldiers in the tunnel, who thereupon burst out, snatched the entrails, and carried them back to Camillus. The explanation of this story lies in the phrase "exta rapere", used to describe the cutting out of the entrails in an Etruscan ritual. The fact that it implies a misunderstanding of the phrase "exta rapere" proves that the story cannot

be of Etruscan origin.⁽³²⁾

As for the use of religious language in the narrative of the war with Veii there is no reason why this should not have been incorporated into the tradition by the annalists - perhaps even by Livy himself. Ogilvie points out (p.626) that Livy gave his fifth book a coherent structure by emphasising the importance of religion as a factor in Roman history: "Book V illustrates how the fortunes of the city veer as her rulers observe and neglect their religious duties". In fact the theme of attention to, and neglect of, religious obligations by the leaders of the two contending cities is fitted into a neatly symmetrical pattern. Victory goes to the Romans because they atone for their impiety; the Veientes fail to do so, and are defeated.

The religious offences of the two sides are exactly the same: "quia solemnia ludorum quos intermitti nefas est, violenter diremisset" writes Livy of the king of Veii (V.1.4); and of the Romans: "ubi neglectas caerimonias intermissumve solemne di arguerent" (V.17.2). Not only is the language parallel, but also the circumstances, if one cares to see significance in the fact that both passages refer to offences relating to the respective annual festivals of the Latin and Etruscan leagues.⁽³³⁾ The parallelism is deliberate⁽³⁴⁾ and is attributable either to Livy himself

or to his Roman predecessors. The underlying religious conceptions seem to be Roman, rather than Etruscan: the outcome of the war is determined not by the irremediable decrees of an all-powerful Fate, but by the human activities which affect the relationship between men and the gods.

There is no justification, therefore, for the theory that the traditional narrative of the war between Rome and Veii was based ultimately on an Etruscan account. In general it seems a rather hazardous proceeding to attribute this or that passage to an Etruscan source merely because it looks 'Etruscan'. (35)

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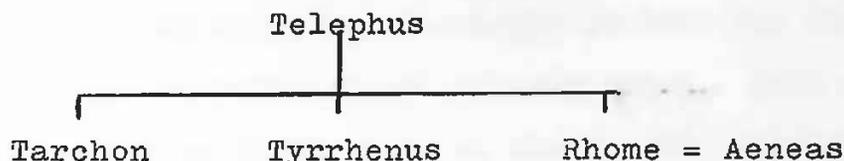
(ii) Etruscan accounts of the founding of Rome ?

It has occasionally been suggested that Greek and Roman writers may have found some information in the works of Etruscan historians about the beginnings of Rome. This view is based on the fact that some of the known versions of the foundation story contain 'etruscanised' elements.

Dionysius of Halicarnassus observed (I.29.2) that for many authorities Rome was a Tyrrhenian foundation. We know of at least one version in which the founding of Rome was connected directly with the tradition of Etruscan origins. This is reflected in the statement of Plutarch

(Romulus, 1,2) that Aeneas married Rhome - a daughter of the Mysian hero Telephus; Telephus is elsewhere recorded as the father of Tarchon and Tyrrhenus. (36)

The unusual presence of Telephus shows that we are dealing with variants of a common tradition. W. Schur traced all these variants back to a common source. (37) This common source, identified by Schur as Timaeus, had enumerated the descendants of Telephus as follows:



F. Schachermeyr believed that the ultimate source of this Telephus variant was a native Etruscan tradition, which had found its way into Greek literature by way of Timaeus. (38) This seems unlikely. Certainly Tarchon was an indigenous Etruscan hero, but there is no reason to suppose that the whole genealogy was first constructed by an Etruscan historian. We may note in passing that it cannot have appeared in Timaeus, because we know that he followed Herodotus in thinking that the Etruscans were descended from Tyrrhenus, the son of a king of Lydia. (39)

According to Schachermeyr the addition of Rhome to the family of Tarchon and Tyrrhenus had a political motive: "Da ist es wohl begreiflich, wenn (die Etrusker) in dieser Version ein Mittel finden wolten, um auf die

altangestamnten politischen Vorrechte Etruriens gegenüber Rom hinzuweisen".⁽⁴⁰⁾ But this is highly speculative; a far more satisfactory explanation of the mixture of the Roman and Etruscan legends of origin is that it was a learned Greek reconstruction; precisely comparable in this respect is the version of the fourth century Sicilian historian Alcimus, who stated that Romulus was the son of Aeneas and Tyrrhēnia (F. Gr. Hist. 560. F.4).

As we shall see, Schachermeyr is not the only scholar to argue that versions which connect Aeneas with the Etruscans grew up in Etruria in the fourth century as expressions of a political tendenz.⁽⁴¹⁾ But the contention rests on a very insecure foundation in view of what is now known about the popularity of the Aeneas legend in archaic Etruria.

More substantial and difficult problems surround another apparently etruscanised version of the foundation of Rome. This is the eccentric narrative which Plutarch relates in the name of a certain Promathion. The account is as follows:-

"They say that Tarchetius, king of the Albans, who was most lawless and cruel, was visited with a strange phantom (*φάσμα θαυμάσιον*) in his house, namely, a phallus rising out of the hearth and remaining there many days. Now there was an oracle of Tethys in Etruria,

from which there was brought to Tarchetius a response that a virgin should have intercourse with this phantom, and she should bear a son most illustrious for his valour, and of surpassing good fortune and strength (*ῥώμη*). Tarchetius, accordingly, told the prophecy to one of his daughters, and bade her consort with the phantom; but she disdained to do so, and sent a handmaid in to it. When Tarchetius heard of this he was wroth, and seized both the maidens, purposing to put them to death. But the goddess Hestia appeared to him in his sleep and forbade him the murder. He therefore imposed upon the maidens the weaving of a certain web, assuring them that when they had finished the weaving of it they should be given in marriage. By day, then, these maidens wove, but by night other maidens, at the command of Tarchetius, unravelled their web. And when the handmaid became the mother of twin children by the phantom, Tarchetius gave them to a certain Teratius with orders to destroy them. This man, however, carried them to the river side and laid them down there. Then a she-wolf visited the babes and gave them suck, while all sorts of birds brought morsels of food and put them into their mouths, until a cow-herd spied them, conquered his amazement, ventured to come to them, and took the children home with him. Thus they were save, and when they were grown up, they

set upon Tarchetius and overcame him. At any rate this is what a certain Promathion says, who compiled a history of Italy". (Plutarch, Rom. 2.3-6).

In this bizarre account the received tradition about the twins is combined with several motifs familiar from other sources. Penelope's web is of course unmistakable, although its precise function in this context is disconcertingly obscure. Most interesting however is the unusual version of the conception of the twins, which has a number of well known parallels. Caeculus, the founder of Praeneste and ancestor of the Roman gens Caecilia was conceived from a spark from the hearth.⁽⁴²⁾ Modius Fabidius, the founder of Cures, was the son of a virgin and the god Quirinus, according to Varro (ap. Dion. Hal. II.48); the name Modius is etymologically related to the Latin muto (mutto) = phallus, and with the phallic deity Mutinus Titinus.⁽⁴³⁾ It is the phallic element in these stories that distinguishes them from comparable Greek myths such as that of Danae, and establishes their specifically Italic character. But the closest parallel to the Promathion story is the version of the birth of Servius Tullius, in which his mother, a slave of queen Tanaquil, is said to have conceived as a result of a union with a phallus which emerged from the hearth in the palace of King Tarquin.⁽⁴⁴⁾

The problems which these stories raise for historians

of Roman religion are very numerous, and it is entirely beyond my competence to make any comment about them, save that the Italic character of the myth seems assured.⁽⁴⁵⁾ Moreover, it seems likely that the myth of a procreating divinity in the form of a phallus from the hearth is Etruscan in origin.⁽⁴⁶⁾

For this reason the version of Promathion is of special interest to us, because it seems to be an Etruscan account of the birth of the Roman twins. This is indicated also by the fact that the king of Alba is called Tarchetius, which is no more than a variant of the name Tarquinius.⁽⁴⁷⁾ Moreover the oracle he consults is Etruscan - "an oracle of Tethys in Etruria". The name Teratius also seems to be Etruscan, and clearly recalls Tarutius, the rich Etruscan husband of Acca Larentia according to Licinius Macer (F.1 Peter). These Etruscan elements have led several scholars to take the view that the whole account attributed to Promathion originated among the Etruscans.⁽⁴⁸⁾

From a historical point of view we should like to know how the version of Promathion fits into the development of the tradition about the foundation of Rome, and in particular how it relates, both chronologically and materially, to the vulgate tradition established by the annalists.

Some scholars believe that the story reflects the memory of Etruscan rule in Latium⁽⁴⁹⁾ and even that it was

created in the period of Etruscan domination at Rome in order to justify the rule of the Tarquins.⁽⁵⁰⁾ This notion, that the account of Promathion originated in the political conflicts of the age of the Tarquins, has recently been elaborated by S. Mazzarino,⁽⁵¹⁾ who imagines that Servius Tullius was the leader of a "democratic" revolt against the regime of the Tarquins.

According to Mazzarino, Promathion should be identified with Promathos of Samos, a writer of the early fifth century B.C.⁽⁵²⁾ Promathos picked up the story of the origins of Rome from the Phocaeans adventurers of the Western Mediterranean, who had been friendly to Servius Tullius but were bitter enemies of the Tarquins. Thus it came about that the reports which the Phocaeans gave their countrymen about Rome were heavily influenced by the propaganda of Servius Tullius' revolutionary party, and that the account which Promathos/Promathion gave of the foundation of the city was little more than a projection back into the mythical past of the events of Servius Tullius' struggle against the cruel Tarquinius/Tarchetius.

This thesis is untenable, not only because of the *prima facie* objections to the equation of Promathion and Promathos,⁽⁵³⁾ but also because its reconstruction of the relations between the Phocaeans and Rome in the sixth century B.C. (on which everything depends) is for the most

part fanciful.⁽⁵⁴⁾ Mazzarino connects the Samian writer Promathos with the internal politics of sixth century Rome by a long and tenuous chain of arguments which turns out to contain a number of weak links. Some of these can be briefly enumerated.

(a) Mazzarino's evidence for close and friendly relations between the Phocaeans and Rome in the time of Servius Tullius consists entirely of the tradition that the latter established a cult of Diana on the Aventine. For Mazzarino this implies that "Servio era devoto alla dea dei Massilioti, coloni dei Focei" (Pensiero Storico I, p.195). This statement presupposes a large number of interdependent propositions, every one of which has been disputed by notable scholars. But if we accept, for the sake of argument, that the traditional date of the Aventine temple is correct, that the cult was the first of its kind among the Latins - i.e. that it was established earlier than that of Aricia, recorded by Cato (Origines F.58) - that it was inspired by the Ionian cult of Artemis at Massilia, and that it was borrowed directly from Massilia, and not through intermediaries such as the Etruscans⁽⁵⁵⁾ - even if we accept all this without question, as Mazzarino does, it nonetheless seems to me to be insufficient evidence for the kind of political or ideological association between the Phocaeans and Rome that Mazzarino imagines. His theory,

that the Phocaeans were so sympathetic to the aims of Servius Tullius that they related tendentious stories about him to their Samian friends, depends not so much on Servius Tullius's attitude to the Phocaeans as on their feelings towards him; but the foundation of a cult of Diana in Rome does not imply as a necessary consequence that the Phocaeans had any strong feelings one way or the other about the Romans.

(b) It is true that according to tradition Rome and Massilia had been friendly from the earliest times;⁽⁵⁶⁾ but our sources do not preserve any record of hostility to the Phocaeans under the Tarquins. Indeed, Trogus Pompeius asserted the exact opposite: "Temporibus Tarquinii regis ex Asia Phocaeensium iuventus ostio Tiberis invecta amicitiam cum Romanis iunxit..." (Justin, 43.3.4). Mazzarino's view that Servius Tullius' policy of friendship to the Phocaeans was peculiar to his reign and that it was reversed by his successor is not supported by any evidence at all. That Tarquinius Superbus was in any way involved in the Etruscan victory at Alalia is entirely arbitrary, as is the assumption that Theophrastus (Hist. Plant. V.8.2) refers to these events. Herodotus does not confirm Mazzarino's view; on the contrary, his narrative begs the question.⁽⁵⁷⁾

(c) Mazzarino argues that the version of Promathion could only have been created in the age of Servius Tullius

or soon after. He contends that the servile status of the twins' mother is a proof of this: only in the "democratic" reign of Servius Tullius, himself the son of a slave, could the founders of Rome have been presented as of servile origin. But the fallacy of this argument is demonstrated by the fortunes of the story of Servius Tullius himself.

There are two traditions about this king, as Gabba has shown.⁽⁵⁸⁾ In one version his servile origin, his sympathy for the plebs (indicated for example by his veneration of Diana on the Aventine), and his supposed intention to set up a republican constitution,⁽⁵⁹⁾ are all adduced to portray him as "ὁ βασιλεὺς δημοτικώτατος" (Dion. Hal. IV.75.3). On the other hand there are clear traces of a rival aristocratic interpretation, in which the story of his birth is modified, and his mother Ocresia becomes a prisoner of war, of noble birth.⁽⁶⁰⁾ These divergent interpretations reflect the political propaganda of the late Republic;⁽⁶¹⁾ Servius Tullius was treated by the Roman writers of the first century B.C. in very much the same way as Athenian political literature in the fourth century treated Solon.⁽⁶²⁾ The important point as far as we are concerned is that the "popular" interpretation of Servius Tullius was a late development which is not safely attested before Licinius Macer, whose 'popularis'

and anti-Sullan tendencies are well known.⁽⁶³⁾

The fact that the tradition about Servius Tullius's slave mother was accepted and even exploited by historians of the late republic indicates that Mazzarino was wrong to assert:-

"Nel terzo secolo (tanto meno poi nel primo secolo a.C.), mai uno scrittore avrebbe immaginato che i gemelli divini, quelli che comunemente chiamiamo Romolo e Remo, potassero nascere da una qualunque schiava".⁽⁶⁴⁾

The general validity of such a statement seems doubtful in any case.

(d) One further argument is decisive. The notion that Romulus' servile birth would be inconceivable in a work of the third century or later is based on the assumption that the version of Promathion originated in Rome: but there is absolutely no guarantee that it did, and as for Promathion himself, the one thing we know for certain is that he was a Greek. His name alone indicates this, and the play on the word *ῥώμη* (strength) suggests strongly that we are dealing with a text which was originally written in Greek.⁽⁶⁵⁾ The fact that Romulus is presented as the son of a slave does not therefore give any indication of the date of the formation of the story nor, a fortiori, of the date of Promathion.

It is clear enough that the correct interpretation of

Promathion's version depends largely on the question of the date of its composition. A high date (Mazzarino) seems unjustified, and most scholars are inclined to place Promathion in the Hellenistic period - at any rate no earlier than the late fourth century B.C.⁽⁶⁶⁾ Gabba has recently advanced four reasons for supposing that Promathion was relatively late:-⁽⁶⁷⁾

(1) The title of the work - *ἱστορία Ἰταλική* - cannot be very early.

(2) The theme of the twins is not attested in Greek accounts of early Rome before the middle of the fourth century.

(3) The dynasty of Alban kings was a late construction.

(4) A fragment of C. Fonteius Capito, preserved by John Lydus⁽⁶⁸⁾ suggests that the story attributed to Promathion might have been created by writers who studied the Etruscan discipline in the period of the Late Republic and early Empire.

The first three of these points were in fact already established by Mommsen,⁽⁶⁹⁾ and I accept them, with the reservation that (3) is not absolutely certain. Tarchetius does not occur in the traditional list of the Kings of Alba; the acute observation of E. Pais⁽⁷⁰⁾ that a certain Tarcinius is given as one of the kings of the Alban dynasty in the *Excerpta Barbari* (p.302 Frick) may or may not be

relevant. The fact that Tarchetius is referred to as a king of Alba certainly seems to connect the account of Promathion with the received tradition of Romulus and Remus, but whether it presupposes a knowledge of the Alban king list is another matter. The list was formed as a result of the discovery that an eighth century date for the foundation of Rome conflicted with the traditional chronology of Aeneas and the end of the Trojan War. This chronological gap was not recognised by Ennius and Naevius, who made Romulus a grandson of Aeneas; but Naevius knew of king Amulius, who must therefore have been originally independent of the Alban king list.⁽⁷¹⁾ The idea that Rome was a colony of Alba need not presuppose a list of Alban kings; probably it reflects a much older tradition.

The first two of Gabba's four points only give us a terminus post quem, and not a very precise one. The later decades of the fourth century cannot be ruled out entirely. The last of Gabba's suggestions, that the account is to be connected with writings on the Etruscan discipline, is a theoretical possibility, but no more than that.⁽⁷²⁾

Promathion seems to have adhered fairly closely to the vulgate tradition in his account of the exposure of the twins. This perhaps means that he knew the vulgate tradition, and simply altered it by the arbitrary addition and insertion of extraneous elements, such as the web of Penelope

and the myth of the phallus in the hearth. This interpretation was favoured by Mommsen, who argued that Promathion was a late Hellenistic compiler who conflated a number of separate themes. (73)

This view seems to me to be confirmed by the fact that the two basic elements from which the account has been built up contradict one another. The prediction which the oracle of Tethys offers king Tarchetius concerns only one child, not twins. (74) It therefore seems reasonable to suppose that the story of the phallus in the hearth was originally independent of the tradition about the twins, (75) and that the two were brought together by the late compiler Promathion, in his "History of Italy".

The tradition that the founder of Rome was the son of a " *φάσκα δαιμόνιον* " in the form of a phallus from the hearth can therefore be treated in isolation. The Italic character of this myth, which has already been referred to, means that the story came to the knowledge of the Greeks from local tradition; obviously it will have been a popular oral tradition in origin. (76)

As I have already pointed out, most of its characteristic elements point to an Etruscan account of the Roman foundation legend. It is perhaps to be connected with the Greek versions of the origins of Rome in which the Etruscans are said to have been involved in the founding

of the city (see above p.256f); but in this particular case we are not dealing with an erudite hypothesis which the Greeks constructed out of their own preconceptions - rather it is a genuine indigenous tradition which, in view of its Etruscan setting, appears to have emanated from the Etruscans themselves.

Heurgon argued that it originated in Caere, because the oracle of Tethys is to be located there.⁽⁷⁷⁾ Promathion, he suggests, may have been an Etruscanised Greek resident in Caere, who wrote an 'Italic history' in which the founding of Rome was presented from a Caerite point of view.

As it stands, Heurgon's view of the Promathion passage is no more than an unprovable hypothesis. But the suggestion that Etruscan (and particularly Caerite) historians might have addressed themselves to a wider public by writing in Greek rather than Etruscan is interesting and calls for further comment.

It may be said, first, that there is no a priori objection to the idea of an Etruscan writing in Greek; on the contrary, it is shown to be plausible by the numerous examples of hellenised barbarians who wrote histories of their own countries in Greek - especially in the Hellenistic age.⁽⁷⁸⁾ The hellenisation of Etruria is a fact that requires no argument; it is interesting to note, however, that Caere in particular was hellenised to an extent

unrivalled by the other Etruscan cities. The Caerites had a treasury at Delphi (Strabo V.2.3 p.220 C; cf. Hdt. I.167) and there were Greeks living in the city already in the sixth century B.C.⁽⁷⁹⁾.

It is also worth noting that the Etruscans had been treated rather unfavourably by Greek historians. Theopompus, Alcimus and Timaeus made objectionable remarks about the Etruscans' way of life.⁽⁸⁰⁾ Others wrote about the cruel treatment of prisoners by Etruscan 'pirates';⁽⁸¹⁾ the 'locus classicus' here is Herodotus's account of the behaviour of the Caerites after the Battle of Alalia (I.167). The Etruscans may have wanted to justify themselves in the eyes of the Greeks. We know that a patriotic tendency of this kind was characteristic of other barbarian historians writing in Greek - such as Berossus and Manetho. Manetho attacked Herodotus's account of Egypt (F. Gr. Hist. 609 F.1), and Fabius Pictor's chauvinism is well known (see below, p.275).

.....

(iii) Caere and the Gauls.

These general points should be considered further in the light of an important passage from the fifth book of Strabo (V.2.3 p.220 C). After completing his account of Etruscan origins, of Demaratus and the Tarquins, and

of Lars Porsenna, Strabo embarks on an additional excursus on Caere, beginning with the words "περὶ μὲν τῆς ἐπιφανείας τῶν Τυρρηνῶν ταῦτα, καὶ ἔτι τὰ τοῖς Καίρεταιοῖς πραχθέντα". To differentiate in this way between the Tyrrhenians and the Caerites is in itself remarkable, and can only indicate that Strabo has changed from a source dealing with the Etruscans in general to one which was confined to the Caerites in particular. The character of the latter source is clearly demonstrated by the polemical tendency of the so-called Caere excursus, which runs as follows:

"(The Caeretani) defeated in war those Galatae who had captured Rome, having attacked them when they were in the country of the Sabini on their way back, and also took away from the Galatae against their will what the Romans had willingly given them; in addition to this, they saved all who fled to them for refuge from Rome, and the immortal fire, and the priestesses of Vesta. The Romans, it is true, on account of the bad managers which the city had at the time, do not seem to have remembered the favour of the Caeretani with sufficient gratitude, for, although they gave them the right of citizenship, they did not enrol them among the citizens, and even used to relegate all others who had no share to the equal right to "the Tables of the Caeretani". Among the Greeks, however, this city was in good repute both for bravery and for righteousness;

for it not only abstained from all piracy, although particularly well suited for it, but also set up at Pytho what is called "the Treasury of the Agyllaei"; for what is now Caere was formerly called Agylla..." etc. (from the Loeb translation).

In his commentary on this passage, W. Aly argued that Strabo synthesised information taken from a Roman and a Greek source, the break between the two being indicated by the phrase "Παρά δὲ τοῖς Ἑλλησιν".⁽⁸²⁾ But a synthesis by Strabo himself is unlikely, as was shown by M. Sordi, the first to grasp the true significance of this account.⁽⁸³⁾ There can be no doubt about the unity of the passage as a whole, because of the deliberate contrast between the respective attitudes of the Romans and Greeks towards Caere:

« οἱ γὰρ οὐκ ἔβουλον ἑκείνην ἀποικισθῆναι τῆν χάριν αὐτοῖς δοκοῦσι παρά δὲ τοῖς Ἑλλησιν εὐδοκίμησεν ἡ πόλις αὐτή... » etc.

The evident anti-Roman tendency is hardly characteristic of Strabo, as Sordi pointed out, and must therefore go back to his source. That such an overtly anti-Roman passage could have come from a Roman source is not absolutely inconceivable, but it is extremely unlikely.⁽⁸⁴⁾ The only serious possibilities are that Strabo's source was either Greek or Etruscan. We may consider them in turn.

A Caerite victory over the Gauls was certainly recorded

by Greek historians because it appears in Diodorus, whose source was probably either Timaeus or Philistus via Timaeus.⁽⁸⁵⁾ But Strabo's source cannot have been the same as Diodorus', because the latter sets the battle in a different context. In Strabo the Celts were attacked on their way back from Rome, whereas in Diodorus Rome appears only incidentally; the Gauls, he says, had passed through Roman territory on their way back from Iapygia, when the Caerites⁽⁸⁶⁾ set upon them in the "Trausian plain" (Diod. xiv, 117.6). The two accounts almost certainly refer to the same event,⁽⁸⁷⁾ but Strabo's version is distinguished by its peculiar 'tendenz'. It expresses an anti-Roman point of view by dwelling on Rome's unfair treatment of the Caerites, who had helped the Romans in their hour of need, but had not been fairly rewarded for their generosity. This emphasis on Rome's unjust treatment of her sometime friends is perhaps suggestive of the hostile Greek propaganda of the late third and early second centuries B.C., which sought to impugn Rome's pretensions to international justice and "fides".

The traditional annalistic account of the expansion of Rome as a series of justifiable wars ("iusta bella") goes back to Fabius Pictor, who seems to have tried to justify Roman policy to his readers;⁽⁸⁸⁾ it seems likely also that he was to some extent impelled to do so by the

hostile assertions of Greek writers such as Philinus of Acragas:

"ὅτι γὰρ τὴν αἵρεσιν καὶ τὴν ὅλην εὐνοίαν, Φιλίνῳ μὲν πάντα δοκοῦσιν εἰ Καρχηδόνιοι πεπραχῆσαι φρονίμως, καλῶς, ἀνδρωδῶς, εἰ δὲ Ῥωμαῖοι τὰναντία, Φαβίῳ δὲ τοῦρπαλιν τοῦτων."

(Polybius I.14.3 = F.Gr.Hist. 174 F.2).

It was Philinus who declared that the Romans were the aggressors in the First Punic War, and that they had crossed into Sicily "παρὰ τοὺς ὄρκους καὶ τὰς συνθήκας" (Polyb. III.26 = F.Gr.Hist. 174 F.1), and this is perhaps only one manifestation of a general tendency of some Greek historians to portray the policy of Rome as an attempt to expand the empire and to enslave the Greeks. A tendentious account of the way in which the Romans had treated Caere perhaps fits into this pattern.

It is worth noting that a fragment of Metrodorus of Scepsis, whose hatred of Rome was proverbial, (89) accused the Romans of conquering Volsinii for the sake of 2000 statues (F.Gr.Hist. 184 F.2). This suggests that an episode in the history of relations between Rome and Caere could also have been exploited by Greek propagandists as an instance of Roman injustice. Indeed it seems to me perfectly conceivable that the source of Strabo's Caere excursus was Metrodorus of Scepsis himself.

But this conclusion is admittedly unprovable; the

explanation offered by Sordi, that Strabo's source was a local Caerite account, seems to me to be equally possible. If we are dealing with a local history of Caere, we must suppose that it was written in Greek, because the odd statement that the Caerites abstained from piracy, which is a direct contradiction of what we read in other sources,⁽⁹⁰⁾ seems to be addressed to the Greeks; and the qualities which the Greeks are said to have discerned in the citizens of Caere - *ἀνδρεία καὶ δικαιοσύνη* - are precisely those which were lacking in the Romans, who were cowardly in the face of the Gauls and unjust in their treatment of the Caerites. This is similar to the method of Philinus, in his comparison of the activities of Rome and Carthage - "*πάντα δοκοῦσιν οἱ Καρχηδόνιοι πεπραχθῆαι φρονίμως, καλῶς, ἀνδρωδῶς, οἱ δὲ Ῥωμαῖοι τὰναντία.*" Perhaps Fabius Pictor had to confront similar accusations concerning the Gallic invasion and the relations between Rome and Caere.

But internal evidence is not sufficient on its own to indicate whether the source of Strabo's Caere excursus was the work of a citizen of Caere writing in Greek, or an independent Greek historian who was hostile to Rome, or perhaps, as Heurgon suggested, a Greek resident in Caere writing from a Caerite point of view.⁽⁹¹⁾ But as no external evidence is available apart from comparative

material, the question must remain open.

.....

(iv) Conclusion

The results of the foregoing discussion are mostly disappointing. The case for Etruscan literary historiography seems reasonable enough in theory, but is unimpressive in terms of concrete evidence. A synthesis of the views and speculations of earlier scholars seemed to me to be necessary because the a priori assumption that the Etruscans produced their own historical literature has sometimes been used to substantiate theories in which passages of extant texts are traced back to Etruscan sources. But the hypothetical existence of Etruscan historiography should be a consequence, rather than a confirmation, of such theories.

The attribution of any given passage to an Etruscan historian does not depend simply on whether or not it derives from "an Etruscan source"; the characterisation of Etruscan sources, when they can be definitely identified as such, is by no means a straightforward matter. It is necessary to draw careful distinctions, first between oral tradition and written sources, secondly between primary material (e.g. official documents, family archives, etc.) and secondary literature, and thirdly between historiography

and other types of literature, such as poetry, drama, sacred writings, and so on.

Of the passages we have been discussing only Strabo's Caere excursus can be said with any confidence to go back to a literary historical source; but here it is difficult to be sure that the source was Etruscan. As for the Etruscan histories mentioned by Varro, we saw in the last chapter that he was probably referring to a documentary collection of archaic material, rather than to literary histories of the Graeco-Roman type. But although we might play down the significance of Etruscan historical literature, the fact remains that independent Etruscan stories and traditions have occasionally found their way into the Roman tradition.

The evidence reviewed in these two chapters throws some interesting light on the way in which Roman historians and scholars came to be influenced by legendary and historical traditions which originated outside Rome. All the passages we have examined are connected in one way or another with events of Roman history. To some extent this is not surprising; since Roman researchers were predominantly occupied with the history of Rome, it is natural that they should have consulted non-Roman sources and borrowed from them only when the information they contained had some relevance to the history of Rome. But this does not

tell the whole story. There are clear traces also of deliberate attempts to romanise stories which originally had nothing to do with Rome. I am not thinking primarily of the romanisation of the Mastarna tradition - because the identification of Mastarna with Servius Tullius was probably the result of an honest if misguided attempt at historical reconstruction. I am referring rather to the arbitrary manipulation of independent pieces of tradition which have been inserted into contexts where they do not really belong.

This, I suggest, is the explanation of the Etruscan elements in the narrative of the Veientine War. I have argued that the surviving accounts of this war do not reflect the use of an independent Etruscan account of the same events; rather, the tone of the Livian tradition has been coloured by the efforts of late annalists, who were responsible for introducing references to half-understood ideas and practices of Etruscan religion. The same applies to the fictitious reports of deliberations among the Etruscans at the annual meetings of the sacred federation of the duodecim populi - in itself a perfectly genuine institution. The story of the king of Veii who disrupted the games by withdrawing the performers who were his own slaves may well reflect a genuine priestly tradition; but the connection of this story with the war

between Rome and Veii is in my opinion an arbitrary distortion. That the king's behaviour at the games provided the Etruscan league with a motive for refusing to help Veii is almost certainly a Roman fabrication.

There are many other instances of this process. We may note, for example, the view of our sources that Tarquin the elder was the son of the Corinthian émigré Demaratus.⁽⁹²⁾ The story of the flight of Demaratus is based on a genuine tradition, as A. Blakeway demonstrated.⁽⁹³⁾ The account will have been preserved in a source which was either Greek or Etruscan, but certainly not Roman.⁽⁹⁴⁾ But the notion that Demaratus was the father of Tarquin is unlikely to have been part of the original tradition, and probably reflects a secondary manipulation.

Another example, which has significant implications as far as the study of Cato is concerned, is the anecdote of Arruns and Lucumo of Clusium, which I discussed in an earlier chapter (above, chapter IV, sect.ii, pp.124-127). I suggested there that the story of the seduction of Arrun's wife had originally existed on its own as a local legend, and that it had subsequently been incorporated into the Roman tradition about the Gallic catastrophe by an annalist who had taken it from Cato's Origines.

This conclusion is admittedly hypothetical; but a certain amount of circumstantial evidence combines to make

it plausible. First it is clear that the arbitrary insertion of pieces of independent local tradition into contexts where they do not really belong is a characteristic feature of the Roman annalistic method. Secondly the role of Cato's Origines in bringing independent traditions of non-Roman Italy to the notice of Roman historians is more important than has been realised hitherto. (95)

Confirmation of both these points will come, I hope, from a discussion of the evidence relating to local traditions of Campania, which will form the subject of the next chapter.

(v) Appendix A: The Etruscans in the Aeneid

The theory that Livy's description of the siege of Veii goes back to an Etruscan source has been developed further by M. Sordi, who finds support for it in a supposedly analogous account in Vergil's Aeneid.⁽⁹⁶⁾ She argues that certain peculiarities in Vergil's picture of relations between Aeneas and the Etruscans are the product of a deliberate attempt to transpose the historical events of Veii into the mythical past. Vergil's Aeneas, she believes, is a legendary prototype of Camillus, and Mezentius, described as "contemptor divum" (Aen. 7.648 and 8.7), corresponds to the impious king of Veii in Livy.

If the Aeneid really contains reflections of the events of the Veientine war, one might be tempted to imagine that Vergil introduced them himself. But Sordi contends that the basic pattern of events in the later books of the Aeneid were already established in the fourth century B.C., because an alliance between Aeneas and the Etruscans (a characteristic feature of Vergil's account) is explicitly referred to by Lycophron (Alex. 1245-9), and hinted at by Alcimus, whose statement that Aeneas married an Etruscan princess perhaps reflects the same tradition (F.Gr.Hist. 560 F.4).

The duplication of mythical and historical events is said to be characteristic of Etruscan thought. Referring

to the system of saecula, Sordi writes: "per gli Etruschi storia e profezia erano inscindibili ed appartenevano, in un certo senso, allo stesso genere letterario" (pp.179-180). Precisely what she has in mind is not entirely clear,⁽⁹⁷⁾ but as far as I understand it her view is that the historical and mythical accounts of these events both go back to a single (Etruscan) source, in which the war of Veii and a parallel account of Aeneas' war against Mezentius were somehow presented side by side.⁽⁹⁸⁾

I do not intend to discuss Sordi's thesis in detail, because it is in my opinion entirely wrong; suffice it to say that the alleged resemblances between Livy and Vergil are not really close enough to warrant the conclusion that the two accounts are based on a common source. We may note, for example, that Mezentius was king of Caere, not of Veii; indeed, there is no mention of Veii anywhere in the Aeneid.⁽⁹⁹⁾ Many similar objections could be made.⁽¹⁰⁰⁾ But even if Livy and Vergil have drawn on a common source, there is no reason why it should be identified as the source of Lycophron 1245 ff. Sordi's arguments on this point constitute a basic methodological error. The elements which are common to Vergil and Lycophron are not the same as those which are (supposedly) common to Vergil and Livy. It cannot be said that Lycophron's narrative, on which so much depends, even remotely resembles Livy's account (or

any other account) of the war between Rome and Veii. The important common element of Livy and Vergil is the complex pattern of conflicting interests which create a division within the Etruscan world. In the Aeneid Mezentius is rejected by his people and fights in isolation against Aeneas and the other Etruscans; in Livy the Veientines arouse the enmity of the Etruscan league and have to fight single-handed against Rome.⁽¹⁰¹⁾ There is nothing like this in Lycophron, who simply speaks of Tarchon and Tyrrhenus as allies of Aeneas. This alliance does not correspond at all closely to anything that took place at Veii. In that war the Etruscans had not been allies of the Romans; as we have seen, the attitude of the cities during the war was neither consistent nor unanimous (above, p.246).

.....

Miss Sordi takes as her starting point the fact that Vergil's account differs in certain fundamental respects from the traditional Latin version. In this traditional account the Etruscans, led by Mezentius, are enemies of the Trojans and allies of the Rutulians.⁽¹⁰²⁾ But in the Aeneid Mezentius does not lead the Etruscans; he is a lone figure, exiled from his kingdom in Caere, who joins Turnus on his own account. The cities of Etruria, stirred to anger by the cruelties of Mezentius during his

period of rule, make an alliance with Aeneas in order to take revenge on their former king (Aen. 8.481 ff.). Now it must be admitted that Vergil's narrative is eccentric, and if we are going to reject Sordi's explanation of the singular role of Mezentius in the Aeneid, we ought in fairness to substitute some alternative hypothesis.

As far as we can see, before Vergil there were two traditions about Aeneas and the Etruscans. First, there was the version reflected in Lycophron and Alcimus, that the Etruscans were friendly to Aeneas. We have rejected Schachermey~~er~~'s view that such stories were deliberately created for a political purpose in an attempt to connect the beginnings of Rome with the Etruscans (above pp.256-258.); and we have argued against Sordi's theory of a transposition of the events of the siege of Veii into a mythical context. The strongest objection to both these hypotheses is that they are unnecessary. As I mentioned earlier, we now have archaeological evidence for the popularity of the Aeneas legend in Etruria in the archaic period.⁽¹⁰³⁾ It seems likely that Aeneas was treated in some of the Etruscan cities as a founder hero; but when he became firmly established in popular belief as the ancestor of the Romans, his relationship with the Etruscans, which had almost certainly found its way into the works of Greek

writers well before that time, was watered down and became a mere alliance. This process undoubtedly explains the version of Lycophron, in which Aeneas arrives in Etruria first, and proceeds from there into Latium and Campania.

In direct opposition to this tradition of a close association between Aeneas and the Etruscans is the story of Mezentius of Caere. This age-old Latin tradition grew up as an aetiological legend relating to the festival of the 'vinalia', and as such it was fully reported by Cato in the first book of the Origines (F.8-12); already in Cato Mezentius is presented as an ally of Turnus and an enemy of Aeneas.

Now it seems to me that the unusual features of Vergil's account can all be explained by the fact that these two contradictory traditions have been combined and reconciled in the Aeneid. My own view is that the harmonisation of the two versions was the work of Vergil himself. Certainly the method of reconciliation - i.e. the isolation of Mezentius - seems to me to be characteristic of Vergil's technique, because there is a close parallel in the story of king Latinus in the seventh book of the Aeneid.

The position of Latinus in the Roman historical tradition is altogether rather difficult. Here again there

are two conflicting traditions: first that Latinus married his daughter to Aeneas and joined the Trojans against Turnus; secondly that the Latins, the subjects of Latinus, fought against the Trojans when they arrived in Italy. The earliest Roman version of these events known to us is again that which Cato gave in his Origines; in the fragments which Servius has preserved for us the discrepancy (if nothing else) is evident. In F.9 Aeneas fights against both Latinus and Turnus; but in F.11 Aeneas marries Lavinia as soon as he arrives ("simul ac venit ad Italiam"), and fights together with Latinus against Turnus. Obviously Servius has given us a confused account of what Cato really said; perhaps Cato gave two rival versions side by side. In any case, there is no doubt that a serious discrepancy existed.⁽¹⁰⁴⁾ Livy was baffled ("duplex inde fama est"), and could not make up his mind whether Latinus and Aeneas came to terms before coming to blows, or the other way round (Liv. I.1. 6-7).

But Vergil could not, of course, present two rival versions side by side in the manner of a historian. He had to create a coherent and intelligible story out of these conflicting traditions. His solution was to dissociate Latinus from the activities of the Latins. Latinus stands alone in his favour to Aeneas (7.373 ff.). In 7.586 ff. he opposes the views of Turnus and his followers

("ille velut pelagi rupes immota resistit..." etc.), and when he fails to dissuade Turnus from fighting he curses him (7.596 ff.) and then abdicates his power.

It seems to me that this is precisely comparable to Vergil's treatment of Mezentius; he too is deprived of this throne, and for much the same reason - to allow him to follow a different course from his people.

A second factor which may have encouraged Vergil to present the story of Mezentius in this peculiar way is his own Etruscan background. Vergil came from Mantua; the inclusion of Mantua among the allies of Aeneas (Aen. 10.198 ff.) is not fortuitous, but is due to the poet's pride in his own origins. This helps to explain why he incorporated the tradition which made the Etruscans allies of Aeneas, rather than follow the usual Roman version - although he could not entirely ignore the latter.

Mezentius' expulsion from Caere is, I suggest, a device introduced by the poet. One of the results of the innovation is that the Etruscans in general are exonerated from the notorious activities of Mezentius commemorated by the festival of the 'vinalia'. But Vergil goes further: to provide the Caerites with a motive for driving him out, Vergil charges Mezentius with excessive cruelty:

"Mortua quin etiam iungebat corpora vivis,
componens manibusque manus atque oribus ora,

tormenti genus, et sanie taboque fluentis
 complexu in misero longa sic morte necabat" (8.485-8).

The interesting point about these lines is that this particular form of torture is elsewhere stated to have been a common practice among Etruscan "pirates". (105) One suspects, and the reference to pirates seems to confirm the suspicion, that the accusation derives from Greek prejudice against the Etruscans. What Vergil seems to be doing here is making Mezentius a scapegoat and charging him with crimes traditionally associated with the Etruscans in general. This interesting interpretation was offered by J. Gagé. (106)

It seems, therefore, that the isolation of Mezentius was Vergil's own idea. He clearly felt no hesitation in making radical changes of this nature; the expulsion of Mezentius from Caere is as much his own innovation as the death of the exiled tyrant at the hands of Aeneas. In Cato's Origines and other sources he outlives Aeneas and Turnus and is killed by Ascanius.

(vi) Appendix B: The Etruscan "League" (107)

There can be no doubt that the Etruscan cities were united in some kind of regular and permanent federal association, but its precise character is very much open to question. That the league had a religious function seems fairly certain,⁽¹⁰⁸⁾ and Livy's account of it is authentic enough in this respect at least. According to Livy the concilia were held annually⁽¹⁰⁹⁾ and in the same place (the fanum Voltumnae); religious ceremonies were held which involved the performance of games (solemnia ludorum - Liv. V. 1.4), and the election of a sacerdos (ibid.). These features correspond closely to those referred to in a rescript of Constantine,⁽¹¹⁰⁾ which speaks of an ancient institution (instituto consuetudinis priscae), meeting annually (per singulos annorum) near Volsinii (apud Volsinios Tusciae civitatem - the fanum Voltumnae?),⁽¹¹¹⁾ electing a sacerdos (sacerdotes creentur) and holding games and theatrical performances (ludos schenicos et gladiatorum munus). This would seem to bear out Livy's view that the Etruscans were united in a religious federation at some time during their history.

But Livy goes much further than this, in that he attributes an effective political function to the league. It is not enough to defend Livy's presentation of the league as a political association on the grounds that

meetings convened for sacred purposes would naturally have given rise to informal political discussion of matters of general importance to the Etruscan peoples as a whole.⁽¹¹²⁾ This is a perfectly reasonable proposition in itself, but it does not account for Livy's statements that the league had a regularly constituted political function and overriding control over the foreign policy of the member states. But this must be the implication of the passages in which the Veientes are accused of making war on their own account without consulting the league;⁽¹¹³⁾ and again, the statement "sanguini tamen nominique et praesentibus periculis consanguineorum id dari ut si qui iuventutis suae voluntate ad id bellum eant non impediunt" (V.17.9) only makes sense if the constituent member states were bound by the decrees of the league council.

Dionysius of Halicarnassus attributes the same sort of powers to the league in his account of the war of Tarquinius Priscus against the Etruscans:

" ψήφισμα ποιούνται πάσας τὰς τῶν Τυρρηνῶν πόλεις κοινῇ τὸν κατὰ Ῥωμαίων πόλεμον ἐκφέρειν, τὴν δὲ νῆ μετέχουσαν τῆς στρατείας ἐκπονοῦσαν εἶναι. "

(Dion. Hal. III 57.1).

That the Etruscan league was a politically constituted organisation at some time during its history is a perfectly

tenable view; but there is much disagreement about the date at which this political function was in operation. Broadly speaking there are two schools of thought: Pareti holds that the Etruscan concilium was a purely religious association in origin, and only acquired political significance in the very latest period of Etruscan independence - in the Third Samnite and Pyrrhic wars.⁽¹¹⁴⁾ But the more general opinion is that the league was originally a political organisation, but that it later disintegrated and became purely sacral. The sacerdos of the later league was in fact no more than the primitive league king, or 'lucumo', whose function had been reduced "ad sacra".⁽¹¹⁵⁾

That Etruria had been united in early times and only subsequently broke up into more independent units is also the view of the ancient sources, such as Strabo, who writes:-

" τότε γὰρ οὐκ ἔτι ἐνὶ ἡγεμονίᾳ ταπτόμενοι μέγα ἰσχυροὶ, χρόνῳ δὲ ὕστερον διαλυθῆναι τὸ σύστημα εἰκὸς καὶ κατὰ πόλεις διασπασθῆναι βία τῶν πλησίων χωρῶν εἰζάντας· οὐ γὰρ ἂν χώραν ευδαίμονα ἀφέντες τῇ θαλάττῃ κατὰ ληστείαν ἐπέθεντο, ἀλλὰ πρὸς ἄλλα τραπόμενοι πελάγη, ἐπεὶ, ὅπου γε συμπνεύσαιεν, ἱκανοὶ ἦσαν οὐκ ἀρμόνασθαι μόνον τοὺς ἐπιχειροῦντας αὐτοῖς, ἀλλὰ καὶ ἀντεπιχειρεῖν καὶ μακρὰς στρατείας ποιεῖσθαι."

(Strabo V.2.2 p.219 C).

Some scholars⁽¹¹⁶⁾ have accepted this as reliable evidence; but it must be admitted that Strabo's remarks do not represent any long standing tradition, but rather an inference, by Strabo himself or his source, in an attempt to reconcile the political disunity of Etruria in historical times with the legendary tradition of the arrival of all the Etruscans in a unified group under the leadership of Tyrrhenus, and the foundation of the dodecapolis by Tarchon.⁽¹¹⁷⁾ This primeval unity, it was to be supposed, must have gradually disintegrated. The legends of Etruscan origins, which presupposed a unified nation under a single ruler, are perhaps partly responsible for the view of our sources that the duodecim populi were originally united under the rule of a single king, or 'lucumo'.⁽¹¹⁸⁾

But the foundation myths cannot be accepted at their face value. Even if the Lydian tradition in some way reflects something that really happened, the notion that the Etruscans arrived in one group in the manner of a Greek colony is hardly tolerable as a historical account.⁽¹¹⁹⁾

It is also to be noted that the references to a supreme king of the Etruscans are associated in our sources with the question of the origin of the fasces. According to Livy, the lictors who attended the Roman king were twelve in number "quod ex duodecim populis communiter

creato rege singulos singuli populi lictores dederint".⁽¹²⁰⁾
 Dionysius of Halicarnassus says that when the Etruscans
 submitted to Rome after their defeat by Tarquinius Priscus,
 they brought to him the twelve axes " λαβόντες ἐξ ἑκάστης
 πόλεως ἓνα " (Dion. Hal. III 61.2).

There is little doubt that the Roman fasces were
 of Etruscan origin,⁽¹²¹⁾ and it is probable also that
 their number really did symbolise the duodecim populi of
 Etruria; but it is open to doubt whether the Etruscans
 were ever ruled by a single king except in the world of
 legend, and the account in Dionysius of the surrender of
 the fasces to Tarquinius Priscus is a palpable fiction.
 Alföldi, who denies the truth of the notion that the
 twelve axes were adopted by the Romans in the age of the
 kings, nonetheless argues that there was a supreme king
 of all the Etruscans in the regal period - apparently
 without realising that the evidence for both propositions
 is the same.⁽¹²²⁾ The aetiological character of the
 statements about the supreme king of the XII populi
 must surely invite suspicion.

The constitutional position of the supreme lucumo
 is something which our sources do not make clear. Was
 he elected, as Livy asserts, or did his position depend
 on the hegemony of the city from which he came, as Servius
 seems to imply? - or was the office held by the individual

city lucumones in turn and in strict rotation?⁽¹²³⁾

Was his period of office lifelong, or was it annual? - or was it perhaps an irregular post, like that of the Roman dictator in the Republic, created only infrequently and for a specific purpose?⁽¹²⁴⁾ What is the relationship, if any, between the supreme lucumo and the "zilath mechl rasnal" attested in Etruscan inscriptions?⁽¹²⁵⁾

It is easy to assert that the zilath was an annual republican magistrate who replaced the earlier lucumo (Etruscan "lauchume") after the overthrow of the monarchy. But the Pyrgi inscription describes the ruler of Caere as "zilath" and adds that he was in the third year of his rule. Moreover the Phoenician version designated him "melek" (= "king"). As for the "zilath mechl rasnal", we do not know precisely what the term implies; "rasnal" perhaps means 'Etruscans' or 'Etruria' if it is the same word as "Rasenna", which is what the Etruscans called themselves (Dion. Hal. I 30.3). But "mechl" remains obscure. It is uncertain whether the "zilath mechl rasnal" was a republican version of the supreme lucumo, or whether he was a collegiate magistrate, a representative of his city at the league council, and the republican equivalent of one of the twelve lucumones.⁽¹²⁶⁾ There is a similar difficulty about the title "praetor Etruriae",⁽¹²⁷⁾ which may or may not be the Latin equivalent of "zilath

mechl rasnal". And how does the "praetor Etruriae" (and for that matter the "zilath mechl rasnal") relate to the sacerdos of Livy V 1.5 and the sacerdotēs of CIL XI 5265? The only thing we can be sure about is the extent of our ignorance in these matters.

What we can say, however, is that a lot of circumstantial evidence militates against the view that the XII populi were in the habit of combining their forces for joint enterprises. Dionysius of Halicarnassus says:-

" εἰ δὲ κοινῇ γίνοντο τῶν δώδεκα πόλεων στρατεία, τοὺς δώδεκα παρέκεις ἐνὶ παραδίδοσθαι τῷ λαβόντι τὴν αὐτοκράτορα ἀρχήν. "

(Dion. Hal. III 61.3).

Strabo, in the passage quoted above (p.292.), also refers to such enterprises, which he designates " *νακρὰς στρατείας* ". This phrase must relate to the distant colonising expeditions which the Etruscans undertook in Campania and the valley of the Po. Some modern scholars accept the view of the sources that these expeditions were the result of concerted action by all the Etruscan cities.⁽¹²⁸⁾ Livy says that each of the twelve cities founded in the Po valley was the colony of one of the original twelve in Etruria proper - "trans Appenninum totidem, quot capita originis erant, coloniis missis" (Liv. V 33.9). The same sort of thing is implied for the Etruscan settlement in Campania, where twelve cities were

founded according to Strabo (V 4.3 p.240 G.).

Clearly these undertakings do imply a degree of united action, but the idea that the colonisation of the Po valley and Campania was the achievement of a political federation of twelve cities which founded twelve new cities in each area is obviously too artificial; and it is no more reliable as historical evidence than the notion, expressed in our sources, that the colonisation took place in the mythical period under the leadership of heroes such as Ocnus, or Tarchon himself.⁽¹²⁹⁾

In fact these colonising expeditions took place in the sixth century B.C.,⁽¹³⁰⁾ which happens also to be the period in which we hear of a supreme lucumo, and of the surrender of the fasces to the Romans. But this is precisely the period for which there is good evidence of violent political hostility and rivalry among the Etruscan cities. It is the age of the great condottieri - of the Tarquins, of Mastarna and the brothers Vibenna, and of Lars Porsenna of Clusium. The François tomb-painting at Vulci shows a struggle between two groups of rival Etruscan cities. One of the "elogia Tarquiniensia" speaks of a war between Tarquinii and Arretium, and of the defeat by Tarquinii of a king of Caere, which dates the events to the (presumably archaic) period when there were still kings at Caere.

To speak of a political federation of twelve cities united under the command of a single lucumo at this time is to reject all the historically reliable evidence in favour of mythical stories. Of course one would not deny that some cities may have formed ad hoc alliances when their interests coincided; the victims of the Vibennae in the François tomb-painting represent a group of cities which were presumably acting in concert. The archaeological record of Capua suggests strongly that more than one city took part in its foundation.⁽¹³¹⁾ But none of this evidence indicates a league of twelve cities comprising the entire nomen Etruscum in a formal and permanent political association. The statements in our sources merely reflect an attempt to give an aetiological account of the origin of the fasces, and to project back into the regal period - sometimes even to Romulus⁽¹³²⁾ - the claim of Rome to be the overlord of all the Etruscan cities.

Notes to Chapter VIII

- (1) R. M. Ogilvie, A Commentary on Livy, Books I-V, 1965, p.628.
- (2) Livy IV.24.2, 25.8, 61.2; V.1.6; 17.7 f.; VI.2.2.
- (3) Cf. J. Bayet, Tite Live, Histoire romaine V, Appendix III, p.127 ff.; M. Sordi, I rapporti romano-etruschi e l'origine della "civitas sine suffragio", Rome 1960, p.10.
- (4) As revealed, for example, in the theory of "saecula". Cf. M. Sordi, op.cit., p.177 ff.
- (5) Livy V.15.1; cf. Cic. de div. I.100; J. Bayet, op.cit. p.128.
- (6) J. Bayet, op.cit. p.127 ff.; M. Sordi, op.cit. p.10 ff; R. M. Ogilvie, op.cit. p.628; J. Hubaux, Rome et Véies, 1958, passim, esp. 121 ff; J. Gagé, Huit recherches sur les origines italiques et romaines, Paris 1950, p.73 ff; id., "Mél. Arch. et Hist." 66, 1954, p.39 ff. In general J. Heurgon, Rome et la Méditerranée occidentale, Paris 1969, p.297.
- (7) Although M. Sordi's extension of it is unacceptable. Her view is that the source was the work of an Etruscan historian of the fourth century B.C. who also transposed the events of Veii into the mythical context of the story of Aeneas. For a fuller discussion of this hypothesis see Appendix A, below pp.282 - 289.
- (8) I have discussed the character of the Etruscan federation more fully in Appendix B, below pp.290-298.
- (9) On which see A. Alföldi, Early Rome and the Latins, p.137 ff.
- (10) L. Pareti, La disunione politica degli Etruschi, "Rendic. Pont. Accad. Arch." 7, 1931, 89-100 (= Studi minori di storia antica I, 1958, pp.283-94).

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| (11) | LIVY V 17.9:- | DION. HAL. IX 1.2:- |
| | <p>"id dari ut si qui iuventutis suae voluntate ad id bellum eant non impediunt. Eum magnum advenisse hostium numerum fama Romae erat."</p> | <p>"τέλος ἐξήνεγκεν ἐξεῖναι τοῖς βουλομένοις τυρρηνῶν μετέχει τῆς στρατείας· καὶ ἐγένετο χεῖρ ἀξιόμαχος ἢ τοῖς οὐλεντένοῖς ἐκούσιως τοῦ παλέου συναρπένη."</p> |
- (12) Cf. G. De Sanctis, Storia dei Romani II², pp.135-6.
- (13) L. Pareti, Studi minori I, p.347.
- (14) Ogilvie, op.cit. p.632 on V.1.3.
- (15) An official chronicle which originates as a list of names - of priests, kings, magistrates or whatever - develops with the addition of brief annotations of events; the information contained in these notes consists primarily of matters affecting the office whose representatives are enumerated in the list, who are generally responsible for maintaining the list and keeping the chronicle (cf. the Rhodian psephisma in Ditt. Syll.² 723). Thus there are regular entries concerning games and festivals in the list of priests of Apollo Erethimios from Camirus (Ditt. Syll.³ 724); and we find in the Attic inscription Syll.³ 88, under the names of the Athenian archons with the formula "ἐπὶ τούτου", notes of annual events of importance for the sanctuary of Asclepius. Further examples are given in R. Laqueur, R.E. s.v. "Lokalchronik" 1087 ff. The tabulae which formed the basis of the Roman pontifical chronicle contained notices of events which were of concern to the college of Pontiffs, such as eclipses (Cato, Origines F.77; Cic. de rep. I.25) or prodigies (Gell. Noct. Att. IV.5). The origins of the notices in the pontifical tabulae can be traced back ultimately to the practical requirements of the college, as C. Cichorius pointed out - R.E. s.v. "Annales", 2250; cf. R. Laqueur, art.cit. 1039 f; F. Jacoby, Atthis, p.63 f. The deliberate chronicling of events of a more general historical nature was a later development and quite distinct from the perfunctory type of record which I have been describing.

(cf. Jacoby, op.cit. p.175). It is now widely recognised that "historical chronicles" (i.e. the recording of events of general importance) were inspired by literary historiography, rather than the other way round. But the account in Livy V.1.3 f. of the disruption of the games at the fanum Voltumnae belongs rather to the more archaic type of notice, and is more characteristic of an entry in a priestly chronicle. Cf. the remarks of Mommsen, Ges. Schr. VIII, p.35, who suggested that the old Etruscan story ("vetus fama Etruriae") in Pliny N.H. II. 54.140 was preserved because of a connection with the shrine of Voltumna at Volsinii.

- (16) J. Gagé, Alpanu, la Némésis étrusque, et l'extispicium du siège de Véies, "Mél. Arch. et Hist." 66, 1954, 39-78. His arguments are mostly visionary. For some criticisms see J. Hubaux, Rome et Véies, pp.128-9.
- (17) That the Veientines should even have known about a rise in the water level of the Alban Lake, which is many miles to the south of Veii, is in itself unlikely. This problem had already occurred to the source of Plutarch (Camill. 3.3): "There was much talk about it in the army that was besieging Veii, so that even the besieged themselves heard of the calamity".
- (18) Livy V.15.3, 16.8-11, etc.; Dion. Hal. XII.10.12; Diod. XIV.93; Val. Max. I.6.3; Plut. Camill. 4.4 f; App. Ital. 8; Zonar, VII.20. Cf. E. Pais, Storia critica di Roma II, p.312 ff; F. Münzer, R.E. s.v. "Furius" (no.44), 328 ff; W. Hoffmann, Rom und die griechischen Welt, "Philologus" Suppbd. 27, 1934, p.129 ff; F. Altheim, Forschungsbericht zur römischen Geschichte, "Welt als Geschichte" II, 1936, p.76 ff; G. De Sanctis, Storia dei Romani², II, p.135 ff; H. W. Parke and D. E. W. Wormell, The Delphic Oracle, I, p.267 ff; Ogilvie, op.cit. p.660 f. on Livy V.15.3.
- (19) E. Pais wrote: "Esso segna uno dei fatti più antichi della storia autentica di Roma e dei reali rapporti di lei con le genti greche" - Storia Critica... II, p.312-3. Cf. G. C. Lewis, Inquiry into the credibility of the Early Roman History, I, p.64; F. Münzer, R.E. s.v. "Furius" (no.44), 328.

- (20) App. Ital. 8. Cf. Parke-Wormell, The Delphic Oracle, I, p.269.
- (21) F. Altheim, art.cit. (n.18), p.76 ff; cf. Parke-Wormell, loc.cit.; Pais, Storia critica II, 313-4.
- (22) Thus G. De Sanctis, Storia dei Romani², II p.137.
- (23) In Livy, Dionysius and Plutarch the "responsa" of the Veientine haruspex and of the Delphic oracle are identical in substance - "suspicious duplicates of the same idea" - and one or the other is clearly superfluous. (Thus Parke, loc.cit.; cf. K. Latte, Rom. Religionsgesch. p.224 n.1; Pais, Storia critica II, 310; Altheim, art.cit. p.76). In Cicero there is no mention of the Delphic oracle in connection with the prodigy; W. Hoffmann inferred from this that the Delphic embassy was not part of the earliest tradition, and rejected the whole account as a late fabrication (Rom und die griech. Welt, p.130 f.). But as Altheim observed, it is only the association with the lacus Albanus that needs to be rejected, and not the story as a whole (Altheim, loc.cit.; cf. Gagé, "Mél. Arch. et Hist." 66, 1954, p.46). The contexts of the two stories were originally separate, as Pais recognised: "due versioni originariamente diverse". If one were to adopt Hoffmann's method, and to argue from the duplication of the two stories that one must be a late fabrication, then it is the tale of the haruspex which must be rejected. This is in fact the conclusion of Parke, loc.cit.
- (24) On this controversial question see now the remarks of E. Gabba, Fondation Hardt Entretiens XIII, 1966, pp.136-7.
- (25) Livy V.19.10, etc. A possible alternative is that the siege of Veii and the construction of the emissarium occurred at roughly the same time (thus De Sanctis, Storia dei Romani² II p.136:... "una approssimativa contemporaneità"). The two explanations can perhaps be combined: "If the Romans really did breach Veii by a cuniculus, and if at the same time a tunnel was being dug at Lake Albano, the successful outcome of the two superficially similar operations would

inevitably be linked in the minds of the superstitions". Thus Ogilvie p.659. Cf. H. H. Scullard, The Etruscan cities and Rome, p.69. The tradition that Veii could only be captured by means of a tunnel may well have given rise to a religious 'action' concerning the outflow of the Alban Lake, a piece of archaic engineering which left a great impression on the Romans of later times - "illa admirabilis a maioribus Albanæ aquæ facta deductio" (Cic. de div. I.100).

- (26) See e.g. G. Wissowa, Religion und Kultus der Römer², p.547; G. De Sanctis, Storia dei Romani IV.2.1 p.361 ff.
- (27) Cf. K. J. Beloch, Röm. Gesch. p.103; P. Fraccaro, "Journ. Rom. Stud." 47, 1957, p.61.
- (28) Vergil, Ecl. VI.13 ff. (Silenus); Homer, Od. IV.351 ff; Verg. Georg. IV.388 ff. (Proteus); Plut. Numa, 15.4 (Picus and Faunus).
- (29) Apollodorus Epit., V.9: " τοὺς ῥυομένους τὴν πόλιν Χρησνοῦς ".
- (30) Sophocles, Phil. 604 ff; 1337 ff; cf. Conon, F. Gr. Hist. 26 F.1,34; Apollodorus, Servius, locc.citt.
- (31) The Helenus parallel was recognised by e.g. Parke, The Delphic Oracle, I p.269, etc. On the Trojan elements see Ogilvie p.628 f. I am not convinced by the arguments of Gagé, Huit recherches p.73 ff., that the Trojan episodes in our sources were part of an original tradition emanating from Veii itself, where the legend of Trojan ancestry was established already in the fifth century B.C. The narrative does not seem to me to imply a precise identification of the Veientes as Trojans, but rather a purely formal assimilation of superficially similar episodes of heroic character, in precisely the same way as the Battle of the Cremera was likened to Thermopylae by Roman historians.
- (32) See Ogilvie 675-6, on V.21.8.
- (33) Cf. J. Hubaux, Rome et Véies, p.148.

- (34) Ogilvie, p.665 on V.17.2.
- (35) Cf. the remarks of J. Bleicken, "Ztschr. d. Savigny-Stiftung f. Rechtsgesch." 78, 1961, p.451.
- (36) Lycophron, Alex. 1245 f; cf. Dion. Hal. I.28.1; Steph. Byz. s.v. "Ταρχωνίδος".
- (37) W. Schur, Griechische Traditionen von der der Gründung Roms, "Klio" 17, 1921, p.137 ff.
- (38) F. Schachermeyr, Etruskische Frühgeschichte, 1929, p.205 ff; id., Telephos und die Etrusker, "Wiener Studien" 47, 1929, p.154 ff. Cf. E. D. Phillips, Odysseus in Italy, "Journ. Hell. Stud." 73, 1953, p.60; M. Sordi, I rapporti romano-ceriti, p.12 n.2.
- (39) F. Gr. Hist. 566 F.62. J. Perret rightly observed that this fragment contradicts Lycophron Alex. 1245-9. (Les origines de la légende troyenne de Rome, p.356 ff; cf. Jacoby, F.Gr.Hist. III B Kommentar p.566). The attempt of J. Geffcken to reconcile the statements of Lycophron and Tertullian (= Timaeus F.62 Jac.) was prejudiced, as Jacoby pointed out, by the unwarranted assumption that Lycophron's source must have been Timaeus. (J. Geffcken, Timaios' Geographie des Westens, p.44 n.1; Jacoby, F.Gr.Hist. 566 F.62 Note 324). The same applies to W. Schur's contention that Lycophron confused several rival accounts which Timaeus had presented separately (Schur, art.cit. p.137 f.). Schachermeyr argued that Timaeus collected local traditions of the West; this is perfectly true as far as the Latins are concerned (F.59 Jac.), and his knowledge of Roman institutions was extensive (F.36, 61 Jac., on which see A. Momigliano, Terzo Contributo p.649 ff.). But his characterisation of the Etruscans (F.1, 50 etc.) appears to be based more on the gossip of Greek authors such as Theopompus (F.Gr.Hist. 115 F.204) and Alcimus (F.Gr.Hist. 560 F.3) than on any information he may have picked up from the Etruscans themselves. (see e.g. T. S. Brown, Timaeus of Tauromenium pp.36-7, 121 n.71; cf. J. Heurgon VQE p.46 ff.). Finally, it is worth repeating the words of Jacoby, who in his commentary on Timaeus rightly insisted that "von seiner Behandlung der Geschichte Etruriens wissen wir

so gut wie nichts". (F.Gr.Hist. III B Kommentar p.565).

- (40) "Wiener Studien" 47, 1929, p.158. Cf. E. Wilken, Die Kunde der Hellenen, 1937, p.180. Schachermeyr seems to regard this as an argument for the Etruscan origin of the tradition: "Das können nur die Etrusker erdacht haben, die Telephidenvariante ist also etruskischer Herkunft..." (Etruskische Frühgeschichte p.206). This interpretation of the alleged political Tendenz is circular because it depends to a great extent on the a priori assumption that the tradition is of Etruscan origin. If it is established that the genealogy was deliberately created for a political purpose (which is by no means certain), one might perhaps be able to divine what that purpose was on the basis of the provenance of the tradition, which must be ascertained independently. Clearly one cannot infer the source or the circumstances of its formation from a supposed tendenz. If a political point of view is expressed in the connection of the mythical origins of Rome with those of the Etruscans, one might reasonably suppose that it implies a situation in which the Romans and the Etruscans were friendly - but this is the exact opposite of what Schachermeyr seems to be arguing.
- (41) See below, Appendix A, p.282 ff.
- (42) Festus s.v. "Caeculus", p.38 L.; Serv. ad Verg. Aen. 7.678; Cato, Origines F.59; Solin. II.9; Verg. Aen. 7.678.
- (43) Cf. Greek Μυτρός (Hesych. s.v.); W. Schulze, Zur Gesch. d. Lat. Eigennamen pp.193-4; F. Marbach, R.E. s.v. "Modius Fabidius", 2329; H. Herter, Mutinus Titinus, "Rh.Mus." 76, 1927, p.418 ff; F. Altheim, Griechische Götter im alten Rom, p.53 f; G. Radke, Die Götter Altitaliens, s.v. "Modius Fabidius", and "Mutunus Tutunus", with bibliography.
- (44) Dion. Hal. IV.2.1 f; Ovid, Fasti VI.627 f; Plin. nat.hist. 36.204; Plut. de Fort. Rom., 10; for further references to the various accounts of Servius Tullius' birth see W. Hoffmann, R.E. s.v. "Tullius" (no.18), 806-7.

- (45) See above, chapter VI note 24 .
- (46) As F. Altheim argued (Griechische Götter im alten Rom p.51 f.) against Wissowa (Religion und Kultus der Römer² p.169); cf. F. Marbach, R.E. s.v. "Tarchetius" 2294-5; L. Euing, Die Sage von Tanaquil, "Frankf. Stud." II, 1933, p.23 f., and the remarks of K. Kerényi, "Gnomon" 1934, pp.134-9, etc.
- (47) F. Schachermeyr, R.E. s.v. "Tarquinius", 2348-9.
- (48) E. Manni, Romulus e parens patriae "Il Mondo Classico" 3, 1933, estr.1, p.3 ff; M. Sordi, I rapporti romano-eterici, pp.16,178 etc.; J. Heurgon, VQE p.312 f; E. Gabba, Tradizione letteraria... Fond.Hardt, Entretiens XIII, 1966, p.148 ff.
- (49) e.g. A. Alföldi, Early Rome and the Latins pp.190,280.
- (50) e.g. L. Pareti, Storia di Roma e del mondo romano, I pp.296-7.
- (51) S. Mazzarino, Antiche leggende sulle origini di Roma, "Studi Romani" 8, 1960, p.389 ff; more fully in Il Pensiero Storico Classico I p.190 ff., cf. II p.64 f.
- (52) F. Gisinger, R.E. s.v. "Promathos", 1285.
- (53) Promathos was a geographical writer, and not, as far as we know, an historian. Cf. A. Momigliano, Terzo Contributo p.56 n.
- (54) For example, the statement (p.195): "(Servio) vedeva in questi greci (i Focei), massimi esploratori dell' occidente, un popolo congeniale alla sua vocazione di rivoluzionario".
- (55) On the question of the date and provenance of the cult, and especially its relation to that of Aricia, see e.g. G. Wissowa, Religion und Kultus², p.247 ff; F. Altheim, Griechische Götter, p.93 ff; id. Der Ursprung der Etrusker p.65 f; A. E. Gordon, in "Trans.Am.Phil.Assoc." 63, 1932, p.177 ff; G. Colonna, in "Parola del Passato"

- 17, 1962, p.57 ff; A. Momigliano, Terzo Contributo, p.641 ff; A. Alföldi, Early Rome and the Latins, p.85 f., and in general K. Latte, Römische Religionsgeschichte, p.169 ff.
- (56) e.g. Justin 43. 3-5; Strabo IV.1.5 p.180 C etc. For a detailed analysis of all the evidence about Rome and Massilia see G. Nenci, Le relazioni con Marsiglia nella politica estera romana, "Rivista di Studi Liguri" 24, 1958.
- (57) Hdt. I.167. See e.g. A. Momigliano, The Origins of the Roman Republic, in "Interpretation: Theory and Practice" (ed. Charles S. Singleton, Baltimore 1969), pp.10-11.
- (58) E. Gabba, Il regno di Servio Tullio: Studi su Dionigi da Alicarnasso II, "Athenaeum" 39, 1961, pp.98 ff., esp. 118 f.
- (59) Liv. I.48, I.60.4; Dion. Hal. IV.40; Plut. Fort.Rom. 10; cf. Tac. Ann. III.28.
- (60) Liv. I.39.5; Dion. Hal. IV.1.2; Zonar VII.9; (Victor) de vir. ill. VII.1.
- (61) Gabba, "Athenaeum" 39 (cit. n.58) p. ; cf. W. Hoffmann, R.E. s.v. "Tullius" (no.18) 819 f.
- (62) Notice especially E. Ruschenbusch, Πρότριος ἱστορία in "Historia" 7, 1958, p.398 ff., who demonstrated that, as far as we know, the historical tradition about Solon began to be exploited by democratic orators for the first time in 355 B.C. (Demosthenes 22,30); it seems likely that this portrayal of Solon as a democrat originated with the publication of a historical work - the Atthis of Cleidemus, published at about the same time (355). See F. Jacoby, Atthis p.75.
- (63) Gabba, "Athenaeum" 39 (cit. n.58) p.119; the fragment of Accius: "Tullius qui libertatem civibus stabiliverat" (Cic. Pro Sest. 123 = Warmington, Remains of Old Latin II p.404, frg.40) is not necessarily the expression of a 'popularis' tendency - Gabba, p.118.
- (64) Pensiero Storico...I p.198. Mazzarino bases

much of his reconstruction of the political events of this period on the assumption that Servius Tullius was identical with the Etruscan Mastarna. He does not seem to realise that this conflicts with the Latin tradition that his mother was Oeresia - although Claudius had been at some pains to point out the discrepancy. Of course, if the equation with Mastarna is accepted as historical, the tradition of Servius Tullius' servile origin must be rejected as a late fiction, designed to explain the name 'Servius'.

- (65) Cf. Heurgon, VQE p.312 f.
- (66) Jacoby, marginal note on F.Gr.Hist. 817; F. Marbach, R.E. s.v. "Tarchetius" 2294; Heurgon, VQE loc.cit.; J. Aust, R.E. s.v. "Caeculus" 1245, etc.
- (67) E. Gabba, Tradizione letteraria... Fond.Hardt. Entretiens XIII, 1966, p.148 f.
- (68) De Magistr. II.12; cf. S. Weinstock, C.Fonteius Capito and the Libri Tagetici, "Papers Brit.Sch. Rome" 18, 1950, p.46.
- (69) Gesammelte Schriften IV.1, p.5 n.1; "Schon dass der Verfasser einer italischen Geschichte darin von den Königen von Alba und den Zwillingen berichtet, schliesst eine frühe Abfassungszeit aus".
- (70) E. Pais Storia di Roma, I, 1 p.189 n.1.
- (71) Serv. (auct.) ad Verg. Aen. 1.273; cf. Ogilvie, Comm. on Liv. pp.44,47, etc.
- (72) Gabba's view has been challenged recently by H. Strasburger, who argues for a date around 300 B.C. (Zur Sage von der Gründung Roms "Sb. Heidelb.Akad.", Phil.-Hist. Klasse, 1968, p.16n.):-"Jedenfalls kann man sich schwer vorstellen, dass nach Timaios, Diokles oder gar Fabius Pictor, dass heisst nach der Entwicklung der für später gultig bleibenden Grundlinien der Romulus-Remus-Sage, irgendein Erzähler noch einmal auf so ausgefallene Züge hatte regredieren können".
The trouble with this statement is that it happens to be untrue. Dionysius of Halicarnassus tells us that the Greeks continued to believe

'false' stories about the origins of Rome even in his own day (I.4.2, cf. I.75.4), and there is plenty of evidence to bear out his view. E. J. Bickermann adduced many examples to show that "although Roman historians, writing in Greek, such as Fabius Pictor and L. Cincius Alimentus, put the Roman saga, combined with the Trojan version, before the Greek reading public, Greek authors either disregarded the Roman account or altered it *ad libitum*". (Origines Gentium, "Class.Phil." 47, 1952, p.67).

- (73) "Die seltsame Erzählung des *Προμαθίων τῆς* in welcher mit der Zwillingenfabel die von der Erzeugung des Servius und von dem Gewebe des Penelope zusammengesponnen sind, ist ohne Zweifel ein spätgriechischer mit Benutzung römischer Annalen geschriebenen Roman" - Mommsen, loc.cit. (n.69).
- (74) Gabba argues (Tradizione letteraria p.148) that there is no contradiction here, because in the legend of the twins Romulus kills his brother, so that in effect only one of the children grows up to become "καλεινότατον". I do not find this convincing; my own view of the text is that the oracle predicts the birth of a single child.
- (75) Mazzarino however suggested that the vulgate tradition presupposes a knowledge of Promathion, rather than the other way round. In this he was re-stating a view which had already been expressed by A. Schwegler, who thought that the phallus-motif was a clue to "die ursprüngliche Idee des Mythos". (Römische Geschichte I, p.430; Mazzarino, "Studi Romani" 8, 1960, p.389; id., Pensiero Storico II, p.64). This 'original idea' gave rise to the later canonical version in which the mother of the twins was presented as a vestal virgin. But the contradiction in Promathion's account suggests that the combination of the two parts of the story was secondary. Equally unlikely, it seems to me, is the view of Mazzarino (Pensiero Storico II, p.66) that Plutarch's source (an antiquarian of the Augustan age, according to Schwartz, R.E. s.v. Diocles (no.47) 797-8) knew of Promathion only through Diocles of Peparethus, who had used him and refuted him.

- (76) Cf. H. Strasburger, art.cit. (n.72), p.16.
- (77) Heurgon, VQE pp.313-4. Tethys was the wife of Neptune; this indicates a place on the coast. Klausen (cited by L. R. Taylor, Local Cults of Etruria p.120 f.) placed the oracle in a shrine of Fortuna at Caere. Heurgon identifies Tethys with Leucothea, the goddess worshipped at Pyrgi. That there was an oracle in Caere is shown by Livy XXI.62. Further discussion and bibliography in L. Euing, Tanaquil p.23 f.; Wissowa, Religion und Kultus², p.110 f.
- (78) See e.g. A. Momigliano, Terzo Contributo p.61.
- (79) M. Guarducci, "Arch.Class." 4, 1952, p.241 f; cf. Heurgon, VQE p.314; id., "Journ.Rom.Stud." 56, 1966, p.3 ff.
- (80) Theopompus (F.Gr.Hist. 115) F.204; Alcimus (F.Gr.Hist. 560) F.3; Timaeus (F.Gr.Hist. 566) F.1. Cf. above, n.39.
- (81) Aristotle Frg. 60 Rose; cf. Cic. ap.Serv. Aen. 8,379 and 485; Val. Max. 9.2 ext.10.
- (82) W. Aly, Strabon von Amaseia, Bd.4, 1957, pp.240-1.
- (83) M. Sordi, I rapporti romano-etruschi... p.32 ff. Cf. above p.226.
- (84) Sordi believes that the polemic is directed not so much against the Romans in general as against the ruling aristocracy, because of the phrase "διὰ τοὺς τότε φαύδως διοικοῦντας τὴν πόλιν". I agree with her (op.cit. p.42 f.) against A. N. Sherwin-White (Roman Citizenship p.51 f.) that the πόλις referred to here is Rome, not Caere. But a third century Roman historian, writing before Fabius Pictor (see Sordi p.46 for the date), seems impossible.
- (85) Sordi, op.cit. pp.25, 35-6; cf. R. Laqueur, R.E. s.v. "Timaios", 1148.
- (86) The manuscripts have "ὕπὸ Κερίων", which was emended by Ed. Meyer to "ὕπὸ Καρίων" ("Rh.Mus." 37, 1882, p.611).
- (87) G. De Sanctis, Storia dei Romani II² p.163

and n.52; M. Sordi, op.cit. p.33.

- (88) See e.g. M. Gelzer, Römische Politik bei Fabius Pictor, "Kleine Schriften" III p.51 ff; K. Hanell, Zur Problematik der älteren römischen Geschichtsschreibung, Fondation Hardt, Entretiens IV, p.149 ff; A. Momigliano, Linee per una valutazione di Fabio Pittore, in Terzo Contributo p.55 f., esp. p.63 f.
- (89) F.Gr.Hist. 184 T.6; H. Fuchs, Der geistige Widerstand gegen Rom, Berlin, 1938, pp.15, 43-4.
- (90) Cf. above, notes 39, 81. NB also Diodorus XV.14.3; Serv. ad Aen. 10.184; cf. Sordi, op.cit. p.44 f.
- (91) Cf. the position of Philinus, who was not himself a Carthaginian, but can hardly be described as an independent Greek historian. Hanell writes, on Philinus F.Gr.Hist. 174 F.1, "Hier führt, meines Erachtens, nicht der erbitterte Bürger von Agrigent das Wort, sondern das Sprachrohr der Karthagischen Regierung" - art.cit. (above, n.88) p.155.
- (92) Polyb., VI.11a.7; Cic. de rep. II.19-20; Liv. I.34.2, IV.3.11; Dion. Hal. III.46.3 f; Strabo, V.2.2 p.219 C, VIII.6.20 p.378 C; Val. Max. III.4.2; Claudius, I.L.S. 212; Plin. N.H. XXXV.16 and 152; Plut. Rom. 16.8, Popl. 14.1; Macrob. Sat. I.6.8, III.4.8, etc.
- (93) A. Blakeway, Demaratus, "Journ.Rom.Stud." 25, 1935, p.129 ff.
- (94) R. M. Ogilvie, Commentary on Livy I-V p.141: "a Roman source is out of the question".
- (95) I was, however, struck by a comment of A. N. Sherwin-White, in his discussion of M. Sordi's theory that Livy's account of the siege of Veii is based on an Etruscan source: "The early dating of the Etruscan historian is essential if he is to be treated on a par with the Greek scraps. But it remains nebulous. Perhaps this material comes down merely through Cato's Origines..." etc. "Journ.Rom.Stud." 51, 1961, p.241.

- (96) M. Sordi, I rapporti romano-etruschi e l'origine della civitas sine suffragio, passim, esp. pp. 10 ff; 177 f; id., Virgilio e la storia romana del IV secolo a.C., "Athenaeum" 42, 1964, p.80 ff. For some criticisms see A. N. Sherwin-White, art.cit. (n.95), p.240 f; J. Bleicken, "Ztschr. d. Savigny-Stiftung f. Rechtsgesch." 78, 1961, p.450 f; R. Gunther, "Deutsche Literaturzeitung", 82, 5, 1961, p.428.

- (97) The doctrine of saecula shows that the Etruscans had a fatalistic view of history; and their idea of what a saeculum was (Censorin. 17.2: "saeculum est spatium vitae humanae longissimum partu et morte definitum") perhaps implies also a sort of biological determinism: the idea that the Etruscan nation was due to last for ten saecula might mean that they interpreted their history as a series of 'lives', or periods divided into phases of infancy, youth, maturity, old age, and so on. This suggests the possibility that a history of the Etruscans, arranged in a "secular" framework, might have presented events of the distant past in such a way that their essential pattern should be seen to be repeated in the events of later times. This would lead to manipulation of old legends in the light of historical experience. But it seems doubtful whether this would have led to a very precise reduplication of particular events, such as Sordi postulates. In any case there is a serious difficulty in Plutarch's statement, (Sulla 7,4): "εἶναι γὰρ ὀκτὼ τὰ σύνπαντα γενή, διαφέροντα τοῖς βίοις καὶ τοῖς ἡθεσιν ἀλλήλων".

- (98) This is an extraordinary notion, but I do not see what else can be meant by the following: "Sembra pertanto di poter concludere che nella fonte originaria la versione mitica e quella storica coesistevano e che ad un ignoto autore etrusco vissuto nella seconda metà del IV secolo va attribuita la doppia redazione delle medesime vicende, tradotte in termini mitici nella leggenda di Enea, narrate in un linguaggio più realistico, anche se impregnato di elementi sacrali, nella storia del conflitto veiente e delle successive lotte con i Latini". Sordi, Rapporti romano-etruschi, p.178.

- (99) Sordi is aware of this difficulty, but her attempt to explain it away (pp.15-16) is patently unconvincing. "La discordanza è tuttavia soltanto relativa", she writes; Vergil's silence on Veii corresponds to Livy's silence on the name of the king. His name was omitted from the original Etruscan account, she suggests, because his memory was condemned by a decree of the Etruscan league.
- (100) For example, she compares Mezentius to the king of Veii because in the Aeneid his allies are the Capenates and Faliscans. But this is not very close to what Vergil really says. The Capenates and Faliscans are led by Messapus (Aen. 7.691 ff.), who has no connection with Mezentius other than that he too fights on the side of the Latins. They are separated in the catalogue in 7.641 ff., and the statement that Messapus is the ally of Mezentius (Sordi, p.15) is thoroughly misleading because there is no special connection between them among the allies of Turnus, and they seem to be entirely independent of one another.
- (101) We are not for the moment concerned with the question of the historicity of this set-up, but rather with the general picture which our sources present.
- (102) See e.g. Liv. I.2; Dion. Hal. I.64.4; Justin, 43.1.13; Varro ap. Plin. N.H. XIV.14.88; Ovid, Fasti, 4, 880 ff; Fasti Praenestini 23rd April (C.I.L. I² p.316). Cato (Origines F.8-12) and Festus (s.v. "Rustica vinalia" p.370 L.) do not state explicitly that Mezentius was the leader of all the Etruscans, but they seem to imply that he was.
- (103) See especially K. Schauenburg, Aeneas und Rom, "Gymnasium" 67, 1960, p.176 ff. The example of J. Perret (Les origines de la légende troyenne de Rome) is a monumental warning to those who attempt to explain the origins of legends as the product of ideological interpretations of historical events. Curiously, the same body of evidence that condemned the thesis of Perret is also decisive in this case.

- (104) For a discussion of this difficulty see A. Schwegler, Römische Geschichte I, p.284 f; H. Peter, HRR I p.cxxxii.
- (105) See the references cited above, n.81, and the remarks of J. Gagé, Huit recherches sur les origines italiques et romaines, p.16 ff.
- (106) J. Gagé, Les Étrusques dans l'Énéide, "Mél.Arch. et Hist." 46, 1929, p.115 ff., esp. p.123 ff.
- (107) Among discussions of this subject see K. O. Müller-W. Deecke, Die Etrusker, I p.320 ff; A. Rosenberg, Der Staat der alten Italiker, 1913, p.51 ff; M. Pallottino, Etruscologia⁶, 1968, p.209 ff; id., Nuovi spunti di ricerca sul tema delle magistrature etrusche, "Studi Etruschi" 24, 1955-6, p.66 ff; J. Heurgon, L'Etat étrusque, "Historia" 6, 1957, p.86 ff; G. Camporeale, Sull'organizzazione statuare degli etruschi, "Parola del Passato" 13, 1958, p.5 ff; R. Lambrechts, Essai sur les magistratures etrusques, 1959, passim, especially p.25 ff., 95 f; L. Banti, Il mondo degli Etruschi, 1960, p.142 ff; H. H. Scullard, The Etruscan cities and Rome, p.231 ff.
- (108) G. Camporeale (art.cit.) denied the existence of any kind of league, religious or political. This seems too extreme: cf. M. Sordi, op.cit. p.19 n.1; W. Eisenhut, R.E. IX A s.v. "Voltumna", 851.
- (109) This must be the implication of Livy IV.25.8; cf. Müller-Deecke, op.cit. I p.330; Scullard, op.cit. p.235: "if the passage is given its natural interpretation".
- (110) CIL XI 5265; see Mommsen, Ges.Schr. VIII, pp.34-5.
- (111) The location of the fanum Voltumnae at Volsinii is based largely on the evidence of CIL XI 5265. L. R. Taylor (Local Cults in Etruria, 1923, p.230-1) is sceptical about this conclusion: cf. Ogilvie, p.571 on Livy IV.23.5. Although one can hardly place any weight on the statement of Valerius Maximus (IX.1 ext.2) that Volsinii was "Etruriae caput", there probably is

a connection between Voltumna and the god Vertumnus, whose cult was centred in Volsinii (Propertius, 4.2.3); Varro calls Vertumnus "deus Etruriae princeps" (L.L.V.46). Cf. W. Eisenhut, R.E. s.v. "Voltumna" 850-851; id., R.E. s.v. "Vertumnus", 1677. P. Ducati, followed by J. Heurgon, maintains that Voltumna was not a goddess but a god, and identical with Vertumnus: Ducati, Etruria antica I, 1927, p.104; Heurgon, Recherches sur...Capone préromaine, p.71 f; id., "Historia" 6, 1957, p.88 n.1; contra, R. Bloch, "Mél.Arch. et Hist." 59, 1947, p.13 n.1.

- (112) Thus e.g. H. Last, Cambridge Ancient History VII p.516; H. H. Scullard, The Etruscan Cities and Rome, p.235 f.
- (113) Liv. IV.24.2, V.17.7.
- (114) L. Pareti, La disunione politica degli Etruschi, "Studi Minori" I p.283 ff; id., I Romani contro gli Etruschi, "Stud.Min." I p.345 ff.
- (115) R. Lambrechts, Essai sur les magistratures... p.26-7; 99 f; M. Pallottino, "Studi Etruschi" 24, 1955-6, p.67-8; A. Alföldi, Early Rome and the Latins p.178-9.
- (116) e.g. Alföldi, loc.cit.
- (117) Strabo V.2.2 p.219 C: "ἐλθὼν δὲ (ὁ Τυρρηνός) τὴν τε χώραν ἀφ' ἑαυτοῦ ἑ Τυρρηνίαν ἐκάλεσε καὶ δώδεκα πόλεις ἐκτίσεν, οὐκιστὴν ἐπιστήσας Τάρκωνα, ἀφ' οὗ Τάρκυνία ἢ πόλις."
- (118) Serv. ad Verg. Aen. 8.475: "'ingentes' autem 'populos' non sine causa dixit; nam Tuscia duodecim lucumones habuit, id est reges, quibus unus praeerat".
- (119) NB the remarks of H. H. Scullard, Etruscan Cities and Rome p.17 f; J. B. Ward-Perkins, "Harvard Studies Class.Phil." 64, 1959, p.1 ff.
- (120) Liv. I.8.3; cf. G. Camporeale, "Parola del Passato" 13, 1958, p.8 f.

- (121) Florus I.1.5; Silius Italicus, Punica 8.483 f. NB Scullard, Etruscan Cities and Rome p.223 - suggesting that Silius Italicus's source might be Cato's Origines.
- (122) "Nobody today questions the statement of our sources that this national federation was governed by lifelong kings" - op.cit. p.178. As a matter of fact our sources state no such thing.
- (123) According to Alföldi (loc.cit.), the later "sacerdos or praetor Etruriae" was "elected annually in rotation from the twelve lucumones". For a comparison of the Etruscan and Ionian leagues see M. Pallottino, "Studi Etruschi" 24, 1955-56, p.67; F. Altheim, Der Ursprung der Etrusker, Baden-Baden 1950, p.61 ff.
- (124) Thus Dion. Hal. III.61.2-3. Cf. R. Lambrechts, Essai sur les magistratures... p.27.
- (125) e.g. T.L.E. nos. 87,137 etc. Discussion in Lambrechts, op.cit. p.95 f., 99 ff.
- (126) Pallottino ("Studi Etruschi" 24, p.67) and Lambrechts (op.cit. p.100 f.) believe that the "zilath mechl rasnal" was a collegiate magistrate, and not the supreme head of the league. Contra, J. Heurgon, "Historia" 6, 1957, p.88 ff.
- (127) e.g. C.I.L. XI.2115; Spart. Vit.Hadr. 19.1.
- (128) Lambrechts, op.cit., p.27 f; Alföldi, op.cit. p.177.
- (129) Tarchon: Verrius Flaccus and Caecina ap. Schol. Veron. ad Verg. Aen. X.200. Ocnus: Verg. Aen. X.198 ff; Serv. ad Aen. X.198, 201, 202; Sil. Ital. Pun. 8.598-9. For a discussion see Müller-Deecke, Die Etrusker, I, p.125 f., II, p.287; A. Grenier, Bologne Villanovienne et Etrusque, p.62 ff; M. Renard, Les origines étrusques de Bologne, "Latomus" i, 1937, 14-24.
- (130) J. Heurgon, Recherches sur...Capoue préromaine, p.62 ff; T. J. Dunbabin, The Western Greeks, Oxford 19 , p.346; B. Combet-Farnoux, "Mél. Arch. et Hist." 69, 1957, p.12; A. Alföldi, op.cit.

p.182 f. (for the settlements in Campania).
 A. Grenier, Bologne Villanovienne et Etrusque,
 p.88 f; G. A. Mansuelli, "C.R. Acad.Inscr."
 1960, p.65 ff; G. A. Mansuelli and R. Scarani,
L'Emilia prima dei Romani 1961, p.245 ff. (for
 the colonisation of the Po valley). For a
 general account of Etruscan expansion in the
 sixth century see M. Pallottino, Etruscologia⁶
 p.139 ff; H. H. Scullard, The Etruscan Cities
and Rome pp.171-220.

- (131) Vulci and Clusium: see Heurgon, Recherches
sur...Capoue préromaine, p.70 ff.
- (132) Plut. Romulus 26: Dion. Hal. II.19, III.61;
 Liv. I.8.2; Lyd. de Mag. I.7.

CHAPTER IX *

(i) The Linen Legion and Campanian notices
in Livy

Under the year 293 B.C. Livy has a curious description of procedures adopted by the Samnites for the formation of an elite force of soldiers known as the linen legion ('legio linteata'):

"It happened that the enemy had made their preparations for the war with the same earnestness and pomp and all the magnificence of splendid arms, and had likewise invoked the assistance of the gods, initiating, as it were, their soldiers in accordance with a certain antique form of oath. But first they held a levy throughout Samnium under this new law, that whosoever of military age did not report in response to the proclamation of the generals, or departed without their orders, should forfeit his life to Jupiter. Which done, they appointed all the army to meet at Aquilonia, where some 40,000 soldiers, the strength of Samnium, came together.

There, at about the middle of the camp, they had enclosed an area, extending approximately two hundred feet in all directions, with wicker hurdles, and roofed it over with linen. In this place they offered sacrifice

(*) For notes to Chapter IX, see below pp. 337-342.

in accordance with directions read from an old linen roll. The celebrant was one Ovius Paccius, an aged man, who claimed to derive this ceremony from an ancient ritual of the Samnites which the forefathers of those present had formerly employed when they had gone secretly about to get Capua away from the Etruscans. On the conclusion of the sacrifice, the general by his apparitor commanded to be summoned all those of the highest degree in birth and deeds of arms; and one by one they were introduced. Besides other ceremonial preparations, such as might avail to strike the mind with religious awe, there was a place all enclosed, with altars in the midst, and slaughtered victims lying about, and round them a guard of centurions with drawn swords. The man was brought up to the altar, more like a victim than a partaker in the rite, and was sworn not to divulge what he should there see or hear. They then compelled him to take an oath in accordance with a certain dreadful form of words, whereby he invoked a curse upon his head, his household, and his family, if he went not into battle where his generals led the way, or if he either fled from the line himself or saw any other fleeing and did not instantly cut him down. Some there were at first who refused to take this oath; these were beheaded before the altars, where they lay amongst the slaughtered victims - a warning

to the rest not to refuse. When the leading Samnites had been bound by this imprecation, the general named ten of them and bade them choose every man another, and so to proceed until they had brought their number up to sixteen thousand. These were named the "Linen Legion", from the roof of the enclosure wherein the nobles had been sworn, and were given splendid arms and crested helmets, to distinguish them from the rest" (Livy, 10.38. 2-12).

This account is confused and contains several absurdities, one of which is the derivation of the name 'Linen Legion' from the covering of the place of sacrifice. Of course this is not the real explanation. The linen legion itself is historical, and owes its name to the simple fact that the soldiers wore linen tunics. Livy himself indicates this in another place (p.40.3), and it is confirmed by Oscan wall paintings from Campania.⁽¹⁾ The derivation "ab integumento consaepti" is to be connected with the curious notion that the ritual was carried out in secret. Another puzzling feature is the fact that the youths are forced to swear two oaths, one binding them to secrecy, the other compelling obedience. The second oath makes sense in the context of a "lex sacrata",⁽²⁾ enacted for the purpose of forming an elite force of soldiers. But the function of the first oath in Livy's account is obscure. It is not clear exactly what was

supposed to be concealed; the phrase "quae visa auditaque in eo loco essent" is rather vague and in any case fails to explain why the oath of secrecy was necessary at all.⁽³⁾

The best explanation of these difficulties is the hypothesis of F. Altheim,⁽⁴⁾ that Livy's source took the historical fact of a linen legion, recruited by a "lex sacrata", and combined it with extraneous elements taken from an independent account of a totally different set of events. The true context for the narrative of the secret ritual is hinted at by Livy himself. The linen legion was selected, we are told, in accordance with a ceremony "ex vetusta Samnitium religione qua quondam usi maiores eorum fuissent cum adimendae Etruscis Capuae clandestinum cepissent consilium" (10.38.6). According to Altheim, here following a suggestion of K. Latte,⁽⁵⁾ details from an account of the Oscan coup d'état at Capua have been incorporated into the narrative of the Samnites' military preparations at Aquilonia in 293 B.C. The oath of secrecy is incomprehensible in the account of the formation of the "legio linteata", but would be perfectly appropriate in the context of a "clandestinum consilium".⁽⁶⁾

The capture of Capua by Oscan-speaking invaders at the end of the fifth century B.C. had no direct connection with the history of Rome, and it follows that any account of these events must ultimately go back to a tradition

that was not Roman. The account of the ritual in Livy 10.38. 2-12 is therefore a good illustration of the way in which the Roman annalists exploited information taken from independent traditions and incorporated it into the story of their own national past. As K. Latte remarks: "Es ist nur natürlich dass die Annalisten aus solchen Quellen die Farben entlehnen, mit denen sie die alte knappe Stadtchronik ausschmückten".⁽⁷⁾

Details of the *coniuratio* at Capua were applied to the account of the selection of the *legio linteata* presumably because of some similarity in the procedures followed on each occasion. Perhaps the explanation of this is that an Oscan tradition had portrayed the conspirators at Capua, like the men of the linen legion, as "*milites sacрати*".⁽⁸⁾

At first sight it is perhaps surprising that the Roman annalists should have consulted and borrowed from an account of Campanian events which had nothing to do with the history of Rome. But it is an undoubted fact that information relating to the independent history of Campania was known to the annalists. This emerges from a study of the so-called 'Campanian notices' in Livy's fourth book. Under the year 423 B.C., Livy records the capture of Capua by the Samnites (IV.37.1). He was clearly somewhat embarrassed by the 'irrelevance' of this

entry - "peregrina res, sed memoria digna eo anno facta". The capture of Cumae three years later is inserted into the narrative of annual events in much the same way (IV.44.12).

These Campanian notices must derive ultimately from the same source as many of the details of the ritual selection of the linen legion.⁽⁹⁾ This latter account shares in common with the section on the seizure of Capua a polemical tone, and both seem to imply a source which was hostile to the Samnites. Moreover, Livy's statement in IV.37.1 - "festo die gravis somno epulisque incolas veteres novi coloni nocturna caede adorti" - with its emphasis on the unexpected and treacherous nature of the attack, together with the fact that it occurred at night, suggests a secret "coniuratio" and is entirely compatible with the description of the ritual and the oath which has been transposed into the context of the Battle of Aquilonia.

These various reports suggest a source which gave a detailed account of the history of Campania, and particularly of Capua. This assumption is confirmed by the passage of Livy (IV.37) in which we find, in the form of an annalistic entry, an account which covers the events of several years. We are told that first ("prius") the Etruscans of Capua admitted the Samnites "in societatem urbis agrorumque", and that subsequently ("deinde") these

new settlers ("novi coloni") occupied the city by rising up against the old inhabitants ("incolae veteres"). The situation thus developed in two distinct stages, the first of which clearly coincides with Diodorus' report, under the year 437 B.C., of the formation of the Campanian nation: " τὸ ἔθνος τῶν Καμπανῶν συνέστη " (Diod. XII.31.1).

The chronological discrepancy between Diodorus and Livy is probably to be explained by the fact that Livy has reported two separate events under one year, whereas in fact they may belong to two separate years. The formation of the Campanian nation, mentioned by Diodorus, is without doubt a reference to the arrangement whereby the new settlers were absorbed "in societatem urbis agrorumque" (10). This interpretation is borne out by a consideration of the etymologies of 'Capua' and 'Campani'.

Livy gives two rival explanations of how Capua got its name: "Capuam ab duce eorum (sc. Samnitium) Capye vel, quod propius vero est, a campestri agro appellatam". The second of these etymologies is also found in Diodorus: " τὸ ἔθνος τῶν Καμπανῶν συνέστη, καὶ ταύτης ἔτυχε τῆς προσηγορίας ἀπὸ τῆς ἀρετῆς τοῦ πλησίον κειμένου πεδίου ". It appears, however, that Livy has registered an etymological conflict where in fact there is none. Common sense indicates, and Diodorus confirms, that the derivation "a

campestri agro" refers not to the city of Capua but to the people, the Campani.⁽¹¹⁾ This confusion is a further sign that two separate developments - the origin of the Campani and the occupation of Capua - are conflated, and to some extent confused, in the account of Livy.

There seems little doubt that the report in Diodorus XII.31.1 goes back to the same source as the Campanian notices of Livy. The derivation of the name Campania "a campestri agro" is common to both; and it is hardly likely that the same isolated notice should have found its way independently into both Diodorus and the Roman annalistic tradition. In other words, the report about "τὸ ἔθνος τῶν Καντανῶν" must have come from the annalistic sources of Diodorus.⁽¹²⁾

Of course, these items must ultimately go back to non-Roman sources; originally they will have been part of a local Campanian tradition, which probably also influenced Greek historians. The question is, how did these notices get into the Roman tradition?

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(ii) Cato's Origines and the supposed "Chronicle of Cumae"

The evidence of Livy and Diodorus shows that we are dealing with a coherent account of events in Campania, recording the struggle between the Samnites and the

Etruscans of Capua (or Voltturnum, as it was then known); the absorption of the former into the citizen body and their final capture of the city in 423 were described in some detail, to judge by Livy X.38, and the occupation of Cumae by the Campanians in 420 was also included.

It is unlikely that references to these events were based on direct consultation of Campanian sources by the annalists, in view of their policy of excluding all external material unless it had some immediate bearing on the doings of the Romans.⁽¹³⁾ The most likely explanation of the presence of these notices in the annals is that they come from the work of a writer who had investigated the history of Capua as a subject worthy of record in its own right - and had integrated the independent history of the Italian communities with that of Rome. This in itself surely points in the direction of Cato.⁽¹⁴⁾ This possibility is made all the stronger by the fact that in both Livy and Diodorus the name 'Campani' is said to derive "a campestri agro". This must indicate a source written in Latin. We are reminded also of Cato's predilection for etymologies.

We know that Cato gave an account of the origins of Capua in his Origines. A familiar passage of Velleius records that he dated its foundation by the Etruscans to the year 471 B.C., and I see no reason in this particular

instance to question either the reliability of Velleius or the sanity of Cato (on Cato's date for the founding of Capua see below pp. 331-336).

There is nothing unlikely in the suggestion that the Origines contained an account of the 'coniuratio' which resulted in the seizure of Capua. The account of the Samnite ritual in Livy X.38 implies a source interested in sacred institutions, and we know that Cato made a study of local laws and customs (F.33,61,94, etc.). That he should have recorded the circumstances that led to the Samnite coup d'état ought not to surprise us.

Obviously Cato himself cannot have been responsible for the transposition of the ritual into the context of the battle of Aquilonia. For him the independent history of Campania was worth studying for its own sake. But the later Roman annalists ignored Cato's message and confined themselves exclusively to the history of their own city in its narrowest sense, and it is they who were responsible for filling out the meagre records of the pontifices with originally unrelated elements from independent traditions.

The character of the original Campanian source(s) from which Cato drew his information cannot be ascertained with any certainty. Many have thought that the Campanian notices go back to a Greek account: Altheim, for example, suggested a local chronicle of Cumae, and connected the

Campanian notices with reports about the life of the Cumaean tyrant Aristodemus Malacus.⁽¹⁵⁾ These reports require further discussion.

A full and detailed biography of Aristodemus Malacus has been preserved in the seventh book of Dionysius of Halicarnassus (VII.3-11) - an account which must go back to an independent Greek source.⁽¹⁶⁾ It is to be noted also that information about Aristodemus of Cumae has somehow intruded into the traditional Roman account of events following the expulsion of Tarquinius Superbus, who is said to have taken refuge with Aristodemus.⁽¹⁷⁾ Eduard Meyer and others believed that the source of all these accounts was the "Κυρναϊκά" attributed to one Hyperochus (F.Gr.Hist. 576), of which a few fragments survive.⁽¹⁸⁾ But F. Jacoby pointed out that the "Κυρναϊκά" could not be the original source of the annalistic notices, because the fragments show that Hyperochus was himself influenced by the Roman annalists, and that the work ascribed to him was a late compilation.⁽¹⁹⁾

In a recent discussion of this material A. Alföldi distinguished two stages in the development of the tradition about Aristodemus of Cumae.⁽²⁰⁾ The various annalistic references in Livy and Dionysius (mentioned above, and n.18), together with the three fragments of Hyperochus, represent a late and contaminated version

of an original Cumaean account. The story of Aristodemus's mistress Xenocrite, related in Plutarch (Mul.virt.26,p. 261E-262D), must also derive from this secondary level of tradition, because in this romance Aristodemus's relief expedition to Aricia (Dion. Hal. VII.5) has been transformed into an attempt to aid the Romans against the Tarquins and Porsenna.⁽²¹⁾ But Alföldi argues, rightly in my opinion, that the detailed biography of the tyrant in Dionysius of Halicarnassus (VII.3-11) is independent of the annalistic notices which immediately precede and follow it;⁽²²⁾ it is a digression which Dionysius has interpolated into his main narrative,⁽²³⁾ and as such it is based directly on an original Greek source.

The source of Dionysius' life of Aristodemus is now widely believed to have been Timaeus.⁽²⁴⁾ This possibility was suggested long ago by Niebuhr (loc.cit. n.16), and argued at length by F. Reuss,⁽²⁵⁾ on the crude but plausible grounds that a polemical and highly-coloured presentation of "the despot's progress" is indicative of Timaeus. Alföldi himself favoured this conjecture, although he seems to have been misunderstood here;⁽²⁶⁾ his suggestion of a local Cumaean chronicle was an attempt to identify not the immediate source of Dionysius, but the source of the Hellenistic author (probably Timaeus) followed by

Dionysius. The character of the source on which this Hellenistic version was based is, however, entirely a matter for speculation. Alföldi's theory of an old local chronicle of Cumae is no more than a theoretical possibility. The important point is that the substance of Dionysius' narrative must derive ultimately from indigenous Cumaean sources of some kind.

Elements of the same Cumaean tradition will have found their way into the Roman annals either through Cato's Origines or some other early Roman writer. Alföldi proposes Fabius Pictor.⁽²⁷⁾ It is not entirely surprising in this case that Cumaean material should have been exploited by the Roman annalists, because of the obvious relevance of the Aristodemus story to the history of the early Roman republic. The traditions about Aristodemus' reign provided an independent account of events in Latium at the end of the sixth century B.C., and confirmed the dating of the expulsion of Tarquin, whose flight to Cumae was probably based on an old Roman tradition.⁽²⁸⁾

It cannot be certain whether or not the other Campanian notices (about the Samnite capture of Capua, etc.) were also transmitted by way of a Greek literary tradition, as Altheim suggested.⁽²⁹⁾ In any case this question does not affect the contention that these fragments of Campanian history first entered the Roman tradition through the

Origines of Cato; the question is whether Cato's work had been done for him by some Greek writer such as Timaeus, or whether his account of the early history of Capua and other Campanian cities was based on independent research. No definite answer to this question is possible; but the description of the Samnite conspiracy at Capua in Livy IV.37.1 and X.38 does not reflect a purely Greek point of view; as we have seen (above, p.322 and n.8) it has undoubtedly been influenced to a certain extent by a native Oscan tradition. This suggests a synthesis of indigenous material by Cato himself.

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(iii) Appendix: Cato's date for the foundation of Capua (F.69)

Cato is alleged to have written that Capua was founded by the Etruscans 260 years before its capture by the Romans in the Second Punic War. This chronology was criticised by Velleius Paterculus, the source of the fragment, and has been almost unanimously rejected ever since.⁽³⁰⁾ K. J. Beloch, for example, found it impossible to believe that Capua should have been founded in 471 B.C., and described Cato's reported statement as an "absurdity" - an opinion echoed in many subsequent works.⁽³¹⁾ But Cato was not in the habit of writing absurdities, and is regarded as a reliable authority in matters of this kind.

It is therefore assumed that he cannot have expressed the view attributed to him by Velleius, and that the latter must have misquoted him.

There is disagreement on what the text of the Origines really said. According to Beloch,⁽³²⁾ Cato dated the foundation to c.600 B.C., by counting back 260 years from the alleged "deditio" of Capua in 343 B.C. (Livy, VII.31.4), or from 338 B.C., when the city became part of the Roman state by the "civitas sine suffragio".⁽³³⁾ Others believe that Cato had referred not to the capture of Capua by the Romans in 211, but to its seizure by the Samnites in 423: the foundation date would then work out at 683 B.C.⁽³⁴⁾

But such reconstructions involve a rather cavalier treatment of a text which is perfectly intelligible and coherent as it stands.⁽³⁵⁾ Moreover, it is plain that Velleius fully appreciated the implications of what he was saying: "quod si ita est, cum sint a Capua capta anni ducenti et quadraginta, ut condita est, anni sunt fere quingenti". He expressed surprise at this late dating, and professed himself unable to accept it. This awareness of the point at issue must surely make it unlikely that Velleius misquoted or misunderstood his source.

It is obviously important to remember that Cato's date for the founding of Capua might not be the true

one;⁽³⁶⁾ a prima facie case against Velleius' accuracy and powers of comprehension can only be made if it can be shown not only that an early fifth century date is absurd, but also that it would have seemed absurd to Cato. But both these propositions are at least open to debate.

Two main arguments have been advanced against a foundation date of 471 B.C. The first is essentially the same as that offered by Velleius, who could not believe that so great a city as Capua should have grown up, flourished, fallen, and risen again in such a short space of time. To this one need only say that 500 years is quite long enough; Velleius' criticism is a piece of empty rhetoric, and need not detain us. But Alföldi interpreted Velleius' words in a rather different way: "As the Samnites wiped out the old governing class of the city as early as 423 B.C., or possibly even in 437 B.C., the Roman writer could not believe that a city of such magnitude could develop and reach her size in only 34, or 48 years respectively". It does not matter that this is an impossible interpretation of the text; it deserves attention as an argument in its own right.⁽³⁷⁾ But it is hardly a decisive objection. For one thing we may note that it would surely have been perfectly possible in antiquity for a new foundation to become a flourishing city in a very short time.⁽³⁸⁾ But in the case of Capua it is

clear that a town already existed on the site when the Etruscans arrived. This emerges from a consideration of the archaeological facts. Recent excavations have furnished evidence of continuous settlement at Capua from a very remote period - at least as far back as the middle of the eighth century B.C.⁽³⁹⁾ On anybody's view this is too early for the Etruscan settlement in Campania, and it follows that the Etruscans must have taken over an existing community on the site of Capua at some subsequent date. The gradual Etruscanisation of the city, beginning with the importation of Etruscan bucchero pottery, goes back to the seventh century B.C.⁽⁴⁰⁾ This might be evidence of "the presence of Etruscans" in Capua already at this date;⁽⁴¹⁾ but while the discovery of Etruscan artefacts can indicate the effects of Etruscan influence and trade, and possibly the presence of Etruscan individuals, it is notoriously difficult to establish political facts from this kind of evidence. The recent finds from the necropolis at Capua do not prove that the city came under the direct political control of the Etruscans at an early date; they have shown only that a high date would not be impossible, as was once thought.⁽⁴²⁾ It is still true that the date of the Etruscan foundation of Capua can only be determined on the basis of general historical considerations.

This brings us to the second main argument that has been levelled against Cato's date. It is that by 471 B.C. Etruscan military power had deteriorated to such an extent that any new enterprise in Campania would have been unthinkable.⁽⁴³⁾ At the end of the sixth century, it is argued, the Etruscans lost control of Latium; the Tarquins were expelled from Rome in 509, and in 504 Lars Porsenna's army under his son Arruns was defeated at Aricia by the Latins, aided by Aristodemus of Cumae. We also learn that the Etruscans were twice defeated in Campania itself. In 524 B.C. (according to an account which we have traced back via Timaeus to native Cumaean sources) they were driven back from Cumae by the Greeks; then, in 474, an Etruscan fleet was destroyed off Cumae by Hieron of Syracuse.

Scholars have pointed out, first, that the Etruscans are unlikely to have begun their occupation of Campania after this series of reverses, and secondly that the two battles which took place at Cumae are best explained on the assumption that the Etruscans were already established in Campania.

These are strong arguments, and it must be admitted as unlikely that the Etruscan colonisation of Campania took place in the second quarter of the fifth century B.C. The most likely historical context for the enterprise is

undoubtedly the sixth century, when Etruscan military and naval power was at its height.⁽⁴⁴⁾ But the dating of the Etruscan venture into Campania does not necessarily fix the date of the founding of Capua.

It is often assumed that the Etruscans advanced into Campania by land. If so, it is a reasonable conjecture that they settled at Capua first, and used it as a base for their later operations and settlements further south. But there is no evidence of any kind to confirm this, and it is equally possible, as Pallottino has suggested,⁽⁴⁵⁾ that the Etruscan colonies arrived by sea. This would suggest that they first occupied the coastal sites, and only subsequently advanced inland to Capua and Nola. Such a movement might have occurred as a result of the decline in Etruscan sea power. If so, the significance of the fact that Cato dated the founding of Capua three years after the Battle of Cumae becomes apparent: the occupation of the inland sites might have been a direct consequence of the defeat of the Etruscan fleet in 474.

The facts, however, are not recoverable in the present state of our knowledge, and further speculation would be futile. The important point is that an early fifth century date for the foundation of Capua by the Etruscans is not out of the question. At any rate we should certainly not be justified in rejecting Velleius' account of what Cato said.

(iv) Notes to Chapter IX

- (1) See F. Weege, "Jahrb. d. deutschen arch. Inst." 24, 1909, p.136 ff.; C. Nicolet, "Mél. Arch. et Hist." 74, 1962, p.505; E. T. Salmon, Samnium and the Samnites, Cambridge 1967, p.105; M. W. Frederiksen, Campanian Cavalry: a question of origins, "Dialoghi di Archedologia" 2, 1968, p.3 ff., esp. p.5.
- (2) The term "lex sacrata" does not in fact occur in Livy's description of the linen legion, but many of the essential features of this old Sabellian practice are recognisable - for example the method of enrolment ("vir virum legere"); cf. Liv. IX.39.5 - and see the remarks of F. Altheim, Lex sacrata: die Anfänge der plebeischen Organisation, "Albae Vigilae", Amsterdam, I, 1940 (cited hereafter as: Altheim, Lex sacrata), p.11 ff., esp. pp.17-18.
- (3) F. Altheim, Historiae Cumanae Compositor, in Untersuchungen zur römischen Geschichte I, 1961 (cited hereafter as : Altheim, Untersuchungen), p.201:- "Was sollte geheimgehalten werden? Der Text des liber linteus, die Opferung, oder die gezückten Schwerter der centuriones? Und warum?".
- (4) Altheim, Untersuchungen, pp.201-2.
- (5) K. Latte, Zwei Exkurse zur römischen ^{Staatsrecht} Geschichte, "Gott.Gel.Nachr." 1, 1934-6, p.69 f.
- (6) Cf. Altheim, Untersuchungen, p.201.
- (7) Latte, art.cit. (n.5) p.69n.
- (8) The seizure of Capua in 423 B.C. was an important stage in the Sabellian occupation of Campania, and took place shortly after the formation of the 'Campanian nation', an event recorded by Diodorus (XII.31.1). It is possible that the Campanians, like other Sabellian peoples, may have preserved a tradition about their own origins in which their migration was viewed in religious terms, and interpreted as the result of a "ver sacrum". It is to be noted that the occupation of Messana in 288 by former Campanian mercenaries of Agathocles was

presented by hostile Greek sources in exactly the same way as the "nocturna caedes" of Capua; but a more favourable version was given by the Oscan poet Alfius Varus (a contemporary of Verrius Flaccus), who connected the enterprise with a "ver sacrum" and portrayed the Mamertini as bound by an oath to Mars (Osc.: "Mamers"). Fest. s.v. "Mamertini" p.276 L.; cf. J. Heurgon, Trois études sur le "ver sacrum", p.25 f; E. T. Salmon, Samnium and the Samnites p.39 n.1. On Alfius see C. Cichorius, Römische Studien p.59 ff; in general see Heurgon, Recherches sur... Capoue préromaine pp.93-4.

- (9) This is the conclusion of F. Altheim, Lex sacrata p.13 ff; id., Untersuchungen p.202 ff.
- (10) This is more satisfactory than the view of Heurgon (Recherches sur... Capoue préromaine, p.88), who believed that Diodorus and Livy were not referring to the same event. For criticism of this interpretation see N. K. Rutter, Campanian chronology in the fifth century B.C., "Class. Quart." N.S.21, 1971, p.58 f. Rutter's view had already been expressed by Altheim (Untersuchungen p.204 f.), whom I have followed in the main text. (Cf. now R. M. Ogilvie, Comm. on Livy 1-5, rev. ed. 1970, p.784, additional note on p.580l.38.).
- (11) Altheim, Untersuchungen p.204. The complex evidence for the derivation of the names Capua, Campani, etc., is analysed by Heurgon, Recherches sur... Capoue préromaine, p.136 ff.
- (12) It is unlikely that Diodorus himself extracted this item from an annalistic source; rather, he will have found it in the so-called chronographic source - i.e. the ~~same~~ source which formed the chronological framework of Diodorus' work, and in which the Greek and Roman eponym-lists (and other chronological data) were synchronised (see K. J. Beloch, Römische Geschichte, p.107 ff.). There is no doubt that the reference to the Campanians in Diodorus XII.31.1 comes via the chronographic source, because it is sandwiched between a mention of the Roman consuls and a chronological reference to the kings of the Cimmerian Bosphorus. Cf. N. K. Rutter, "Class. Quart." N.S.21, 1971, p.59.

- (13) See e.g. F. Münzer, Adelsparteien und Adelsfamilien, p.46; cf. A. Alföldi, Early Rome and the Latins, p.122; A. J. Toynbee, Hannibal's Legacy I p.285-6; E. T. Salmon, Samnium and the Samnites, p.2 ff., etc.
- (14) The idea that Cato might be the source of these notices was first put into my head by an incidental remark of R. M. Ogilvie: "Originally they will have come either from a work like Cato's Origines or from a Greek historian from the west". (Commentary on Livy 1-5, p.581 on 4.29.8). The same suggestion is also made by Rutter, "Class. Quart." N.S.21, 1971, p.58.
- (15) Altheim, Lex sacrata pp.14-15; id., Untersuchungen, pp.206-7; id., "Welt als Geschichte" 2, 1936, p.76.
- (16) See B. G. Niebuhr, History of Rome (Trans. Thirlwall), I, p.553 and n.1224; A. Schwegler, Römische Geschichte, II, p.193; K. O. Müller-W. Deecke, Die Etrusker I, p.147; Ed. Meyer, Geschichte des Altertums, 2nd ed., Stuttgart 1937, III, p.750 n.1; W. von Christ, Griechische Nachrichten über Italien, "Sb. Bayr. Akad.", 1905, p.62 ff; B. Combet-Farnoux, Cumes, L'Etrurie et Rome à la fin du VIe siècle, "Mél. Arch. et Hist." 69, 1957, p.29 f; R. Werner, Der Beginn der römischen Republik, Munich 1963, p.386; A. Alföldi, Early Rome and the Latins, p.63 ff.
- (17) Livy II.14. 5-9; 21.5; 34.3-5; Dion. Hal. V.36. 1-4; VI.21.3; VII.1.3; 12.1-3.
- (18) Ed. Meyer, loc.cit. (n.16); Altheim, works cited in n.15 and Epochen der römischen Geschichte I, p.102 and n.
- (19) F.Gr.Hist. III B, Kommentar pp.606-8, Noten 352-3; Alföldi, Early Rome and the Latins p.57 ff; cf. G. De Sanctis, Storia dei Romani I² p.438 n.79.
- (20) Alföldi, op.cit., pp.56-72, followed by E. Gabba, Considerazioni sulla tradizione letteraria sulle origini della Repubblica, Fondation Hardt, Entretiens XIII, pp.144-7, and E. Gjerstad, Porsenna and Rome, "Opuscula Romana" 7, 1967-9, pp.149-161, esp. p.159 f.

- (21) Alföldi, op.cit. p.58 f.
- (22) Alföldi, op.cit. p.62 f., here following W.von Christ, art.cit. (n.16) p.62 f.
- (23) This is confirmed by the apology with which Dionysius introduces the account of Aristodemus (VII.2.5); "ἀφορμαῖς δὲ τῆς τυραννίδος ὁποῖαις ἐχρήσατο καὶ τινὰς ἦλθεν ἐπ' αὐτὴν ὁδοὺς καὶ πῶς διώκησε τὰ κατὰ τὴν ἀρχὴν καταστροφῆς τε ὁποῖαις ἔτυχεν οὐκ ἄκαιρον εἶναι δοκῶ μικρὸν ἐπιστῆσαι τὴν φωναϊκὴν διήγησιν κεφαλαιωδῶς διεξελθεῖν."
- Cf. Christ, art.cit. p.63; G. De Sanctis, Storia dei Romani I², p.438 n.79.
- (24) U. Cozzoli, Aristodemo malaco, in Miscellanea greca e romana, Rome 1965, pp.5-29; M. W. Frederiksen, "Dialoghi di Archeologia" 2, 1968, p.29 n.59. Cf. Ed. Meyer, loc.cit. (n.16); J. Heurgon, Recherches sur... Capoue préromaine p.64. Contra, W. Christ, art.cit. pp.70-71.
- (25) F. Reuss, Timaios bei Plutarch, Diodor und Dionys von Halicarnass, "Philologus" 45, 1886, p.245 ff., esp. pp.271-77.
- (26) Alföldi, op.cit., p.68:- "...possibly extracted by Dionysius from Timaeus". Frederiksen, loc.cit. (n.24), wrongly attributed to Alföldi the view that Dionysius himself used a local chronicle of Cumae.
- (27) Alföldi, op.cit. p.70 ff.: "...It is obvious that the Greek helpers of Pictor put at his disposal the unique data of the Cymaeian chronicle - in the adaptation we have just quoted or another..." (p.71).
- (28) Livy II.21.5; Cic. Tusc. III.12.27; Dion. Hal. VI.21.3; (Victor) de vir. ill. 8,6. The authenticity of this tradition is upheld by W.von Christ, art.cit. pp.61-2; but note the contrary view of G. De Sanctis, Storia dei Romani I² p.438, n.78; F. Schachermeyr, R.E. IV A s.v. "Tarquinius" (no.7), 2389.
- (29) See above, n.15. Gabba is sceptical about

this conclusion: see Tradizione letteraria... p.145, and in "Miscellanea Rostagni" p.192 and n.24.

- (30) One notable exception was Grote - History of Greece (1869 edn.) III p.356. More recently, only Pallottino has upheld the late date (see below nn.38,41, and 45).
- (31) K. J. Beloch, Campanien, Berlin 1879, p.8 f. Cf. A. Alföldi, Early Rome and the Latins, p.184, citing L. Pareti, La Tomba Regolini-Galassi, 1947, p.498. W. Johannowsky has suggested that Cato deliberately lowered the date of the foundation of Capua because of a feeling of animosity towards the city engendered by a recollection of the events of 214-11 B.C. - "Studi Etruschi" 33, 1965, p.686 n.15.
- (32) Beloch, Campanien, loc.cit. (but Beloch seems to have changed his mind when he wrote the Griechische Geschichte: see n.34 below). Cf the bibliography cited in A. Alföldi, op.cit. p.183 n.5; older works listed by H. Peter, HRR I², p.75, note on Cato, Origines F.69.
- (33) See e.g. M. Sordi, I rapporti romano-eritri p.118 f; A. J. Toynbee, Hannibal's Legacy I, p.400 ff., with bibliog.
- (34) Beloch, Griechische Geschichte I², p.245 n.1; L. Pareti, La Tomba Regolini-Galassi p.498 ff; id., Storia di Roma I, p.150.
- (35) Cf. G. De Sanctis, Storia dei Romani I², p.432 n.61; J. Heurgon, Recherches sur...Capoue préromaine, p.63 n.1.
- (36) The method of dating - stetisse autem Capuam, antequam a Romanis caperetur, annis circiter ducentis et sexaginta - clearly goes back to Cato himself, because Velleius had to make further calculations in order to translate this information into terms that meant something to himself and his readers - quod si ita est, cum sint a Capua capta anni ducenti et quadraginta, ut condita est, anni sunt fere quingenti. The fact that Cato worked out the date by counting back from the capture of the city indicates that he was probably using information

available in Capua itself. Rosenberg suggested a local chronicle of the city (Einleitung und Quellenkunde zur röm. Gesch. p.164), while De Sanctis, who had a very good understanding of Cato's methods, argued that Cato's date was an error arising from a mistaken interpretation of documentary evidence: "Piuttosto è da credere che circa il 470 cominciassero la liste degli eponimi di Capua e che da queste abbia dedotto a torto Catone la data della fondazione della città". (Storia dei Romani I² p.432 n.61).

- (37) A. Alföldi, op.cit. p.184; cf. Beloch, Campanien p.8-9; Heurgon, Recherches sur...Capoue préromaine p.63; L. Pareti, locc.citt. (n.34); H. H. Scullard, The Etruscan Cities and Rome, p.191.
- (38) Cf. M. Pallottino, Il filoetruschismo di Aristodemo e la data della fondazione di Capua, "Parola del Passato" 11, 1956, p.86.
- (39) W. Johannowsky, Gli Etruschi in Campania, "Klearchos" 5, 1963, p.62 ff; cf. id., "Studi Etruschi" 33, 1965, p.685 ff; id., in A. Alföldi, Early Rome and the Latins, p.420 ff; Alföldi, op.cit. p.185 ff; H. H. Scullard, op.cit. p.191 f.
- (40) Johannowsky, opp.citt. (n.39) and in Greci e Italici in Magna Grecia, "Atti d. pr. convegno d. studi sulla Magna Grecia", 1962, p.248; cf. D. Mustilli, ibid. p.183.
- (41) Alföldi, op.cit. p.185.
- (42) See Johannowsky, in "Klearchos" 5, 1963, p.62 f., for an account of the way the excavations have altered our knowledge of the Etruscans in Campania.
- (43) See works quoted in n.37.
- (44) J. Heurgon, Recherches sur...Capoue préromaine p.63.
- (45) M. Pallottino, "Parola del Passato" 11, 1956, pp.85-6, and 86 n.1; id., "Arch.Class." 12, 1960, p.120. Cf. Scullard, op.cit. pp.190-1.

CHAPTER X *

(i) Cato and the origin of the Sabines

In an earlier chapter we examined a passage of Dionysius of Halicarnassus containing a summary of Cato's account of the Sabines.⁽¹⁾ As was pointed out, this is the most substantial fragment to survive from the second and third books of the Origines, and as such it can be used to illustrate the character of the material contained in those books and Cato's method of handling it. But the fragment is also worth studying in its own right, for the light it throws on the development of the tradition about the origin of the Sabines. A discussion of Cato's treatment of this matter will, I hope, provide a fitting conclusion to the thesis, because the fragment so clearly exemplifies - and indeed confirms - many of the arguments I have advanced concerning the relation of Cato's Origines to the non-Roman historical tradition.

.....

We happen to be well informed about the legend of the origin of the Sabines. It is perhaps surprising, in view of the general paucity of our sources, that so many versions have come down to us; this must to some extent reflect the importance of the question for Roman antiquarians.

* For notes to Chapter X, see below pp. 368-372.

It is clear, however, that among the various surviving reports, one basic tradition predominates over all the others. This is the belief that the Sabines were in some way related to the Spartans. During the late Republic and early Empire the notion that the Sabines were actually descended from the Spartans seems to have been firmly rooted in the Roman literary tradition. Poets of the first century A.D. spoke of the Sabines as Spartan and apparently felt no need to explain themselves further. Thus Ovid:-

"Protinus Oebalii rettulit arma Tati..." (2)

and Silius Italicus:-

"Publicola, ⁿigentis Volesi Spartana propago." (3)

How this tradition originated is a matter of some dispute, but most scholars are agreed that the theory of a connection between the Sabines and Sparta was already known to Cato, who recorded the story in his Origines. (4)

This view has recently been challenged by J. Poucet. (5) He argues not only that the 'traditional view' is open to question, but that it is demonstrably wrong. He believes that Cato made no mention whatever of Spartans in his account of the Sabines, and that the 'Spartan theory' of Sabine origins appeared for the first time in the Annals of Cn. Gellius, who was writing at the end of the second century B.C.

It seems to me, for reasons which will become apparent, that Poucet's final conclusions are mistaken. But he was undoubtedly right to emphasise the uncertainty of the information furnished by the direct evidence, and to give serious attention to the problems raised by the conflicting testimonies of Servius (auctus) and Dionysius of Halicarnassus.

Passages from these two writers form the basis of our knowledge of what Cato said about the origin and early history of the Sabines. In Dionysius II.49. 2-3 we learn that, according to Cato, the Sabines took their name from Sabus, the son of a local god called Sancus, and that they originally lived at Testruna near Amiternum. Dionysius goes on to summarise Cato's account of the colonisation of the Sabine territory, and his description of the size of the country and its distance from the sea. The passage is clear and straightforward, and there can be little doubt that Dionysius has preserved the general lines of a passage of Cato's Origines.

But Dionysius then goes on to relate (II.49. 4-5) a story about a group of Spartans, who left their country at the time of Lycurgus and migrated to Italy; some of these Spartans settled among the Sabines. Dionysius concludes with the observation that the story explains the Spartan characteristics of the Sabines' way of life. This

Spartan story has also been attributed to Cato, and was included among the fragments of the Origines in H. Peter's edition.⁽⁶⁾

At first sight, however, it would seem that Dionysius has made a clear distinction between the two passages. The words with which he introduces the Spartan story seem to imply that it should be separated from the preceding Catonian account:-

" Ἔστι δὲ τις καὶ ἄλλος ὑπὲρ τῶν Σαβίνων ἐν ἱστορίαις ἐπιχωρίοις λεγόμενος λόγος..."

Moreover, it will be noticed that Dionysius has attributed the story to a source other than Cato, namely the *ἱστορίαις ἐπιχωρίοις*, whatever they may have been. As it stands, therefore, the chapter of Dionysius need not imply that Cato's Origines contained references to a relationship between the Sabines and Sparta.

Confusion arises, however, when we turn to the second of the 'fragments', the passage of Servius. In his note on Vergil's phrase "Curibusque severis", Servius writes:-

"Aut 'severis' disciplina, aut rem hoc verbo reconditam dixit, quia Sabini a Lacedaemoniis originem ducunt, ut Hyginus ait de Origine urbium Italicarum, a Sabo, qui de Perside Lacedaemonios transiens ad Italiam venit et expulsis Siculis tenuit loca, quae Sabini habent...Cato autem et Gellius a Sabo Lacedaemonio trahere

eos originem referunt; porro Lacedaemonios durissimos fuisse omnis lectio docet, Sabinorum etiam mores populum Romanum secutum idem Cato dicit: merito ergo 'severis' qui et a duris parentibus orti sunt et quorum disciplinam victores Romani in multis secuti sunt." (Serv.(auctus) ad, Aen. 8.638 = Cato F.51).

There is an obvious discrepancy between this passage and the statements attributed to Cato by Dionysius of Halicarnassus. In Dionysius Cato is reported to have said that the Sabines took their name from Sabus⁽⁷⁾ the son of a local god; but it is clear that this person cannot have been the same as the Lacedaemonian Sabus mentioned by Servius.

There are two possible explanations of this difficulty: (a) Cato has been misquoted or misunderstood by either Servius or Dionysius (or conceivably both); (b) Cato gave two rival versions, of which Dionysius reported one, Servius the other.

The solution which is most frequently advanced⁽⁸⁾ is that Servius confused his several sources at this point. The hypothesis is supported by the fact that the name of Cn. Gellius is mentioned alongside that of Cato - which at once suggests that Servius may have mixed up the ideas of the two writers. It is necessary, however, to ask why Servius mentioned Cato in this context. Poucet thinks

that he did not use Cato's Origines directly, but found Cato quoted at some point by Cn. Gellius and mistakenly attributed to him the views of that annalist.

But this explanation does not account for the second mention of Cato by Servius later in the passage. Moreover, it is unlikely that Servius should have consulted the bulky work of Cn. Gellius at first hand.⁽⁹⁾ But even if we allow that Servius' error may be due to a confusion of the versions of Cato and Cn. Gellius, perhaps because of a mention of Cato by Gellius, we are still a long way from Poucet's conclusion that Cato made no reference to Spartans in his account of the Sabines. The fact that Servius' evidence is unreliable does not mean that what he says is the exact opposite of the truth.

Poucet supports his contention with only one argument, and it is not a strong one. He writes: "Mais, dans le cas de Caton dont on connaît la position hostile à la race et à la culture helléniques, peut-on supposer avec vraisemblance qu'il ait abandonné une solution proprement italique des origines sabinnes pour adopter la thèse d'une provenance grecque du peuple sabin tout entier, ou tout au moins de son héros fondateur?" Against this one need only refer to the numerous fragments which show that Cato ascribed a Greek origin to many Italian cities and peoples (see above, Chapter VI, p. 157 ff.). Poucet admits this,

but nonetheless maintains that "il semble qu'il n'hésitait pas lorsqu'il pouvait choisir entre une solution italique et une solution grecque". But there is not the slightest evidence that Cato adopted a policy of this nature. A second argument against the Catonian authorship of the Spartan theory has been advanced by S. Mazzarino.⁽¹⁰⁾ He believes that Cn. Gellius reacted against Cato, and attempted a polemical refutation of what he found in the Origines. One can only say, however, that there is not a scrap of evidence in the fragments of Gellius to justify this view.⁽¹¹⁾ "La polemica di Cneo Gellio contro Catone," writes Mazzarino, "... è ricostruibile solo per ciò che riguarda i Sabini". But of Gellius' treatment of the problem of Sabine origins we know only what Servius tells us - that is, that Gellius said exactly the same as Cato!⁽¹²⁾

Mazzarino also suggests that the "ἱστορίαι ἐπιχωρίαι" referred to by Dionysius signify the Roman annalists, and in this particular instance the annals of Cnaeus Gellius himself. In this he is reverting to the view of Niebuhr and Kiessling.⁽¹³⁾ It is true that Dionysius refers to Cincius Alimentus and Calpurnius Piso as "ἐπιχωρίαι συγγραφαίς" (XII.4.2); but when Dionysius is dealing with the Sabines it seems only reasonable to assume that "native histories" refer to local histories of the Sabines. As H. Peter pointed out, this is con-

firmed by Dionysius' description of Sabus the son of a local god - "Σάβου τοῦ Σάγκου δαίμονος ἐπιχωρίου" (14)

In any case the story of the Spartan colony in Dionysius does not correspond to what we know of Cn. Gellius' version. Strictly speaking we have to distinguish between two traditions, or rather between two branches of one tradition. On the one hand there is the notion of a colony of Spartan "σύνοικοι", who settled among an already existing people of indigenous origin; and on the other hand there is the theory that the Sabines were entirely of foreign extraction, the descendants of a Spartan colony led by Sabus the Lacedaemonian.

The first of these theories we find in Dionysius, and comes, we are told, from native histories. The second is that of Cn. Gellius, and is also to be found, with further embellishments, in later writers such as Hyginus (Servius, loc.cit.). Poucet states that Gellius and Dionysius were independent of one another - and so they probably were, in the sense that Dionysius was not using Gellius; but the views they represent - (a) indigenous Sabines joined by Spartan *σύνοικοι*, and (b) immigrant Sabines descended from a Spartan colony - cannot ultimately be independent of one another. Unless we suppose that, by a remarkable coincidence, the two traditions arose separately and independently, we must assume that one is

antecedent to the other.

Which, then, is the earlier version? A priori it would seem that the more extreme version is the later - i.e. that the notion of the Sabines as pure Spartans is a later refinement of the more moderate view that they were a mixture of indigenous people and Spartan *σύνοικοι*. If this is in fact the case we must dispense with the view of Poucet and Mazzarino that the legend was dreamed up by Cn. Gellius in a fit of aetiological speculation: 'the Sabines live a Spartan life - ergo they are of Spartan origin'.⁽¹⁵⁾

But in point of fact the Spartan legend can be shown to be earlier than Cn. Gellius. To presuppose a purely aetiological origin for the tradition is to ignore its significance as a historical phenomenon with deep roots and going back at least to the fourth century B.C. Moreover it was well established in local tradition long before it first appeared in Roman literature. I shall give justification for this view later, but I state it as a fact for the present.

The novelty of Gellius' version lay in the fact that he gave the Sabines a purely Spartan origin, and nominated Sabus Lacedaemonios as their eponymous founder. His interpretation looks like a rather arbitrary distortion of the original legend which we find reflected

in Dionysius. It is perhaps worth noting here that (16)
Gellius seems to have had a liking for perverse variants,
rather in the manner of his successors, the Sullan
annalists.

As for the possible source of Gellius, all the indications seem to point to Cato. Let it be said, first of all, that the Spartan story given by Dionysius, which represents an earlier stage in the development of the tradition than Gellius' version, is by no means incompatible with Cato's statement that Sabus was the son of a local god and that the Sabines came originally from Amiternum. The story of the Lacedaemonian *σύνολκοι* presupposes that the Sabines already existed as a people, and how they got their name is irrelevant.

That Cn. Gellius used Cato is likely enough in itself; the fact that he gave attention to the matter of Italian origins shows that he was influenced (as were other historians of the second century B.C.) by Cato's Origines.⁽¹⁷⁾ Moreover, two of the most important of Gellius' predecessors, Fabius Pictor and Cincius Alimentus, can be excluded, in view of what they said about the 'mores' of the Sabines. Fabius had characterised the Sabines in a most eccentric fashion, as Strabo tells us:-

" φησὶ δ' ὁ συγγραφεὺς Φάβιος Ρωμαῖους ἀισθέρθαι τοῦ κλοῦτου τότε πρώτων, ὅτε τοῦ ἔθνους τούτου (sc. τῶν

Σαβίνων) κατέστησαν κυρίως "(18)

It need hardly be said that this statement is contradicted by a large body of testimony given by other writers.⁽¹⁹⁾ Some modern commentators have been so surprised by it that they have attempted to take the rather strangely worded phrase " αἰσθέσθαι τοῦ πλούτου " to mean that the Romans first perceived their own wealth when they became rulers of the Sabines.⁽²⁰⁾ In other words, the Romans realised that they were relatively well off when they compared themselves with the Sabines. But this interpretation of the text is not really justified on linguistic grounds alone; the sense of the Greek implies, on the contrary, that the Romans were poor and that the Sabines were rich. That this was what Fabius Pictor really said is confirmed by a reference in Dionysius' account of the story of Tarpeia:

" καὶ αὐτὴν (sc. Τάρπειαν), ὡς μὲν Φάβιος τε καὶ Κίγκιος γράφουσιν, ἔρως εἰσέρχεται τῶν πεδλίων, ἃ περὶ τοῖς ἀριστεροῖς βραχίουσιν ἐφόρουν, καὶ τῶν δακτυλίων· χρυσοφόροι γὰρ ἦσαν οἱ Σαβίνοι τότε καὶ Τυρρηγῶν οὐκ ἦπτον ἀβροδίαιτοι ".⁽²¹⁾

This may be simply an attempt to rationalise the story of Tarpeia, and to explain how the Sabines came to be wearing golden "armillae"; but the language of Dionysius suggests that Fabius Pictor was deliberately

insulting. The contrast between Fabius and the unanimous verdict of all other sources is so striking⁽²²⁾ that one is almost inclined to agree with A. Alföldi's contention - that Pictor's denigration of the Sabines was due to his personal hatred of the Appii Claudii, a Sabine gens.⁽²³⁾

In any case Fabius' view was the exact opposite of the traditional opinion, and was no doubt also inconsistent with the facts. This is revealed by the use of the word "τότε", which at least implies a contrast between the condition of the primitive Sabines in the days of Titus Tatius and their situation in the age of the writer himself. Cincius Alimentus followed Fabius.

It is more than likely, therefore, that when Cato turned to the early history of the Sabines he wrote in a spirit of reaction and in an attempt to counter the allegations of his two established forerunners, Fabius and Cincius. It would not be unreasonable to view Cato's account of the Sabines partly as a piece of polemic. The validity of this hypothesis is enhanced by the fact that Cato was himself closely connected with the Sabines (see below). In these circumstances, it would seem that a reference to the Spartan legend, together with an assertion that the customs of the Sabines were in fact Spartan, would be very well suited to Cato's purpose. It is likely, there-

fore, that Cato should have recorded the story of the Spartan colony if he knew about it.

That Cato did know of the story seems likely in view of the fact that it was a local tradition. This, of course, is attested by Dionysius of Halicarnassus, whose reference to 'native histories' implies some kind of documentary record.⁽²⁴⁾ It is perhaps possible that the written " *ἱστορία ἐπιχωρία* " were a late compilation, available to Dionysius (or his source), but later than Cato. Nevertheless, I think it can be shown that the Spartan story formed part of a tradition of long standing, and goes back long before Cato.

We have several times remarked on Cato's diligence in investigating local traditions of Italy; but it is especially likely that he should have given some account of what the Sabines thought about their own past, because he maintained throughout his life a close connection with the Sabine country and its people. Cato was born at Tusculum,⁽²⁵⁾ but he spent his early life among the Sabines, on an estate which he received as an 'heredium'.⁽²⁶⁾ This fact is significant enough in itself, and it is an attractive hypothesis that the influence of this robust people and their proverbially severe way of life played a part in shaping Cato's own famous reputation for traditional austerity. He himself refers to his rugged upbringing

among the Sabines in the speech De Suis Virtutibus
(frg.128 Malc.):-

"ego iam a principio in parsimonia atque in duritia atque industria omnem adulescentiam meam abstinui agro colendo saxis Sabinis silicibus repastinandis atque conserendis".

It is probable that Cato identified his 'duritia' with that of the Sabines. In view of the numerous references in ancient writers to the "disciplina" of the Sabines, one would expect Cato to have made something of his Sabine background, and Frg.128 Malc. seems to confirm that he did. Certain chapters of Plutarch suggest Cato set himself up as a patron of the Sabines;⁽²⁷⁾ we are told that he developed his oratorical gifts by acting as an advocate for anyone who needed his services in the villages and towns around Rome; and he apparently offered his help free of charge (Plut. Cato, I.4-5). Valerius Flaccus learned from Cato's slaves how their master went every morning to the local market place and pleaded for all those who needed him (Plut. Cato, III.2).

As I have argued, the structure and content of the Origines imply that Cato was attempting to champion the peoples of Italy, and to stress the fact that Roman history was the history of Italy as well as of Rome. His connections with the Sabines cause one to suspect that he gave special

prominence to their early history and to the part they played in the story of early Rome; and it so happens that Servius confirms this assumption:-

"Sabinorum etiam mores populum Romanum secutum idem Cato dicit".

The implication of this assertion is that the traditional virtues of the Romans, which Cato struggled to defend throughout his career, were not originally Roman at all, but Sabine.

It is therefore probable that Cato should have applied himself carefully to the question of the origin of the Sabines in his Origines. The suggestion that he recorded the native traditions has everything to recommend it, especially as he was himself a Sabine by domicile, if not by birth.

Finally, it must be emphasised that the passage of Dionysius which is explicitly stated to come from Cato (II.49. 2-3) bears out the general conclusion that Cato was familiar with, and made use of, local traditions of the Sabines. This must surely be the implication of the phrase

"... ἐπὶ Σάβου τοῦ Σάγκου δαίμονος ἐπιχωρίου, τοῦτον δὲ τὸν Σάγκον ὑπὸ πικρῶν Πισίων καλεῖσθαι Δία "

And the precise geographical and (relative) chronological details in the account of the invasion of the Reatine

territory suggest that it was based on a coherent indigenous account.

My conclusion, therefore, is that Cato's knowledge of the earliest history of the Sabines derived from local sources, possibly including a written account; further, the source of Dionysius' Spartan story (II.49 4-5) was almost certainly Cato. The *ἱστορία ἐπιχωρία* referred to there will have been cited by Cato himself.

If Cato was the source of Dionysius, we must ask why the latter drew a distinction between Cato and the *ἱστορία ἐπιχωρία*. H. Peter, who was very strict in excluding doubtful fragments, nevertheless inserted the Spartan story of Dionysius because he thought that Dionysius found the local histories quoted as a source in Cato's Origines:-

"Quod vero auctoritati domesticorum Sabinorum annalium Catonis nomen cessit, hic mos illorum scriptorum fuit, ut si quem is quem transcriberent, citasset fontem, veterem auctorem praeferrent". (28)

This seems to me to be the most likely interpretation of Dionysius' citation of *ἱστορία ἐπιχωρία*. The phrase " ἔστι δέ τις καὶ ἄλλος ὑπὲρ τῶν Σαβίνων ἐν ἱστορίαις ἐπιχωρίαις λεγόμενος λόγος..." need not indicate a change of source. It should not be translated: "but there is an alternative account of the Sabines

in the local histories", but rather: "there is also a further story about the Sabines in the local histories...". In other words, it is a continuation of the preceding (Catonian) account.

.....

(ii) The legend of the Spartans in Italy

We must now turn to the fundamental question of how the Spartan legend originated in Italy. My contention that Cato reported a relationship between the Sabines and the Spartans rests ultimately on the assumption that the legend originated early and was already firmly established as a popular tradition among the Sabines when Cato began his investigation of the early history of Italy. Poucet's arguments are convincing enough as far as they go, but he confines himself too closely to the internal problems of the direct evidence in Dionysius and Servius. It seems to me that he has not taken sufficient account of the wider implications of the legend.

In fact the tradition which connects the Sabines with Sparta is only one of several manifestations of a more general tendency to assimilate the Spartans with the Sabellic peoples as a whole.⁽²⁹⁾ In the account of the 'local histories' in Dionysius the Spartan émigrés are said to have arrived, after many adventures, at Feronia

(so called because they were borne across the sea).
 Feronia is a little way down the coast from Terracina,
 and it is not a coincidence that no fewer than three
 towns in the immediate neighbourhood were also thought
 to have been Spartan foundations. They are Formiae,⁽³⁰⁾
 Caieta⁽³¹⁾ and Amundae.⁽³²⁾ The Lacedaemonians who
 founded the last named city are also said to have been
 of the Pythagorean sect. This combination of Spartan
 and Pythagorean elements clearly points to the influence
 of the cities of Magna Graecia, and particularly of Tarentum.
 It is often suggested that the legend of Spartans in central
 Italy originated among the people of Tarentum, who imposed
 a Spartan origin-legend on the native peoples of the
 hinterland.⁽³³⁾

The evidence for this reconstruction is a passage
 of Strabo (V.4.12 p.250 C):-

" τινὲς δὲ καὶ Λάκωνας συνοίκους αὐτοῖς (sc. τοῖς
 Σουνίταις) γενέσθαι φασὶ καὶ διὰ τοῦτο καὶ φιλέλληνας
 ὑπάρξαι, τινὰς δὲ καὶ Πιτανάτας καλεῖσθαι. δοκεῖ δὲ
 καὶ Ταραντίνων πλάσμα τοῦτ' εἶναι, κολυκευόντων
 ὀρόρους καὶ περὶ θυμαρένους ἀνδρώπους καὶ ἄρα
 ἔξοικουμένων, οἳ γε καὶ ὀκτὼ μυριάδας ἑσπελοῦν ποτε
 τῆς πεζῆς στρατιᾶς, ἵππιας δ' ὀκτακισχιλίους ".

This statement confirms that the legend originated
 in Tarentum, and informs us that it was invented for a

political purpose. From this we can infer that the story was created at a relatively early date, because the mention of a military alliance between the Samnites and Tarentum points to the period when the Samnites and Tarentines were still politically independent - i.e. at least before the capture of Tarentum by the Romans in 272 B.C.

We must suppose that the Tarentines invented a story, purporting to be the product of antiquarian research, that a colony of Spartans had arrived in Samnium in the distant past and had been absorbed by the native population. The aim will have been to show that the Samnites were related by descent to the inhabitants of Tarentum, itself a Spartan colony. Such an attempt to assimilate the Samnites and Tarentines could be dated at almost any time in the late fourth or early third century B.C., because it would be consistent with what we know of the historical situation in Southern Italy at that time. Friendship between the Samnites and Tarentum can be traced back into the fourth century B.C. This is indicated for example by Livy, who records that in 326 the Samnites were supported by Tarentum in their defence of Neapolis (Liv. 8.27, etc.). It has also been plausibly argued that the Tarentines gave the Samnites financial aid in their wars against Rome.⁽³⁴⁾ Moreover, coins of the fourth century, bearing

the legend ΣΑΥΝΙΤΑΝ (reversed) were minted for the Samnites by the Italiot Greeks, which is clearly a sign of good relations. (35)

Strabo's statement also shows that the story of the Spartan colony was accepted by the Samnites themselves. He tells us not only that the Samnites "ὄν τούτο φι-
 ἑλληνας ὑπάρξαι", but also that some of them were called 'Pitanatae'. This last piece of information is important, because it can be confirmed by independent evidence. This evidence consists of some coins of the fourth century, which are inscribed

ΠΕΡΙΠΟΛΩΝ ΠΙΤΑΝΑΤΑΝ (36)

The coins were found in Samnium and are therefore proof that people called Pitanaetae really were living there in the fourth century. The name was perhaps a corruption of some local (Oscan) word or place name which sounded like the district of Laconia - in the same way as Amunclae became Amycle. The important fact, however, is that the Spartan legend is shown to have been established as early as the fourth century B.C., and moreover that the Samnites themselves had accepted it, because the coins were used by them. In this sense, therefore, we can say that the Spartan legend had already become a part of local Italic tradition.

Strabo's statement that the legend of a Spartan colony

was fabricated by the Tarentines for a political purpose receives confirmation from a comparison with further signs of Tarentine propaganda in the same period. Several other anecdotes and fragments of evidence seem to indicate a general attempt to assimilate the native peoples of Italy with the Tarentines, and to show that friendly contact between them went back far into the past.

First we may note that a conversation is reported by Cicero (Cato Maior 12.41) between Plato, Archytas of Tarentum and C. Pontius Herennius (the Samnite leader whose son defeated the Romans at the Caudine Forks in 321 B.C.). It is likely that Cicero took this story from a Pythagorean source, probably Aristoxenus.⁽³⁷⁾ Other stories of a similar kind can be traced back to Aristoxenus with some certainty. He is supposed to have written that Pythagoras himself was a Tyrrhenian - from one of the islands from which the Tyrrhenians were later expelled by the Athenians (Aristoxenus fr.11 Wehrli). Again we find that according to Aristoxenus the pupils of Pythagoras included Lucanians, Messapians, Picentines and Romans (Aristox. fr.17 Wehrli). This is probably also the origin of the tradition that king Numa was a pupil of Pythagoras, which is parallel to the view of Aristoxenus (fr.43 Wehrli) that the famous lawgivers Zaleucus and Charondas were Pythagoreans.⁽³⁸⁾ It may be that the story of a Spartan

colony among the Samnites also goes back to a Pythagorean source. At any rate all these pieces of evidence can be taken as signs of the efforts of the Tarentines to extend their influence over the indigenous inhabitants of central and Southern Italy in the fourth century.

The legend of the Lacedaemonian *εὐνοικοί* was created with specific reference to the Samnites. But it should not surprise us that it later came to be associated with the Sabines. The Sabines and Samnites were closely related, as is shown by the etymological similarity of their names.⁽³⁹⁾ And there was an ancient tradition that the Samnites were descended from the Sabines, having migrated after a "ver sacrum" (Strabo V.4.12 p.250 C). The Spartan story will have been attached to the Sabines by a simple transference, perhaps based on the idea that the alleged arrival of the Lacedaemonian colonists had taken place before the migration of the Samnites from their Sabine homeland. Possibly aetiological considerations played a part in this. The same process is obviously implied in the statement of Trogus Pompeius, that the Lucanians practised a Spartan system of education (Justin 23.1.7), since the Lucanians were no more than an offshoot of the Samnites.

In any case it is likely that the Sabines would have been pleased to adopt a tradition which connected them

with Sparta. As we have seen it would not be the only example of a native people accepting an origin-legend imposed on them by the Greeks.

.....

(iii) Conclusion

In conclusion it can be said that what we know of Cato's account of the origin of the Sabines illustrates, by way of example, three of the main contentions of this thesis as a whole.

First, the treatment of the early history of the Sabines in the Origines confirms that independent local sources were ^aavailable to Cato and were widely used by him.

Secondly, the history of the Spartan story illustrates the way in which hellenocentric legends had penetrated into the local popular traditions of central Italy long before the Roman conquest; and that when Cato attributed a Greek origin to some of the native peoples of Italy he was not necessarily drawing on erudite hypotheses of purely literary currency, but was faithfully repeating stories which had long since been established in the national tradition of the Italian communities themselves.

Finally we may observe that the subsequent fortune of the legend in ~~the~~ Roman literature bears out our view that Cato's Origines was of crucial importance in bringing

into the received tradition of the Romans stories which had originated outside Rome.

.....

(iv) Appendix: Coins of the 'Pitanatae' (40)

A place called Περικολίον is referred to by Thucydides (III.99; cf. 103.3, 115.6), apparently located between Locri and Rhegium. However, it seems certain that the coins inscribed ΠΕΡΙΠΟΛΑΝ ΠΙΤΑΝΑΤΑΝ do not come from this place;⁽⁴¹⁾ rather, they are to be connected with the Pitanatae mentioned by Strabo p.250 C., because they were found in Samnium, and small coins of this kind rarely travel far.⁽⁴²⁾

Their date cannot be precisely fixed, but they are probably of the fourth century because the types copy the gold coins of Syracuse of the late fifth or early fourth century B.C.⁽⁴³⁾ Some of these coins are signed by Cimon and Euaenetus, and this connects the gold with the silver series which contains coins similarly signed.⁽⁴⁴⁾ Euaenetus' name is found also on coins of Catana, depopulated in 404, and Camerina, depopulated in 405 - dates which therefore provide a rough terminus ante quem for the career of Euaenetus. The Syracusan coins signed by him cannot reasonably be placed later than c.350 B.C. at the latest; and the coins of the Pitanatae, which copy them, are not

likely to be much later than the original issue.

That the types copy coins of Syracuse, rather than of, say, Tarentum, need not surprise us, because Syracusan types were widely circulated, and were copied all over the Greek world. (45)

(v) Notes to Chapter X

- (1) Dion. Hal. II.49. 2-5 = Cato, Origines F.50.
See above, Chapter IV, pp. III-2; IV-6.
- (2) Ovid, Fasti I.260. Cf. Fasti III.230: "Oebalii matres". Oebalus was a mythical king of Sparta.
- (3) Sil. Ital. Punica II.8; cf. Punica VIII.412: "Therapnaeo a sanguine Clauso". The Valerii and Claudii were the most famous of the Sabine gentes. It is perhaps significant that Sparta was one of the Greek cities of which the Claudii became patrons in the later Republic.
- (4) See e.g. Ed. Meyer, Gesch. des Altertums III² p.462 n.2; E. Pais, Storia di Roma I, p.316 n.1; id., "Atti d. reale Accad. Arch.", Naples, N.S.2, 1910, p.27; H. Phillip, R.E. s.v. "Sabini", 1574; P. Fraccaro, Studi Varroniani, p.232; E. C. Evans, Sabine Cults, pp.28-9; E. J. Bickermann, "Class. Phil." 47, 1952, p.74; J. Berard, La colonisation grecque, p.467; E. Gabba, "Athenaeum" 38, 1960, p.186; A. Alföldi, Early Rome and the Latins, p.152; E. Rawson, The Spartan Tradition in European Thought, p.99.
- (5) J. Poucet, Les origines mythiques des Sabins, in "Etudes Etrusco-Italiques", Université de Louvain 1963, p.155 ff.
- (6) F.50. As far as I am aware, Peter was the first to attribute this additional story in Dionysius to Cato. I can find no trace of any attempt to connect Cato with Dionysius II.49. 4-5 in the works of Niebuhr, Schwegler or Lewis, all of whom cite this passage but with no suggestion that its source might be Cato. Indeed, Niebuhr thought that it derived from Cn. Gellius - see below, note 13.
- (7) The Mss. have "ἐν Ἐαβίῳ". F. Sylberg, in his edition of Dionysius (1586) emended this to read "ἐν Ἐάβῳ". Poucet (art.cit. p.163 n.4) thinks that Sylberg was influenced by the passage of Servius quoted above, apparently without noticing that it made the contrast between the two

passages even sharper. But it seems to me that there is more justification for Sylberg's correction than Poucet allows. It is worth noting Silius Italicus, Pun. VIII.421 f:-

"Ibant et laeti pars Sancum voce canebant,
Auctorem gentis, pars laudes ore ferebat,
Sabe, tuas, qui de proprio cognomine primus
Dixisti populos magna ditioe Sabinos"

- lines which surely refer to the indigenous hero, the son of Sancus, mentioned by Dionysius.

- (8) H. Peter, HRR I² p.cxxxiv; Poucet, art.cit. p.161, and works cited there.
- (9) He is more likely to have used Cato directly, since he quotes from the Origines many times (F.4,5,8,9,10,11,13,14,18,31,32,38,45,46,48,51,54,55,60,62,70,76,80,85,89,115,119,122,123,137,138,139,140; cf. Peter, HRR I² p.clvii f.), but mentions Gellius on this one occasion only. But it is more probable that Servius cited early writers such as Cato and Gellius from second and third hand sources, and that the confusion is due to more complex circumstances.
- (10) S. Mazzarino, Il Pensiero Storico Classico, II p.89 ff.
- (11) We know that Cato delivered a speech pro L. Turio contra Cn. Gellium (frg.206 Malc.); but this is a totally isolated piece of evidence, and it is impossible to say whether or not the Gellius referred to is to be identified with the annalist of that name,
- (12) Mazzarino believes that Servius cited Cato and Gellius together because both emphasised the 'severity' of the Sabines. He adds "la polemica di Gellio contro Catone consisteva in questo: che Catone deduceva la severità dei Sabini dalla loro stessa origine montanava Italica, da 'Sabo loro dio indigeno'; mentre Gellio la spiegava con la severità Spartana" (op.cit. p.90). But this simply begs the question.
- (13) Mazzarino, op.cit. p.90. Cf. B. G. Niebuhr, History of Rome, I, p. 104 n. 329 ; A. Kiessling, De Dionysii Hal. antiquitatum auctoribus Latinis,

- Diss, Bonn, 1858, p.35.
- (14) H. Peter, HRR I² p.cxxxiv n.2.
- (15) Poucet, art.cit. p.172; Mazzarino, op.cit. p.90.
- (16) Dion. Hal. II.31.1 (= Cn. Gellius F.11 Peter):-
 "ταῦτα δὲ γενέσθαι (rape of the Sabine women)
 τινὲς μὲν γράφουσι κατὰ τὸν πρῶτον ἐνιδυτὸν τῆς
 Ῥωμύλου ἀρχῆς, Ἰναῖος δὲ Τέλλιος κατὰ τὸν τέταρτον".
- (17) F. Münzer, R.E. s.v. "Gellius" (no.4) 999;
 F. Leo, Gesch. d. röm. Lit. I p.329, on Cassius
 Hemina.
- (18) Strabo V.3.1 p.228 C = F.Gr.Hist. 809 F.27 =
 Fabius Pictor F.20 Peter.
- (19) Cicero, Vat., 36; Fam., 15.20.1; Lig., 11.32;
 Livy, I.18; Vergil, Aen. 8.6; 6.811; Georg., 2.532;
 Horace, Od., 3.6.39; Epod., 2.41; Epist., 2.1.25;
 Ovid, Am., 2.4.15; Met., 14.797; Propertius, El.,
 2.32.47; Columella, 10.137; Martial, Ep., 10, 32;
 11.15; Juvenal, Sat., 6.163-4; 3.85, 169; Statius,
Silv. 5.1.123.
- (20) E.g. H. L. Jones, in the Loeb edition:-
 "Fabius the historian says that the Romans
 realised their wealth for the first time when they
 became established as masters of this tribe".
- (21) Dion. Hal. II.38.3 = F.Gr.Hist. 809 F.6 = Fabius
 Pictor F.8 Peter; F.Gr.Hist. 810 F.3 = Cincius
 Alimentus F.5 Peter.
- (22) Dionysius points the contrast: in II.49 he
 speaks of "τὸ λιτοδίαιτον" - but in II.38 he
 describes the Sabines as "ἄβροδίαιτοι", citing
 Fabius Pictor.
- (23) Alföldi, Early Rome and the Latins, p.152.
- (24) Plutarch also refers to the Spartan tradition,
 and states explicitly that it was what the Sabines
 themselves believed: "Σαβίνοι βούλονται Ῥωμαίωνων
 ἑαυτοὺς ἀποίκοις γεγονέναι" - Plut. Numa, 1.
- (25) Nepos, Cato 1; Plut. Cato Maior, 1; Cic. Planc.
 8.20; de leg. 2.2.5; Schol. Bob. Pro Sulla 23 p.80
 Stangl; cf. Cic. de rep. 1.1; A. Gell. N.A. 13.24:

"homo Tusculanus".

- (26) Nepos, Cato 1; Cic. Cato Maior 24 and 46; de rep. III.40; Cato, orat. frg.128 Malc.
- (27) Cf. M. Gelzer, R.E. s.v. "Porcius" (no.9) 109; D. Kienast, Cato der Zensor, p.36.
- (28) H. Peter, HRR I² p.cxxxiv.
- (29) Cf. J. Bérard, La colonisation grecque de l'Italie méridionale et de la Sicile dans l'antiquité, 2nd ed. Paris 1957, p.466.
- (30) Strabo V.3.6 p.233 C. It was formerly called Hormiae "διὰ τὸ εὐορρον".
- (31) Ibid. It was called Caieta "τὰ γὰρ κοῖλα πάντα καίετας οἱ Λάκωνες προσκροεῦουσιν".
- (32) Serv. ad Aen. 10.564: "et ab Amyclis provinciae Laconicae civitate ei nomen inditum est".
- (33) See e.g. Ed. Meyer, Gesch. d. Altertums² III, p.462 n.2; E. Pais, La pretesa origine spartana dei Sanniti, dei Sabini e dei Romani, "Atti. Accad. Arch." Naples, N.S. 2, 1913, pp.25-39; E. Ciaceri, Storia della Magna Grecia, I, 1924, pp.98-9; J. Bérard, La colonisation grecque... (cit. n.29), p.467, etc.
- (34) See P. Wuilleumier, Tarente des origines à la conquête romaine, Paris 1939, pp.73,92 etc.
- (35) The dialect is Doric. E. T. Salmon (Samnium and the Samnites, p.71 n.6) suggests: "perhaps they were minted at Tarentum as a compliment to the Samnites".
- (36) For a discussion of these coins, see appendix p. 366 f.
- (37) See P. Wuilleumier, Cicéron: Caton l'ancien, Paris 1955 ("Les belles lettres"), p.53.
- (38) Cf. E. Gabba, Tradizione letteraria..., Fondation Hardt, Entretiens XIII, p.158.
- (39) See E. T. Salmon, Samnium and the Samnites p.30.

- (40) For discussion see B. V. Head, Historia Numorum² p.27. Pictures of two examples can be found in SNG II (Lloyd) no.44, and SNG V (Ashmolean) n.26.
- (41) As was suggested by B. V. Head, in the first edition of Hist. Num. p.91.
- (42) Thus Mommsen, Gesch. d. römischen Münzwesens, p.119; cf. B. V. Head, Hist. Num.² p.27; A. Sambon, Les monnaies antiques de l'Italie I, Paris 1903-4, p.104.
- (43) C. Seltman, Masterpieces of Greek Coinage, Oxford 1949, p.93, no.41.
- (44) On Cimon and Euaenetus see e.g. Seltman, op.cit. p.17 ff., etc.
- (45) Cf. coins of Locris, - Hist. Num.² p.336, fig.190.

CHAPTER XI: EXCURSUS *

(i) The Ktiseis

The table below covers the period down to the first century B.C. It contains (a) a list of authors who wrote works entitled Ktiseis (it may not be comprehensive, but it mentions all those known to me, and I trust that there are no significant omissions), (b) a brief account of what is known about these works, and (c) a statement of the particular problems arising in each case.

1. XENOPHANES OF COLOPHON (F.Gr.Hist.450)

Diogenes Laertius (IX.20) writes of Xenophanes:

"Ἐποίησε δὲ καὶ Κολοφῶνος κτίσιν καὶ τὸν εἰς Ἑλέαν τῆς Ἰταλίας ἀποικισμὸν ἔπη Διοχίδια". (T.1 Jac.)

This statement probably refers to a single poem.⁽¹⁾

We know nothing else about it, unless the few lines cited by Athenaeus (XII.526a = frg.3 Diels), on the luxury of the Colophonians, are taken from the Ktiseis.⁽²⁾ But the context of this fragment is uncertain, and obviously it cannot be taken as proof that Xenophanes wrote such a work.⁽³⁾ The possibility that Diogenes Laertius's information comes from the work "On Poets" by the literary

(* For notes to Chapter XI, see below pp. 401-407.

forger Lobon of Argos⁽⁴⁾ must cast serious doubts on the authenticity of the citation. The view that Herodotus's account of the Phocaeen adventures in the West (I.163-7) is based on Xenophanes seems far-fetched.⁽⁵⁾

2. "CADMUS OF MILETUS" (F.Gr.Hist.489)

The "Κτίσις Μιλήτου καὶ τῆς ὁδοῦ Ἰωνίας"
(Suda s.v. Κάδμος Μιλήτιος = T.1 Jac.), ascribed to the sixth century writer Cadmus of Miletus, was a Hellenistic forgery.⁽⁶⁾

3. HIPPIYS OF RHEGIUM (F.Gr.Hist.554)

The bibliography of this West Greek historian given in the Suda (s.v. Ἴππυς = T.1 Jac.) consists of five titles: "Σικελικαὶ πράξεις "; "Κτίσις Ἰταλίας "; "Σικελικά " in five books; "Χρονικά " in five books; and three books of "Ἀργολικά ". It is probable, however, that at least some of these titles should be combined. For instance we may safely equate the Σικελικαὶ Πράξεις and the Σικελικά . A fragment attributed to " Ἴππυς ἐν τῷ Περὶ Χρόνων " (F.1 Jac.) refers to the foundation of Croton. This suggests that we should perhaps identify the Χρονικά with the Κτίσις Ἰταλίας . Jacoby argued that because the Χρονικά and the Σικελικά are each given five books they too may have been alternative titles for a single work. He

therefore assimilated the *κτίσις Ἰταλίας*, the *Χρονικά* and the *Σικελικά*. In other words the *κτίσις Ἰταλίας* and the *Σικελικά* may have been not separate works but rather subsections of a comprehensive account (*Χρονικά*?) of early Italy and Sicily.⁽⁷⁾

Not much more can be said about the *κτίσις Ἰταλίας* as such, and in general Hippias remains an obscure figure. His date, in particular, is a matter of uncertainty. The direct evidence, such as it is, indicates a date in the first half of the fifth century B.C., which would make Hippias the earliest known writer of *Ktiseis* in prose, as well as by far the oldest Greek historian of the West. But other considerations would suggest a somewhat lower date. The matter is complex and has aroused a good deal of controversy; I have therefore added an extended discussion of it in an appendix at the end of this chapter.

4. ION OF CHIOS (F.Gr.Hist.392)

Ion's "*Χίου κτίσις*" is the earliest reliably attested work of its kind. The title is given by Schol. Arist. Peace 835 = T.1 Jac., and Etym. Magn. p.569 = F.3 Jac.⁽⁸⁾ It is certainly one of the earliest known *Ktiseis* in prose. The earlier view that the "*Χίου κτίσις*" was a poem⁽⁹⁾ can no longer be maintained.⁽¹⁰⁾ B. Schmid's theory, that the work was composed in verse and published in prose form only in the Hellenistic age, is unwarranted.⁽¹¹⁾

In view of the uncertainties over Xenophanes and Hippys, it is possible that the Ktiseis as a special branch of literature - i.e. special works devoted exclusively to the subject of foundations - began with Ion in the second half of the fifth century.

We possess three certain fragments of Ion's Ktiseis, including one of substantial length (F.1 Jac. = Paus. VII.4.8). Blumenthal adds to this number a story which Aelian ascribed to "οἱ τὰς ὑπὲρ τῆς Χίου συγγράψαντες ἱστορίας".⁽¹²⁾ But the passage contains a typical piece of Hellenistic paradoxography, and it is doubtful whether these 'historians' include Ion.⁽¹³⁾

5. HELLANICUS OF LESBOS (F.Gr.Hist.4)

A work entitled Ktiseis is attributed to Hellanicus of Lesbos by Athenaeus (X.447 C = F.66 Jac.) and in Pap. Oxy. XIII,1611, col II,212 (= F.68 Jac.), if Allen's restoration "Ἑλλάνη]κος ἐν [ταῖς Ἑθνῶν (?)] κτίσεσι" is justified. Stephanus of Byzantium quotes from "κτίσεις Ἑθνῶν καὶ πόλεων (s.v. Χαριμάται = F.70 Jac.). We hear also of a Περὶ Χίου κτίσεως (Schol.Hom. Od. .294 and Tzetz. ad Lyc. Alexandra 227 = F.71 a-b Jac.), which is perhaps only a subheading of the κτίσεις Ἑθνῶν... etc. In a note Jacoby argued strongly for the independence of the Περὶ Χίου κτίσεως citing the precedent of Ion's work on the same subject, but he seems to have revised his

opinion subsequently.⁽¹⁴⁾

Hellanicus' Ktiseis are normally classified among his ethnographical works⁽¹⁵⁾ which constitute the least known area of his literary activity. Jacoby believed that the *Περί Ἑθνῶν* (F.69) and the *Ἑθνῶν ὀνομασία* (F.67) were identical with the *Κτίσις Ἑθνῶν καὶ Πόλεων*, and argued that it was an ethnographical compilation of learned or antiquarian character, similar to the *Ἑθνῶν κατάλογος καὶ Πόλεων* written at about the same time by Damastes of Sigeum (F.Gr.Hist. 5 F.1). Certainly F.66 indicates that the Ktiseis were concerned to some extent with "νόμοι".

A further problem is the relationship of the Ktiseis to the *Βαρβαρικά Νόμιμα* (F.72-3). It has been argued that Hellanicus' ethnographical studies were comprehended in two general works: the Ktiseis, dealing with Greek colonisation in the Aegean and Asia Minor, and the Barbarika Nomima, which was concerned with Barbarian customs. But a reference in Stephanus of Byzantium shows that barbarian tribes were also treated in the Ktiseis (F.70), and it is therefore possible that Barbarika Nomima was a subtitle for a part of the Ktiseis. Unfortunately the wretched state of our evidence makes it impossible to give a definite answer to any of these questions.⁽¹⁶⁾

In view of this uncertainty, we must confine our attention to the known fragments of the Ktiseis. The

position can be briefly summarised:-

The Ktiseis dealt with νόμοι (F.66, cf. 67), with the geographical location of places (F.70, cf. 69), and with barbarians as well as Greeks (ibid.). F.69 must indicate that the Περὶ Ἐθνῶν (i.e. the Ktiseis?) was arranged in the form of a Periodos:

" Βόσπορον διαπλεύσαντι Σίνδοι, ἄνω δὲ τούτων Μαιῶται Σκύθαι ". (17)

The most substantial fragment (F.71) is from the Περὶ Χίου Κτίσεως, and concerns the settlement of Lemnos. It describes the origin of the inhabitants - the Sinties- and gives an explanation of their name based on their occupation:-

" διὰ τὸ πρῶτους ὄπλα ποιῆσαι πολεμικὰ, παρὰ τὸ σίνεσθαι τοὺς πλησίον καὶ βλάπτειν ".

6. CHARON OF LAMPSACUS (F.Gr.Hist.262)

" Χάρων Λαμφακηνός ἔγραψε κτίσεις πόλεων ἐν βιβλίοις β' " (Suda, s.v.).

Unless F.6 is taken from this work, we know nothing else about it apart from the entry in the Suda. But there are no good a priori grounds for doubting its existence. Schwartz's view that "κτίσεις Πόλεων" was merely a subtitle of the "Ἔθροι Λαμφακῆνων" is unjustified. (18) Jacoby puts it in the same category ("Sammelliteratur") as Hellanicus' "κτίσεις Ἐθνῶν καὶ Πόλεων" and

Damastes' "Ἑθνῶν κατάλογος καὶ πόλεων". (19)

7. ARISTOTLE

Plutarch speaks once of Aristotle's "κτίσεις καὶ Πολιτεῖαι", in Mor. 1093 B. (20) But the value of this citation should be judged in the light on the fact that in the same sentence the title Hellenica is attributed to Herodotus, and Persica to Xenophon. We are evidently dealing with a reference to Aristotle's collected constitutions; and if the title "κτίσεις καὶ πολιτεῖαι" signifies anything, it is that (to judge by the Ath. Pol.) each of Aristotle's Politeiai was divided into two parts: the "archaeologia" and the descriptive "politeia". (21)

8. ZOPYRUS (F.Gr.Hist.494)

Our information about Zopyrus consists solely of the three fragments listed by Jacoby, and these are unhelpful. A scholiast on Homer purports to quote from the fourth book of Zopyrus' "Μιλῆτου κτίσις" (F.1) - a quotation which deals with the text of Homer Il.10.274. His date is "probably pre-Hellenistic". (22)

9. MENEKRATES OF ELEA (Müller, F.H.G. II p.342)

This author is mentioned three times by Strabo. Apparently he was a pupil of Xenocrates, who followed Spensippus as head of the Academy from 339-314 B.C. (frg. 3 Müller = Strabo XII.3.22 p.550 C.). Menecrates' floruit

can therefore be placed in the age of the Diadochi.⁽²³⁾ He is mentioned once as the writer of a "Ἑλλησποντιακὴ Περίοδος", cited for information about the Hali-zones, who lived in the mountains behind Myrleia (*ibid.*), and in another place Strabo refers to "Μενεκράτης ὁ Ἐλαίτης ἐν τοῖς περὶ κτίσεων" (frg.1 Müller = Strabo XIII.3.3 p.621 C.). This latter fragment informs us that in early times the Pelasgians had inhabited the entire Ionian coast and adjacent islands from Mycale downwards. A third fragment (frg.2 Müller = Strabo XII.8.3 p.572 C.), is concerned with the Mysians - their origins and the derivation of their name; but we are not told whether this information derives from the *Περίοδος*, or from the *Περὶ κτίσεων*, or from some other work.

10. PHILOCHORUS (F.Gr.Hist.328)

A *Σαλαρινὸς κτίσις* is listed among the works of this author by the Suda (s.v. *Φιλόχορος* = T.1 Jac.), but we know nothing else about it. Here again we are faced with the possibility that the *Σαλαρινὸς κτίσις* was not an independent work at all, but rather a section dealing with Salamis in Philochorus' most famous work, the Atthis. Jacoby seems to have regarded the *Σαλαρινὸς κτίσις* as a separate work, but suggests that it may have been one of a group of "preliminary studies for the Atthis".⁽²⁴⁾

11. CALLIMACHUS

Callimachus' " *Κτίσεις νήσων καὶ πόλεων καὶ μετρονομασίας* " is yet another example of a work known only from a bibliographical list in the Suda (s.v. *Καλλίμαχος*). It is listed among his prose works. The title is interesting because of the word *μετρονομασίας* (cf. the *Ἑθνῶν ὀνομασίας* of Hellanicus) - which reflects the importance of the study of names in the *Κτίσεις* . We know from other fragments - especially from the Aetia - that foundation stories had a particular fascination for Callimachus. We may mention especially the fragment dealing with the foundation of Zancle (P.Oxy. XVIII, 2080; cf. above ^{p.153,} n.2). But to draw conclusions about the content of Callimachus' Ktiseis from such fragments is obviously to beg the question. It is worth observing, however, that a strong aetiological tendency is present in the remains of other Ktiseis, for example those of Apollonius Rhodius, in which the origins of local customs are connected with the events which attended the colonisation of the area.

12. APOLLONIUS RHODIUS (J.U.Powell, Coll.Alex., Oxford 1925, pp.5-7, frgs.4-11)

Apollonius appears to have written a series of epic poems about foundations. We have evidence of the following titles: *Ἀλεξανδρείας κτίσις*, *Καύκου κτίσις*, *Ῥόδου κτίσις*, *Κνίδου κτίσις*, *Ναυκράτews κτίσις*.

The anonymous *Λέσβου κτίσις* cited by Parthenius (Erot.XXI.2) is normally attributed to Apollonius; ⁽²⁵⁾ certainly it contains the same sort of thing as the fragments of Apollonius' Ktiseis.

(a) Ἀλεξανδρείας κτίσις

The one surviving fragment of this poem (frg.4 Powell) concerns the poisonous snakes which were formed from the drops of blood of the severed head of Medusa, as they fell on to the sand during Perseus' flight over Libya. (cf. Apoll. Argon. IV.1513 ff.). The precise connection of this myth with the foundation of Alexandria is a matter for speculation. Perhaps it comes from a description of the site of Alexandria as it was before the city was founded. ⁽²⁶⁾

(b) Ναυκράτειας κτίσις

Our information about this poem comes from Athenaeus VII 283e-284a - a passage which actually quotes six lines of verse (frgs.6,7 and 8 Powell). They are taken from a passage dealing with Apollo's love for the Samian nymph Ocyrhoe, and with Pompilus, a sailor from Miletus who, while attempting to ferry Ocyrhoe from Miletus to Samos, was changed into a fish by Apollo. Again we can only speculate how this story was related to the founding of Naucratis; but it is to be noted that Strabo (XVII. 1.18 p.801 C.) makes Naucratis a Milesian foundation. In Herodotus' account, the Greeks who settled in Naucratis

in the reign of Amasis included some Milesians, who built a temple to Apollo in the city (II.178).

(c) Ῥόδου κτίσις

Steph. Byz. s.v. Δώτιον· πόλις Θεσσαλίας....
 τὸ Θηλυκὸν Δωτηΐς καὶ Δώτιος.... Ἀπολλώνιος ὁ
 Ῥόδιος ἐν Ῥόδου κτίσει. "Ὅσα τε γαίης
 ἔργα τε Δωτιάδος πρότερον κάνον Αἰγυπτίης". (fr.10
 Powell)

These lines make no sense as they stand, and the text is presumably corrupt. The Haemoniaaeans are unknown. It is clear, however, that the fragment in some way refers to the town of Dotium in Thessaly. This town was probably connected with the Rhodian foundation legend through Triopas, who expelled the Pelasgians from the Dotian plain and subsequently founded Cnidus (Diod. V.61.1). According to the Megarian writer Dieuchidas, (27) Triopas' followers quarrelled among themselves after his death: some, under the leadership of Phorbas, founded Ialysus, and others, led by Periergus, founded Camirus; while a third group "returned to Dotium":

" τῶν μὲν εἰς τὸ Δώτιον ἀναχωροῦντων ".

The reference to Dotium in this obscure fragment of Apollonius' Ῥόδου κτίσις probably indicates, therefore, that the poem contained a version of this Triopas legend, in which the Rhodians came originally from Dotium.

Another fragment of Apollonius (frg.11 Powell), which gives an aetiological account of why the Rhodians

offer unburnt sacrifices to Athena, is probably taken from the *Ῥόδου Κτίσις*

(d) Κνίδου Κτίσις

The only fragment that survives from the *Κνίδου Κτίσις* concerns the etymology of the Thracian town of Psykterios, so called because it was there that Heracles wiped the sweat from his body (*ἀναψύχω* = to cool down) after his defeat of Adramyles (Steph. Byz. s.v. *ψυκτήριος* = frg.6 Powell). We do not know how this information is to be connected with the foundation of Cnidus.

(e) Καύνου Κτίσις

Parthenius twice acknowledges Apollonius as one of his sources for his Erotica. He refers once to a *Καύνου Κτίσις* (Erot. XI), and once simply to the Caunus (Erot. I). The first of these references relates to the story of Byblis, who fell in love with her brother Caunus. He rejected her advances and fled from his home in Miletus to the country of the Leleges, where he founded the city named after him. This, says Parthenius (Erot. XI.3), is the version of most authors (*"οἱ πολλοί"*), presumably including Apollonius. The other place in which Parthenius claims to have used Apollonius is in the Romance of Lycus (Erot. I). The latter was an emigré from Argos who married the daughter of Aegialus, king of Caunus, and received part

of the kingdom as a dowry.

Most editors add a third fragment to the *Καύνου Κτίσις* (or perhaps to the *Κνίδου Κτίσις*) - a passage consisting of the following lines:

" Οὐδ' ἵπποι ὀρθρινὰ κατὰ κλισίας χρενέθεσκον
οὔτε βόες πληρεσσιν κατηνάζοντο κάπησιν
νηυσὶ δὲ πρηκτὰ κέλευθα χελιδονίης ἀπὸ πέτρης
Πληγάδας ἀξείνους καὶ ὄπου φινῆια δόρπα,
Ἄρπυιαὶ ἄτλητον ἐπὶ ψῶαν πνεΐεσκον."

frg.5 Powell = Anecd. Par. IV. p.16 Cramer.

Two things can be said about this text; first, that it is almost certainly a fragment of Apollonius⁽²⁸⁾ and secondly that it is not from the Argonautica. It must therefore be from some other poem of Apollonius. Its attribution to the *Καύνου* or *Κνίδου Κτίσις* is based on the conjecture that it refers to the Carians.⁽²⁹⁾

(f) Λέσβου Κτίσις

The anonymous *Λέσβου Κτίσις* cited by Parthenius (Erot. XXI) is probably correctly ascribed to Apollonius. We have seen that Parthenius took some of his stories from Apollonius' Ktiseis, and the character of the fragment is consistent with the rest of the evidence for Apollonius' Ktiseis.

The fragment relates the story of Pisidice, a daughter of the king of Methymna, who fell in love with Achilles

as he was ravaging the island. She betrayed her city to him, but was subsequently stoned to death by Achilles' men.

The fragments we have been discussing leave little room for doubt about the general character of Apollonius' Ktiseis. For the most part the evidence speaks for itself. Two features are especially prominent: first, that the Ktiseis contained a good deal of erudite information, as is shown particularly by the aetiological fragments (e.g. 6 and 11 Powell), and secondly that they took the form of light romances, in which all kinds of fanciful and mildly erotic stories from mythology were woven into an account of foundations of cities. These romantic tales seem to have ranged freely, and to have had little direct bearing on the basic elements of foundation stories as such. (30)

13. DIONYSIUS OF CHALCIS (Müller F.H.G. IV p.393 f.)

We possess at least 13 fragments of the Ktiseis of Dionysius of Chalcis, a work which originally filled five books. This we learn from Pseudo-Scymnus, who mentions among his sources

" Ἐφόρω τε καὶ τῷ τὰς κτίσεις εἰρήκοτι
ἐν πέντε βίβλοις Χαλκιδεῖ Διονυσίῳ ". (31)

One of the most difficult problems concerning this

author is his date. E. Schwartz,⁽³²⁾ following Müller, placed him in the fourth century, because Strabo lists him between Scylax of Caryanda and the Hellenistic poets Euphorion and Alexander of Aetolia.⁽³³⁾ But this method of arguing is unreliable, because there is no guarantee that Strabo was enumerating these authors in chronological order. As A. Baumstark argued,⁽³⁴⁾ the only secure dating criterion is the terminus ante quem provided by later writers who can be shown to have used Dionysius. The earliest of these is probably Pseudo-Scymnus, whose work was composed at the beginning of the first century B.C. But it is probable, as Baumstark suggested, that Strabo (loc.cit.) only knew of Dionysius through Demetrius of Scepsis, who was writing c.170-150 B.C. We thus have a terminus ante quem for Dionysius of Chalcis of the middle of the second century B.C.⁽³⁵⁾

In some of the fragments Dionysius is only cited as evidence for the location of a place, or the form of a name, or something of that kind (e.g. frgs.2,2a,7 etc.). We can see that he studied the genealogies of gods and heroes (3,4,6,11) and the origins of names (1,3,4,7,9,10,12), as well as foundations, origins of peoples, and related matters (1,2a,4,10,11). In frg.5 we are given part of the foundation-oracle that was offered to Meleus, the leader of the Pelasgians. Most of the fragments,

however, are brief extracts which give little indication of the character of Dionysius' Ktiseis. But a predilection for antiquarian research is revealed by the obscure and eccentric variants presented in some of the fragments - e.g. on the parentage of Achilles (frg.6) and possibly in the unusual version of the ancestry of Rhomus hinted at in frg.11.⁽³⁶⁾ There is also perhaps some reason to think that he, like Apollonius Rhodius, used the foundation legends as a framework within which to recount novelistic and fanciful stories (frgs.1,3,4 etc.).

14. DIOCLES OF PEPARETHOS (F.Gr.Hist.820)

Plutarch tells us that Diocles of Peparethus was the first author to put before the Greek public a version of the canonical account of the foundation of Rome. (Plut. Rom. 31 = F.Gr.Hist. 820 T.2a). The notorious "Diocles-problem" concerns the precise relation of Diocles' version to that of Fabius Pictor who is said by Plutarch to have followed Diocles in most essentials (ἐν τοῖς πλείστοις). This question has already been touched on in an earlier chapter⁽³⁷⁾ and need not detain us here. The points which seem relevant to the present discussion are as follows:-

(i) The date is fixed by Plutarch, who makes it clear that Diocles was earlier than Fabius Pictor; this is not inconsistent with the statement of Athenaeus

(II.22 p.44 E = T.2b Jac.) that Diocles was mentioned by Demetrius of Scepsis, writing in the first half of the second century B.C.

(ii) When Plutarch describes Diocles as "ὁ δοκεῖ πρῶτος ἐκδοῦναι Ῥώμης κτίσιν" (Rom. 8,9 = T.2b Jac.) it is by no means certain that he was referring to a work concerned solely with the foundation of Rome and entitled "Ῥώμης κτίσις" - although Jacoby seems to have inferred that he was. Plutarch's words could equally well indicate that Diocles' account of the founding of Rome occurred in some more general work of unknown scope.

It is of course impossible to determine exactly which elements of Plutarch's account of the birth of the twins (Rom. 4-8) should be attributed to Diocles. (38)

15. POLEMON OF ILION (39)

Polemon of Ilion was an antiquarian who flourished in the first half of the second century B.C. This date comes from the Suda (s.v. Polemon), where we are told that he lived in the reign of Ptolemy Epiphanes and was a contemporary of the grammarian Aristophanes of Byzantium. This is confirmed by the evidence of Dittenberger Syll.³ 585,114, which records that Polemon of Ilion was proxenos at Delphi in 177-6 B.C.

An enormously learned man, Polemon was praised for his erudition by Plutarch (Quaest. Conv. V.2.9), and was

nicknamed "στηδοκόπας" because of his interest in monuments and inscriptions (Athen. VI.234d.). He was usually known, however, as "the Periegete". Of his works, which survive only in meagre fragments, we know some thirty five titles.⁽⁴⁰⁾

But the classification of these works is difficult. It is clear enough that the 'Αντιγραφαί - polemical works attacking earlier writers such as Timaeus and Neanthes of Cyzicus - form a separate group (Preller, op.cit. (n.43) p.69 ff.) as do the letters (Preller p.107 ff.); otherwise one has to try to differentiate between miscellaneous works which cannot be placed in any particular category (Preller p.123 ff.) and the large group of titles which come together under the general heading of Περιήγησις κοσμική (Preller p.22 f., 34 ff.).

Our main concern of course is with the Ktiseis, of which we are given several titles: here the question is whether they are to be regarded as an independent group on their own, as Susemihl thought (op.cit. I.670), or whether they should be taken together with works on local monuments, paintings, the treasures of Delphi, the Acropolis at Athens, etc., which constitute the Περιήγησις κοσμική.⁽⁴¹⁾ Unfortunately this question cannot be answered because there is no way in which the precise character of Polemon's Ktiseis can be judged: once again we find that our sources

of information dry up at the crucial point. We know almost nothing at all about the Ktiseis beyond the bare titles. These are as follows:

(a) Κτίσεις τῶν ἐν Φωκίᾳ πόλεων καὶ περὶ τῆς Ἀθηνάων συγγενείας αὐτῶν.

This work is referred to only by the Suda. There are no fragments, but the title itself, with its mention of the relationship between the Phocian cities and the Athenians, is most remarkable, and recalls the passage of Pausanias X.35.8, which mentions the Athenian origin of the inhabitants of the city of Stiris;⁽⁴²⁾ but it is to be noted that there are no good grounds for Preller's view (50, 181 ff.) that Pausanias borrowed extensively from Polemon.⁽⁴³⁾

(b) Κτίσεις τῶν ἐν Πόντῳ πόλεων.

Here again we know nothing more than the title, preserved by the Suda.

(c) Κτίσεις Ἰταλικῶν καὶ Σικελικῶν πόλεων

This work is referred to by Schol. Apollon. Rhod. Argon. IV.324 (= frg.37 Preller): "ὁ δὲ Καυδιακὸς σκόπελος τῆς Ἐκιοθίας, πλησίον τοῦ Ἰστροῦ· οὐ μνημονεύει Πόλεμων ἐν κτίσει Ἰταλικῶν καὶ Σικελικῶν (sc. πόλεων).

There seems to be no obvious answer to the question of how Polemon came to mention the "Καυδιακὸς σκόπελος" in a work on the foundation of Italian and Sicilian cities.

Polemon is cited by Festus as the author of an eccentric account of the origin of the Salii at Rome. They were so named, he believed, because they had been instructed in warlike leaping by a certain Arcadian called Salius, who had been brought to Italy from Mantinea by Aeneas (Fest. s.v. Salios). Whether this absurdity is to be regarded as a fragment of the κτίσεις Ἰταλικῶν καὶ Σικελικῶν πόλεων as Preller thought (p.69, frg.38) is an open question.

Finally we may take note of a fragment, unknown to Preller, which undoubtedly derives from the κτίσεις Ἰταλικῶν καὶ Σικελικῶν πόλεων :

ΕΤΥΜ. MAGN. s.v. "Μεδιταῖα κυνίδια, λέγεται, ὅτι πλησίον Ἰταλίας νησὸς ἐστὶ Μεδίτη, ἐξ ἧς εἶσι Μεδιταῖα κυνίδια. Πόλεων δ' ἕκτισε Σικελίας (= ἐν κτίσει Σικελίας - Deichgräber RE XXI. 1301) κελύτης φησὶν εἶναι.⁽⁴⁴⁾

16. DEMOSTHENES OF BITHYNIA (F.Gr.Hist.699)

Demosthenes is normally placed in the third or second century B.C.,⁽⁴⁵⁾ but there is no proof of this, and a considerably later date, perhaps even the period of the early empire, cannot be excluded.⁽⁴⁶⁾ He is best known as the writer of a lost epic poem, called Βιθυνιακὰ . The title Ktiseis is attested only once,⁽⁴⁷⁾ and the fact that foundations of cities were mentioned in the Bithyniaka (Steph. Byz. s.v. Βιθυνιακός , = F. Jac. etc.), might

suggest that Ktiseis was an alternative title for that work. (48) This seems unlikely, however, in view of the fact that the one fragment of the Ktiseis refers to a town in Thessaly:-

Steph. Byz. s.v. Ὀλιζαίων, πόλις Ἡετταλίας.....
 ὠνομάσθη δὲ ἀπὸ τοῦ Νικρά εἶναι. Θετταδοὶ γὰρ
 ὡς ἱστορεῖ Δημοσθένης ἐν κτίσει, τὸ Νικρὸν ἄλιον καλοῦσι.

It cannot be absolutely certain whether these Ktiseis were in prose, (49) or in verse like the Bithyniaka - and therefore comparable to the Ktiseis of Apollonius of Rhodes. B. Schmid stated that the words of the fragment - τὸ Νικρὸν ἄλιον καλοῦσι - fit into the last three and a half feet of a hexameter line (op.cit. p.88); but this is untrue, as the last syllable of ἄλιον must scan long before καλοῦσι .

.....

(ii) Appendix: The Date of Hippias of Rhegium

(a) Evidence that Hippias was a writer of the fifth century B.C.

The only explicit evidence we possess concerning the date of Hippias is given in the biographical notice in the Suda (T.1 Jac.), where it is stated that he flourished in the first half of the fifth century B.C. ("γεγονώς ἐπὶ τῶν Περσικῶν "). We are informed by the same source that Hippias' work "Σικελικαὶ πράξεις" was

subsequently epitomised by a certain Myes. We know nothing more about this Myes, but as the name is extremely rare it is possible that he was identical with the Myes who is mentioned as a follower of Pythagoras.⁽⁵⁰⁾ The Pythagorean Myes was a citizen of Posidonia, and he is therefore to be dated probably before the middle of the fourth century at the latest, because Posidonia fell to the Lucanians at the end of the fifth century.⁽⁵¹⁾

A more positive terminus ante quem for Hippys is provided by the statement of Plutarch that he was mentioned by Phantias (or Phaenias) of Eresus, a pupil of Aristotle, who was writing at the end of the fourth century.⁽⁵²⁾

Finally, Stephanus of Byzantium cites Hippys as the first author to have called the Arcadians "Proselenoi";
"*Ἰππύς δ' ὁ Πηγιῶς λέγεται πρῶτος καλεῖσθαι προσελήνους τοὺς Ἀρκάδας*" (F.7 Jac.). The notion that the Arcadians had existed before the creation of the moon was widespread among Greek authors,⁽⁵³⁾ and was certainly well established by the time of Aristotle, who rationalised it.⁽⁵⁴⁾ Since Hippys is described as the first to have advanced this theory, it follows that the source of Stephanus of Byzantium regarded him as a very early writer - a point already noticed by Wilamowitz.⁽⁵⁵⁾

(b) Evidence that Hippys was a writer of the third century B.C.

The arguments that suggest a late date for Hippys

are based for the most part on the interpretation of the internal evidence of the fragments themselves. It is believed that for one reason or another several of the surviving fragments cannot have been written by an author of the fifth century B.C. A number of considerations have been advanced in support of this view, some more cogent than others. I shall discuss only the more important of these arguments, referring in a note to those which seem to me to be obviously mistaken or inconclusive.⁽⁵⁶⁾

We may begin with an argument which was propounded most forcefully by L. Pareti. A scholiast on Apollonius of Rhodes (IV.257) informs us that Hippys spoke of the Egyptians as the earliest race of men (F.6 Jac.). The text is corrupt, but the general sense becomes clear when the fragment is compared with a parallel passage from Diodorus. In fact the similarity of the two passages extends even to the wording:-

HIPPIYS F.6 Jac.:-	DIODORUS I.10.1:-
<p>Ἱππυς δὲ τοὺς Αἰγυπτίους πρώτους στοχάσασθαι τῆς τοῦ ἀέρος κρᾶσεως καὶ γονιμώτατον εἶναι τὸ τοῦ Νείλου ὕδωρ.</p>	<p>φασι τοίνυν Αἰγυπτίους κατὰ τὴν ἐξ ἀρχῆς τῶν ὄλων γένεσιν πρώτους ἀνθρώπους γένεσθαι κατὰ τὴν Αἰγύπτου διὰ τε τὴν εὐκρασίαν τῆς χώρας καὶ διὰ τὴν φύσιν τοῦ Νείλου.</p>

Since this passage of Diodorus - and indeed almost the whole of Diodorus' first book - is based directly on

the work about Egypt by Hecataeus of Abdera,⁽⁵⁷⁾ Pareti inferred that the latter must also have been the source of Hippys. Hecataeus of Abdera's work on Egypt was written in the last decades of the fourth century, almost certainly during the years 320-315 B.C.;⁽⁵⁸⁾ it therefore follows that Hippys F.6 Jac. must have been written some time after that.⁽⁵⁹⁾

But there is no reason to suppose that the facile notion expressed in these texts originated with Hecataeus of Abdera. The fact that the Egyptians themselves are cited in support of the statement that Egypt was the home of the first men is no guarantee that Hecataeus had consulted the Egyptian priests directly,⁽⁶⁰⁾ or that he was the first to publicise the native Egyptian tradition among the Greeks. On the contrary, it is evident that this Egyptian tradition was widely known to the Greeks already in the fifth century, because Herodotus attempted to refute it in his account of Psammetichus' famous experiment with the children (Hdt. II.2).

The comment about the temperate climate (Diodorus' *εὐκρασία*) and the fertility of the land, as factors providing favourable conditions for life, was a common-⁽⁶¹⁾ place in ethnographical writing, particularly about Egypt. The similarity between Hippys of Rhegium and Hecataeus of Abdera need not imply, therefore, that the one was directly

dependent on the other; a perfectly tolerable alternative is that they both drew on the same source. In other words, F.6 does not necessarily mean that Hippys was later than Hecataeus of Abdera; consequently it does not give any indication of the date of Hippys.

A rather more persuasive argument was offered by F. Jacoby, who suggested that the title "Chronica"⁽⁶²⁾ was totally unsuitable for a work of the fifth century B.C. This contention may not be absolutely compelling on its own,⁽⁶³⁾ but it is certain that no writer earlier than the third century B.C. could have given the synchronism attributed to Hippys by Antigonus of Carystus (F.3 Jac.):

" φησὶν ἐν Ἀθήναις ἐπὶ βασιλῆως Ἐταίνετου, ὀλυμπιάδος ἑκτης καὶ τριακοστῆς, ἐν ἣ Ἀρυτάρκας Λάκων νικᾷ σταδίου, τῆς Σικελίας ἐν Παιδίκοις οἰκοδομηθῆναι τόπων.... " etc.⁽⁶⁴⁾

(c) Synthesis

The question is, how are we to reconcile these apparently contradictory items of evidence? The most radical explanation is clearly that of Jacoby, who simply dismissed Hippys as a forgery. That is to say, he argued that Hippys never existed, and that the fragments attributed to him were in fact taken from books written in the fourth or third century B.C. and passed off as the work of a fifth century historian. As for the identity of the forger, the shadowy figure of Myes, the "alleged epitomator", comes

readily to hand.⁽⁶⁵⁾ But this is not the only possible explanation of the discrepancy between the internal evidence of the fragments and the testimony of later critics. It is conceivable, as Pareti argued,⁽⁶⁶⁾ that Hippys was a bona fide writer of the third century whom later scholars placed in the fifth century as a result of an honest error. Jacoby himself offered the conjecture that Hippys was dated to the fifth century because the period covered in his work may only have extended as far as the age of the Deinomenids.⁽⁶⁷⁾ However that may be, it is clearly possible that the Suda is in error; the article on Hippys already contains at least one obvious blunder,⁽⁶⁸⁾ and the source of Stephanus of Byzantium (F.7) may not have had access to information independent of the scholarly tradition reflected in the Suda.

But Jacoby and Pareti are in agreement when they argue that the extant fragments derive from a work which cannot have been earlier than the age of Timaeus and Eratosthenes, in view of the Olympiad-date in F.3. Whether this work was genuine or a forgery is secondary to the basic problem of its date.

But there is a decisive objection to the view that our fragments come from a writer of the third century B.C. As De Sanctis realised, the statement that Hippys was mentioned by Phanias of Eresus (F.5) proves that a work

under the name of Hippys existed already in the second half of the fourth century.⁽⁶⁹⁾ Jacoby's attempt to date the forgery to the fourth century rather than the third is an unhappy compromise, because the Olympiad-date in F.3 (on which everything depends) excludes the fourth century no less than the fifth. In sum, it must be acknowledged that the chronological statement in F.3 cannot have appeared in a work known to Phaneas of Eresus.

If Plutarch has given a reliable account of what Phanas wrote (and there is no reason to believe that he has not), there remain two possible explanations of the discrepancy. First, it is surely possible that Hippys was a bona fide writer, but that the works attributed to him in the Hellenistic period included some forgeries in addition to genuine items. This, if I have understood it correctly, was the view of Wilamowitz.⁽⁷⁰⁾ Secondly, it has been plausibly suggested that the synchronism in F.3 did not occur in the original text of Hippys, but was a later interpolation, perhaps by Myes, perhaps by an unknown editor.⁽⁷¹⁾ It is a fact that Olympic dates were sometimes interpolated into early historical works, as the example of Xenophon's Hellenica shows (I.2.1, II.3.1, etc.). Clearly, if either of the conjectures offered here is correct, the lateness of any one fragment cannot be said necessarily to prove the lateness of the rest.

Although the evidence discussed here has demonstrated that all or part of at least one work of Hippys was by a later hand, it has also shown that the other fragments should probably be attributed to a genuine historian of the fourth century or earlier. The date of the real Hippys must of course remain uncertain in view of the general unreliability of the Suda. But there is no conclusive argument to prove that the Suda is wrong in the present instance. (72)

(iii) Notes to Chapter XI

- (1) F. Jacoby, F.Gr.Hist. III B Kommentar, p.296; id., Atthis, p.364 n.62.
- (2) See e.g. C. M. Bowra, Early Greek Elegists, p.110 f.
- (3) In spite of the assertions of O. Immisch, "Philologus" 47, 1890, p.208.
- (4) G. Crönert, "Charites F. Leo", 1911, p.139; F. Jacoby, Abhandlungen zur griechischen Geschichtsschreibung, p.149 n.27; id., Atthis, p.364 n.62 etc.
- (5) O. Immisch, loc.cit. (N.3), followed by H. Fränkel, "Hermes" 60, 1925, p.174 ff. In general see B. Schmid, Studien z. griech. Ktisisagen, pp.24-36; F. Jacoby, F.Gr.Hist. III B Komm., p.296.
- (6) See F. Jacoby, "Klio" 9, 1909, p.115 f. (= Abhandlungen p.55 f.); id., R.E. s.v. "Kadmos" (no.6) 1473-6; id., F.Gr.Hist. III B Komm., pp.402-3; III B Suppl.I, pp.610-11.
- (7) F. Jacoby, R.E. s.v. "Hippys", 1927.
- (8) Athenaeus calls it "Περὶ Χίου" (F.2) and Pausanias "συρραφῆ" (F.1).
- (9) Diehl, R.E. s.v. "Ion von Chios", col.1864; W. Schmid, Gesch. d. griech. Lit. I.2, p.514.
- (10) See especially F. Jacoby, Some Remarks on Ion of Chios, "Class.Quart." 41, 1947, p.4 f. (= Abhandlungen p.149 f.) following A.von Blumenthal, Ion von Chios, 1939, pp.17-18, 23.
- (11) B. Schmid. Studien z. griech. Ktisisagen, 45-7.
- (12) Hist. Anim. XVI.39: Blumenthal, op.cit. (n.10), pp.16-17, F.17.
- (13) Cf. H. Färber, "Phil.Wochenschr." 1940, 195-6. For a general discussion of Ion's Ktisis see B. Schmid, Studien... etc. pp.43-52.

- (14) Abhandlungen, p.150 n.28. Contrast Atthis p.364 n.62 where he reverts to the view originally expressed in R.E. VIII (1913) s.v. "Hellanikos", col.136-7.
- (15) Jacoby, R.E. VIII, 114.
- (16) The central problems are discussed by L. Pearson, Early Ionian Historians, Oxford, 1939, 193 ff.
- (17) Cf. Jacoby, F.Gr.Hist. I, (Komm.) p.454.
- (18) R.E. s.v. "Charon" (no.7) col.2180.
- (19) See F.Gr.Hist. III A Komm. p.4, and Charon von Lampsakos, in "Stud. Ital. Fil. Class." n.s. 15, 1938(1939) p.218 (= Abhandlungen p.186).
- (20) Non posse suaviter vivi sec. Epic., ch.10 = Aristotle, fragmenta p.259 Rose.
- (21) Cf. frgs. 484,485,504,546,547 Rose, - which illustrate that Aristotle touched on the subject of foundations of cities in his Politeiai.
- (22) Jacoby F.Gr.Hist. III B Komm. p.411.
- (23) Cf. Christ-Schmid-Stählin, Griech. Literaturgesch. II,⁶ p.242 n.6.
- (24) Jacoby, Atthis, p.291 n.11.
- (25) J. U. Powell, Coll. Alex. p.7, on frg.12.
- (26) Wilamowitz, Hellenistische Dichtung, II p.256.
- (27) ap. Athen. VI.262, = F.Gr.Hist. 485 F.7.
- (28) Tzet. ad Lyc. 1285 quotes the fourth line as a line of Apollonius.
- (29) Cf. J. U. Powell, op.cit. on frg.5, following Meinecke, Analecta Alexandrina 402-3; B. Schmid, Studien z. griech. Ktisissagen p.82.
- (30) B. Schmid. op.cit. p.53.
- (31) Ps. Scymn. 115 Müller; cf. Harpocration s.v. ἡπταίωσιν τετραίωσιν, referring to the fifth book

of Dionysius' Ktiseis (= frg.3).

- (32) R.E. V s.v. "Dionysius" (no.107) 929.
- (33) Strabo XII.4.8 p.566 C = Dionys. Chalc. frg.7.
The fourth century date is accepted also by B. Schmid, op.cit. 92-3; F. Jacoby, marginal note on F.Gr.Hist.840 F.10; H. Strasburger, Zur Sage von der Gründung Roms, "Sb.Heidelb.Akad." 1968, p.12.
- (34) "Philologus" 53, 1894, 703 ff.
- (35) A. Baumstark, art.cit.; cf. J. Perret, Les Origines de la légende troyenne de Rome, 1942, 387-8; C. J. Classen, "Historia" 12, 1963, 451 n.22, and "Gnomon" 1971, p.480.
- (36) Dion. Hal. I.72.6 = F.Gr.Hist.840 F.10.
- (37) See above, p. 58 and n.13.
- (38) Cf. C. J. Classen, "Historia" 12, 1963, 454 n.38. A clear statement on the limitations of our knowledge of Diocles is given by A. Momigliano, Secondo Contributo, 403.
- (39) L. Preller, Polemonis Periegetae Fragmenta, Leipzig 1838; Müller, F.H.G. III p.108 ff.
- (40) Listed in the excellent article by K. Deichgräber in R.E. XXI, s.v. "Polemon" (no.9), 1288-1320; cf. Preller, op.cit. 18-20; F. Susemihl Gesch. griech. Lit. i.d. Alexandrinerzeit I, 665-676; Christ-Schmid-Stählin, Gesch. griech. Lit. II⁶ p.243^f.
- (41) Thus Preller, op.cit. 18-19; cf. Deichgräber, R.E. XXI, 1291-2.
- (42) Preller, op.cit. 54; Deichgräber art.cit. 1299.
- (43) See the detailed refutation by J. G. Frazer in Pausanias' Description of Greece, 1898, I p.lxxxiii ff.
- (44) M. E. Miller, Mélanges de littérature grecque, p.213.
- (45) Christ-Schmid-Stählin, Gesch. griech. Lit. II⁶ p. 321; Susemihl, op.cit. I, p.404.

- (46) E. Schwartz, R.E. s.v. "Demosthenes" (no.),
188 f.
- (47) Steph. Byz. s.v. Ὀλιγύων = F.Gr.Hist. 699 F.10.
- (48) B. Schmid, Studien z. griech. Ktisissagen,
p.88 n.1.
- (49) As J. U. Powell thought (Coll.Alex. p.26).
- (50) Iambl. Vit. Pyth. 267. Cf. Bux, R.E. s.v.
Myes (no.1); Von Fritz, R.E. s.v. Myes (no.2)
etc. The identification is made more probable
by the fact that Hippys can be shown independently
to have been influenced by Pythagorean doctrines.
(F.5 Jac.).
- (51) G. De Sanctis, Ricerca sulla storiografia
siceliota (ΣΙΚΕΛΙΚΑΙ), Palermo 1957, p.2.
But Pareti was right to point out that a later
date for Myes is not out of the question (L'opera
e l'eta di Hippys di Regio, "Riv. di cultura class.
e med." I, 1959, p.110). It is not to be supposed
that the Greek and Etruscan populations of the cities
of Campania were entirely wiped out when they fell
into the hands of Sabellian tribes at the end of
the fifth century - although our hostile sources
would have us believe that this was the case (e.g.
Liv. IV.37.1). A better account is given by Strabo,
who records (V.4.7 p.246 C) that the list of magis-
trates at Neapolis continued to include Greek names
mixed with the Campanian ones - and that the city
largely retained its Hellenic character.
- (52) Plut. de def. orac. 23, p.422d-e = Hippys
F.5 Jac.
- (53) Including, interestingly, Dionysius of Chalcis,
in frg. 1 of the Ktiseis (Müller, PHG IV, p.393).
- (54) Frg. 591 Rose; cf. Ernst Meyer, R.E. s.v.
"Proselenoi", 844-5; Jacoby, F.Gr.Hist. III B
Kommentar, p.485 and n.40.
- (55) Wilamowitz, Hippys von Rhegium, "Hermes" 19,
1884, 447-8.
- (56) Jacoby tried to make capital out of the fact
that Hippys was not mentioned by Strabo or Dionysius

of Halicarnassus (F.Gr.Hist. III B Komm. p.482. followed by K. J. Dover in Gomme-Andrewes-Dover, Commentary on Thucydides vol. IV, Books VI-VII, Oxford 1970, pp.199-200). The weakness of such an argument is obvious. Indeed, W. Schmid used the same evidence - or lack of it - to support exactly the opposite conclusion; in other words, Dionysius' failure to mention Hippys was held to indicate that he was an ancient writer whose work had been superseded by more up-to-date historians (Schmid-Stählin, Gesch. d. griech. Lit. I, 2, 1933, p.703). In general see R. van Compernelle, Etude de chronologie et d'historiographie siceliotes, Brussels 1959, p.443; F. W. Walbank, The Historians of Greek Sicily, "Kokalos" 14-15, 1968-9, p.478 n.12. In the same way, scholars have disagreed radically on what inferences should be drawn from a comparison of Hippys F.2 Jac. and an inscription recording the same story (I.G. IV² 1, 121-4; the story concerns a woman who was miraculously cured of a tape worm). De Sanctis argued that Hippys' version is the more rational and coherent, and therefore the earlier, of the two; but this seems a rather questionable assumption. Wilamowitz, for one, thought that the story in the inscription "besser und ursprünglicher ist als bei Hippys" (art.cit. p.449). In any case, as Walbank pointed out (477-8), Hippys' version may simply be drawn from a better source.

- (57) See now the important discussion by Oswyn Murray: Hecataeus of Abdera and Pharaonic Kingship, "Journ. Egypt. Arch." 56, 1970, 141-171 (with bibliog.).
- (58) Murray, art.cit. p.144.
- (59) Pareti, art.cit. p.106-7.
- (60) Cf. the remarks of Murray, art.cit. p.151 f.
- (61) Cf. especially Justin II.1.5, and the references cited in T. Cole, Democritus and the sources of Greek Anthropology, 1967, p.180 n.13.
- (62) This title is mentioned in the Suda (T.1 Jac.) and is confirmed by Zenobius, Prov. 3.42: "Ἰερί Χρόνῳ" (= F.1 Jac.).
- (63) See the remarks of DeSanctis, op.cit. (n.2) p.5.

- (64) The regular use of Olympic dating in historiography was first introduced in the early third century B.C., probably by Timaeus (Polyb. XII. 11.1) - see Jacoby, F.Gr.Hist. III B Suppl. I, p.382 ff; T. S. Brown, Timaeus of Tauromenium, p.10 ff; F. W. Walbank, A Historical Commentary on Polybius II, 1966, p.347 f.
- (65) Jacoby, F.Gr.Hist. III B Komm. p.382 ff. cf. id., R.E. s.v. Hippias, 1927 ff; K.von Fritz, Die griech. Geschichtsschreibung I, Berlin 1967, Anmerkungen 238 n.136.
- (66) Art.cit. (n.2), pp.110-111. Pareti did not argue "that Hippias is a late forgery", as Walbank thinks (art.cit. p.478 n.13).
- (67) Cf. Pareti, p.111. Nonetheless, I agree with DeSanctis (op.cit. p.2) that this hypothesis is very unlikely.
- (68) The Suda article ends with the words: "οὗτος πρῶτος ἔγραψε παρωδίαν καὶ χωταίμβον καὶ ἄλλα" - which refers not to Hippias but to Hipponax. Cf. Wilamowitz, art.cit. (n.6) p.444.
- (69) DeSanctis, op.cit. pp.1-2.
- (70) Wilamowitz, art.cit. 444-452; F. Susemihl, Gesch. d. griech. Lit. i. d. Alexandrinerzeit, II p.29.
- (71) E.g. Jacoby Atthis p.307 n.44; De Sanctis op.cit. p.2; R.van Compernelle Etude de chronologie... etc. (cit. n.56) p.442; Walbank, art.cit. p.478.
- (72) De Sanctis (p.8) attempted to find support for the fifth century date in the internal evidence; he inferred from the fact that the fragments show a predilection for digressions, marvels and romantic stories, that Hippias wrote history in the manner of Herodotus. Cf. Walbank, art.cit. p.478 n.11; E. Manni, Da Ippi a Diodoro, "Kokalos" 3, 1957, 136-7. But such features are obviously not exclusively characteristic of fifth century writers. Nonetheless, De Sanctis certainly succeeded in showing that the character of the fragments is not incompatible with the early date assigned to Hippias by the Suda. There is no need to discuss the eccentric theory

of Wilamowitz, that Hippys was a contemporary of Thucydides, writing at the end of the fifth century B.C. This is based on the conjecture (it is no more than a conjecture) that in the Argolica (see T.1 Jac.), Hippys will have used the work on the priestesses of Argos by Hellanicus of Lesbos. (art.cit. p.444).

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