AN INTERIM REPORT ON THE ORIGINS OF ROME

By ARNALDO MOMIGLIANO

INTRODUCTION

With three volumes of E. Gjerstad's *Early Rome* still outstanding we are exactly in the middle of the new phase of the research on the origins of Rome. Other works due to appear very soon (perhaps they will already have appeared by the time these pages are published) include A. Alföldi's T. S. Jerome Lectures on *Early Rome and the Latins*, R. Werner's *Der Beginn der römischen Republik* and a new volume by H. Müller-Karpe, the author of *Vom Anfang Roms* (Heidelberg, 1959). Furthermore, historians and archaeologists most actively engaged in this field of research, such as M. Pallottino, R. Bloch, R. Peroni and P. Romanelli, have still much to contribute. In these circumstances any attempt to draw conclusions is clearly premature. But Gjerstad, Alföldi and Bloch have already presented their theories in outline [1] with the explicit or implicit purpose of having them discussed before they are given their final shape. The present paper, which represents two of the three J. H. Gray lectures delivered before the University of Cambridge in March, 1963,[2] is intended as a contribution to this preliminary discussion.[3] My objections are offered in the hope that they will ultimately be removed. As in my discussion of the problem of the *Scriptores Historiae Augustae* [4]—and with the same principles of method—I am not committed to any theory. I am trying to distinguish between what seems to me proved and what seems to me not proved. At the same time I am convinced that working hypotheses are indispensable in any subject. In the second part of my paper I shall therefore offer some tentative and speculative suggestions for the social and political organization of Early Rome. Though space does not allow me to discuss in full alternative hypotheses,[5] I hope I may be trusted to have given them careful

[1] E. Gjerstad, 'The Earliest History of Rome', *Acta Congressus Madvigiani* I, 1958, 375–96; 'Discussions concerning Early Rome', *Opuscula Romana* III, 1960, 69–102 (which lists his earlier contributions starting with the paper in *Bull. Comm. Arch. Com.* 73, 1949–50, 13); *Legends and Facts of Early Roman History* (Scripta Minora Regiae Societatis . . . Lundensis), Lund, 1962. Professor Gjerstad had already contributed a fundamental paper on the republican Comitium in *Opusc. Archaeol.* 2, 1941, 97–158. For Gjerstad's absolute chronology cf. *Bull. Paletnol. Ital.* 64, 1954, 295–7.

A. Alföldi summarized his views in *Gymnasium* 67, 1960, 193–6. Later papers: *AJA* 64, 1960, 137–44; *Studi e Materiali Storia Rel.* 32, 1961, 21–39; *Antidoron E. Salin*, 1962, 117–36; *Röm. Mitteil.* 68, 1961, 64–79.

R. Bloch, *The Origins of Rome*, Engl. transl. with additions, London, 1960, must be supplemented by various papers of which the most important seem to me the following: 'Une tombe villanovienne près de Bolsena et la danse guerrière dans l'Italie primitive', *Mél. École Rome* 70, 1958, 7–37; 'Rome de 509 à 475 environ avant J.C.', *REL* 37, 1959, 118–31; 'Le départ des Étrusques de Rome selon l'annalistique', *Rev. Hist. Relig.* 159, 1961, 141–56 (cf. also *CRAI* 1961, 62–71); appendix to Tite-Live, *Histoire Romaine* II, ed. J. Bayet, Paris 1962, 101–133.

P. Romanelli opened the new stage of research: see *Quaderni di Roma* 1948, 381–395.

[2] The first lecture on the literary sources, and especially on Fabius Pictor, used materials to be published in my forthcoming Sather Lectures on *The Classical Foundations of Modern Historiography* ch. IV, and in 'Timeo, Fabio Pittore e il primo censimento di Servio Tullio', *Miscellanea di Studi Alessandrini in memoria di A. Rostagni*, Torino, 1963, 180–7. Cf. my previous papers: *JRS* XLVII, 1957, 104–14 (= *Secondo Contributo* 1960, 69–87); *Riv. Stor. Ital.* 71, 1959, 529–56; *Rend. Acc. Lincei* 8, 15, 1960, 310–20.

[3] cf. my remarks in *Riv. Stor. Ital.* 73, 1961, 802–8, of which *Cultura e Scuola*, no. 2, 1962, 68–74, is only an expanded version; *Rend. Acc. Lincei* 8, 17, 1962, 387–92, and *Maia* 15, 1963, 47–8, for further discussion with Gjerstad and Alföldi. In my paper in *Miscellanea di Studi Alessandrini* I also discuss Alföldi's views on Timaeus.

[4] cf. my *Secondo Contributo*, 1960, 105–43.

[5] Bibl. references are strictly limited and normally confined to very recent literature. Pre-war research is summarized in P. Ducati, *Come nacque Roma*, Rome, 1940, and, better, in E. Ciaceri, *Le origini di Roma*, Rome, 1937. Much of the literary sources is now conveniently collected in G. Lugli, *Fontes ad Topographiam Veteris Urbis Romae* I, 1952 ff., and I. Scott Ryberg, *An Archaeological Record of Rome from the seventh to the second century B.C.*, London–Philadelphia, 1940, is still indispensable. The most recent valuable synthesis is P. de Francisci, *Primordia Civitatis*, Rome, 1959 (cf. the review by A. Bernardi, *Studia et Documenta* 27, 1961, 381–94). M. Pallottino, 'Le origini di Roma', *Archeol. Class.* 12, 1960 (1961), 1–36, is important and gives full bibl.: cf. his paper in *Studi Romani* 5, 1957, 256–68. L. Adams Holland, *Janus and the Bridge*, American Academy in Rome, 1961, touches almost all the problems of Roman origins and is invariably well informed. New discoveries were summarized by B. Andreae, *Arch. Anz.* 1957, 110–358, in an exemplary way. There is also a very useful survey by T. Hackens, *Ant. Class.* 30, 1961, 484–506, and it is interesting to see how the *doyen* of the students on the origins of Rome, A. Piganiol (*Essai sur les origines de Rome*, 1917), has reacted to the more recent developments, *Journ. Savants*, 1960, 18–28. Finally, I have to express my debt to F. Altheim's constant questioning of traditional theories, notwithstanding disagreements with his methods and conclusions: see now especially his *Römische Geschichte* I–II, Frankfurt, 1951–3, and *Untersuchungen zur Römischen Geschichte* I, Frankfurt, 1961. For further information cf. F. Altheim, *Bibliographie seiner Schriften*, zusammengestellt von E. Merkel, which includes publications up to 30th June, 1958 (Frankfurt, 1958).

consideration. From the start I should like to express my admiration for the thoroughness and acumen of E. Gjerstad's epoch-making work.

I. THE VARIOUS TYPES OF EVIDENCE ON EARLY ROME

(i) Let me summarize the situation about our literary sources on Early Rome. The first Roman historians (Q. Fabius Pictor, Cincius Alimentus) lived in the third century B.C. and wrote in Greek. Later in the second and first centuries B.C. (beginning with M. Porcius Cato the Censor) there developed a historiography in Latin. Only short fragments are preserved in quotations and we do not know exactly how these early historians proceeded to collect information about the past of Rome. We can, however, form a more precise idea of their general method from the sections of Diodorus' Bibliotheca which deal with Roman history and from the books of Livy and Dionysius of Halicarnassus that have come down to us. Diodorus probably used Roman historians of the second century B.C.—the theory that he used Fabius cannot be maintained; whereas Livy and Dionysius seem to have relied mainly on Roman historians of the first century B.C. In addition, Dionysius consulted antiquarians such as Varro. For events earlier than the First Punic War the sources of our direct sources were dependent on: (1) a pontifical chronicle (*Annales maximi*) which apparently concentrated on events with religious implications (such as famines and eclipses); (2) popular traditions and traditions of individual aristocratic families and religious corporations (*fratres arvales*):[6] these traditions were supported by inscriptions, written funeral speeches, data of the census (banquet songs existed but do not seem to have played an important part in transmitting historical facts); (3) official lists of magistrates which certainly existed from about 509 B.C. onwards; (4) other documents, such as religious calendars, texts of treaties, laws (e.g. the Twelve Tables); (5) accounts of Greek, and probably Etruscan, historians.[7] Furthermore, paintings and sculptures must have helped to preserve the recollection of important historical events. Etruscans and Romans used painting to celebrate events. The Etruscan painting of Vulci which has revealed to us the fights between Caeles Vibenna and Mastarna on the one side and the Roman Tarquin on the other probably belongs to the late fourth century B.C.[8] The lost picture in the Curia Hostilia, representing the victory over King Hieron in 263 B.C. (Pliny, *NH* 35, 22), was after all not perhaps the first of its kind. As is well known, a painting which is generally attributed to the second century B.C. was found in a tomb of the Esquiline and represents dealings between the Roman general Q. Fabius and (an enemy general?) M. Fannius: the scene seems to refer to the Samnite Wars.[9] Historical and mythical scenes were represented in reliefs in the Basilica Aemilia; Professor Carettoni's recent publication has notably enriched our knowledge of them.[10] They do not belong to the original Basilica of 179 B.C. but to a later reconstruction. They are, however, in the republican tradition of historical evocation.

The tradition on the origins of Rome which emerged from the various elements must have been authoritative by 260 B.C. Timaeus, who received information from natives of Latium, already knew of the Lavinian Penates and of Servius Tullius' reforms.

Though a sort of vulgate developed on early Roman history we must not exaggerate its strength and uniformity. Even Roman historians knew how to exercise their discretion or their fantasy. The very date of the foundation of Rome remained long uncertain. Timaeus put it in 814 B.C. and made it contemporary with the foundation of its rival, Carthage; Fabius Pictor chose 748, Cincius Alimentus 728. The so-called traditional date of 753 B.C. was made authoritative by the great Varro only in the first century B.C.[11] In

[6] It will be enough to refer to R. Besnier, 'Les Archives privées, publiques et religieuses à Rome au temps des Rois', *Studi in memoria di E. Albertario*, Milano, 1953, II, 1–26.

[7] On Etruscan sources cf. M. Sordi, *I rapporti Romano-Ceriti e l'origine della civitas sine suffragio*, Rome, 1960, 143.

[8] On the date W. L. Brown, *The Etruscan Lion*, 1960, 160. There are some adventurous theories in J. Gagé, *Mél. École Rome* 74, 1962, 79–122.

[9] I was not persuaded by G. Zinserling, *Eirene* (Praha) I, 1960, 153–86, but see also his other paper, 'Republikanische Historienmalerei und Tagespolitik', in *Sozialök. Verhältnisse im Alten Orient und im Klass. Altertum*, Berlin, 1961, 346–54. An alternative interpretation by A. Alföldi, *Der frührömische Reiteradel*, 1952, 50, is discussed by Ernst Meyer, *Gnomon* 25, 1953, 185, and F. Altheim, *Röm. Geschichte*, II, 1953, 438.

[10] *Riv. Istit. Naz. Archeologia e Storia dell'Arte* 19, 1961, 5–78. cf. H. Furuhagen, *Opuscula Romana* III, 1961, 139–55.

[11] cf. R. Werner, *Gnomon*, 1963, 193–4. Recent literature in E. Bickerman, *La cronologia nel mondo antico*, Florence, 1963, 68. Cf. B. Niese–E. Hohl, *Grundriss der röm. Geschichte*, 1923, 90.

some cases it is patent that the republican historians agreed among themselves simply because they copied each other. When a historian or antiquarian with some initiative cared to check the facts, trouble started. Mastarna, always known to the Etruscan tradition, is never mentioned in the annalistic tradition. He was rediscovered by Roman antiquarians acquainted with the Etruscan tradition such as Verrius Flaccus and the Emperor Claudius. Similarly Pliny the Elder (*NH* 34, 139) and Tacitus (*Hist.* 3, 72) learned somewhere that Porsenna had compelled Rome to surrender. My friend O. Skutsch proved in *JRS* XLIII, 1953, that there was a tradition, ignored by all the historians of Rome but remembered by Silius Italicus, that the Capitol fell to the Gauls—geese notwithstanding. An aberrant tradition is not necessarily better than the orthodox one. The name of one Egnatius has been preserved for us simply because he maintained that Remus survived the famous quarrel. Our informant, the anonymous compiler of the *Origo gentis Romanae* 23, 6, unfortunately does not tell us how Egnatius concluded his story.

Neither in Antiquity nor in the Middle Ages was the traditional account ever submitted to a searching examination. New facts and new interpretations, if they ever emerged, remained isolated episodes. The humanists of the fifteenth and sixteenth centuries had to start with Livy and Dionysius, and their progress was slow. L. Valla was the first to notice that on his own evidence Livy made a mistake in presenting Tarquinius Superbus as the son of Tarquinius Priscus (1, 46, 4). Research on the Roman political institutions progressed much more securely than that on political history. In the sixteenth century scholars were discussing the ' lex curiata ' much more competently than the history of the individual kings. The outburst of forgeries and fictions about the history of all the nations of Europe which started with Annius of Viterbo in 1498 is typical of the situation. If Annius showed that Noah under the name of Ianus came to stay ' in agro Viterbensi ', later to be the proud birthplace of Annius himself, Florian de Ocampo (1543) could say that the first Romans were Spaniards ; whereupon the Portuguese Manuel de Faria y Sousa (1628) replied that the majority of the founders of Rome were Portuguese. The first modern dissertation on the origins of Rome, *De Origine Romanorum*, by Ottaviano Ferrari written about 1586 (Graevius, *Thesaurus* I), is a refutation of Annius and Florian de Ocampo. It is perhaps unnecessary to add a reminder that the Trojans Brutus and Francus, both more ancient than Romulus, remained the ancestors of the Britons and the Franks, even if Polydore Vergil and Claude Fauchet protested. As a result of such discussions the question of the origins of Rome was shown to be dependent on an evaluation of the sources and on a reconsideration of the chronology. By applying both criteria Johannes Temporarius was the first, in about 1582, to reject the traditional story of Romulus as a tale spread by the Greek Diocles of Peparethus (*Chronologicarum Demonstrationum libri III*). Twenty-five years later the father of modern historical geography Philippus Cluverius rejected both the story of Aeneas and that of Romulus, and he was at least partially followed by Samuel Bochart. The Dutch scholars of the last decades of the seventeenth century who re-examined the origins of Rome in a conservative spirit knew well enough that they had to answer the objections of the sceptic. Three main works followed each other within one year : in 1684 the two Leiden professors Theodorus Ryckius and J. Gronovius published respectively a *Dissertatio de primis Italiae colonis* and a *Dissertatio de Origine Romuli* ; their pupil J. Perizonius published his great book of *Animadversiones historicae* in 1685 and later (1709) added a *Dissertatio de Historia Romuli*. Gronovius was a conservative with a difference : he believed that Romulus came from the East and brought Hebrew words and institutions to Rome. Both Ryckius and Perizonius tried to defend the essential elements of the Roman tradition more soberly. Both mentioned the banquet songs as one of the channels through which authentic tradition could be preserved. As has been pointed out in a recent dissertation by H. J. Erasmus,[12] which appropriately comes from Leiden, it was commonly believed by Dutch and Danish scholars of the late sixteenth and of the seventeenth centuries that popular poetry was an important source of historical information.

Perizonius' *Animadversiones* opened the series of thorough studies of the ancient sources which goes on into the eighteenth and nineteenth centuries and includes the names

[12] *The Origins of Rome in the Historiography from Petrarch to Perizonius*, diss. Leiden, 1962, and my discussion of it in *Riv. Storica Italiana* 75, 1963, 390–394.

of De Beaufort, Niebuhr and Mommsen. Niebuhr gave a prominence to the banquet songs as the main ultimate source for early Roman history which went far beyond the intentions of Perizonius. Mommsen saw the weaknesses of the banquet songs hypothesis and developed the principle already enunciated by J. Rubino that the tradition about archaic institutions is more reliable than that on military and political events. Mommsen was of course well aware of the importance of linguistic, epigraphical and archaeological evidence, even for the questions of archaic Roman history. But when he wrote his *Römische Geschichte* and later his *Römische Forschungen*, the amount of non-literary evidence about archaic Rome was still very small. Quite early he discovered the importance of the Roman Calendar for archaic Roman religion, but only in his very last years did he witness the excavations of Giacomo Boni and the find of the *Lapis niger*.

To-day the main task of the historian is to combine literary and non-literary evidence—and among the non-literary evidence we must attribute equal importance to the linguistic data and to the archaeological explorations. There is furthermore a type of literary evidence which is in a class of its own—the evidence of religious ceremonies for the development of the city of Rome. I am reserving for the second part of this paper an attempt to trace the social and institutional development of Rome under the kings, and shall now proceed to a brief examination of these three main groups of facts : the linguistic data, the religious ceremonies and the archaeological discoveries. My main purpose is to see how far they support or contradict or supplement the main lines of the literary tradition, as represented by Livy and Dionysius of Halicarnassus. By implication I assume that the literary tradition, however doubtful, must still be our guide. I shall later try to justify more precisely my point of view. For the moment I shall simply say that no necropolis, however rich, can ever replace the living tradition of a nation. The historian must repeat with the Psalmist ' The dead praise not the Lord, neither any that go down into silence '.

(ii) *Linguistic Evidence.*—The inhabitants of Rome spoke the Indo-European language we call Latin. The evidence for Latin being spoken in Rome is not earlier than the second part of the sixth century B.C. to which, for reasons of archaeological comparison, the Duenos Vase certainly belongs : the *Lapis niger* of the Forum is probably not much later, though the evidence for its date is much more tenuous. But two sixth-century vases with Etruscan words have been found in Rome, one on the Capitol and the other on the Palatine.[13] Moreover, a dedication by Aulus Vibenna found at Veii (middle of the sixth century) may belong to one of the two brothers Vibenna who played a part in the history of monarchic Rome.[14] The Etruscan influence in the sixth century is thus linguistically confirmed. Rome was, however, never bilingual in the way in which England was bilingual after the Norman invasion. To begin with, the Twelve Tables composed in the middle of the fifth century B.C., as far as we have them, are in pure Latin. Secondly, the words borrowed from Etruscan are comparatively few in the Latin language. ' Histrio, lanista, mantisa ' are some of the most certain. ' Populus, familia, litera, catena, columna ' are much less certain borrowings.[15] ' Populus ' is the most interesting item of this second series. If of Etruscan origin, it would indicate a considerable influence of Etruscan political notions in Rome. But the evidence seems to me unsatisfactory.[16] Some Greek words, such as ' triumphus ' (θρίαμβος) and ' gruma ' (γνῶμα) must have come to Rome through Etruria. Greek linguistic influence in the fifth century is now clearly documented at

[13] M. Pallottino, *Studi Etruschi* 22, 1952–3, 309 ; *Bull. Comm. Arch. Com.* 69, 1941, 101–7.

[14] M. Pallottino, *Studi Etruschi* 13, 1939, 455.

[15] It will be enough to refer to A. Ernout, *Bull. Soc. Linguistique* 30, 1930, 82–124, reprinted in *Philologica* 1946, 21–51 ; id., *Aspects du vocabulaire latin*, 1954. cf. the criticism of W. Schulze's theory on the Latin name-system by G. Bonfante in *Mélanges J. Marouzeau*, 1948, 43–59. General works by A. Meillet (5th ed., 1948) ; G. Devoto (2nd ed., 1944), L. R. Palmer (1954), E. Pulgram (1958), V. Pisani (1962) and F. Altheim's strangely conceived *Geschichte der lateinischen Sprachen*, 1951 are here presupposed. See also C. Battisti, *Sostrati e parastrati nell' Italia preistorica*, Florence, 1959, 102–70 and Ernst Meyer, *Röm. Staat und Staatsgedanke*, 1961, 467.

[16] G. Devoto, *Studi Etruschi* 6, 1932, 243–60, has, however, found wide support : cf. Walde-Hofmann, *Lat. Etym. Wört.* s.v. ' populus ' (1949). On ' plebs ' see O. Szemerényi, *Innsbrucker Beiträge zur Kulturwiss.* 15, 1962, 184. Cf. C. Battisti, *St. Etruschi* 27, 1959, 385–414 ; G. Radke, P-W s.v. Populonia.

Lavinium. Sabine influences are much more conspicuous and perhaps to be found even in domestic terms such as ' bos ', ' scrofa ', ' popina ', ' rufus ', ' lupus '. One has the impression that an exact demarcation line between Sabine and pure Latin elements is impossible.[17] This again confirms tradition, according to which the mixture of Sabine and Latin elements started with Romulus and amounted to a fusion. I cannot follow Devoto when he maintains that the existence of three words in Latin to indicate the red colour—' rufus ', ' ruber ', and ' rutilus '—is to be related with the three Romulean tribes.[18] I wish history were so simple. But the composite nature of the Roman population—and the transitory character of the Etruscan rule—is clearly confirmed by linguistic evidence.

Evidence from Religious Customs.—The notion that the Luperci in the festival of the Lupercalia used to run around the Palatine seems to confirm the tradition that the Palatine was the earliest centre of the settlement called Rome. Doubts have been expressed, notably by A. Kirsopp Michels in *Trans. Am. Phil. Assoc.* 84, 1953, but do not seem to be sufficiently well founded.[19] Another aspect of the question deserves more attention. The Luperci were divided into two groups: the Quinctiales and the Fabiani. Now we know from Livy 5, 46, 2, that the Fabii used to celebrate their own ' sacra ' on the Quirinal and were therefore especially connected with that hill.[20] This would of course not be enough to conclude that the two groups of Luperci represented the fusion of two communities. But we know also that the archaic priesthood of the dancing warriors—the Salii—was divided into two groups, and this time their original localization on two different hills is made patent by their names: Salii Palatini and Salii Collini, that is Salii of the Quirinal. It seems reasonable to conclude that both Luperci and Salii were priesthoods resulting from the fusion of two communities, one of which was centred on the Palatine and the other on the Quirinal. At this point we may add that at least from Varro onwards the Quirinal had been connected with the Sabines of Titus Tatius (*LL* 5, 51), though the name Quirinal is not Sabine: (arx) Tarpeia is more ' Sabine '. Next comes the mysterious festival of the Septimontium. The relevant texts by Festus, pp. 458, 476 L., raise several problems of interpretation which must not detain us here, but we can accept the conclusion that the *Septem montes* concerned in the festival were the three parts of the Palatine (Cermalus, Palatium and Velia), the three parts of the Esquiline (Oppius, Cispius and Fagutal) and finally the Caelius.[21] The fact that the Palatine was not represented as one body may point to a phase earlier than the traditional foundation of Rome. On the other hand, the fact that the Caelius and the Esquiline were involved may point to the opposite conclusion that the Palatine community had already extended its influence to the neighbouring hills. I do not think that we can reach a definite conclusion on this point. More important is the negative deduction that the other hills, notably the Quirinal, were still outside. This can be taken as a further confirmation of the theory that the fusion of the Palatine and of the Quirinal communities represents a well defined stage in the development of Rome. The festival of the Argei, so precisely described from the topographical point of view by Varro, *LL* 5, 45, must represent yet another stage, when Rome was divided

[17] M. G. Bruno, ' I Sabini e la loro lingua ', *Rend. Ist. Lombardo* 95, 1961, 501–41; 96, 1962, 413–42, 565–640. But note also G. Bottiglioni, *Studi Etruschi* 17, 1943, 315–26 (with bibl.).

[18] G. Devoto, *Athenaeum* 31, 1953, 335–43; cf. P. G. Scardigli, *Parola del Passato* 78, 1961, 181–9. cf. G. Devoto, *St. Etruschi* 26, 1958, 17–25. On Lavinium, A. Degrassi, *Inscr. Lat. Lib. Reipublicae* II, 1963, 1271 *a*.

[19] Kirsopp Michels is approved by Gjerstad, *Legends and Facts* 10, but cf. K. Latte, *Röm. Religionsg.*, 1960, 84, n. 4. On the name J. Gruber, *Glotta* 39, 1960–61, 273–6; M. P. Nilsson, *Latomus* 15, 1956, 133–36 = *Opusc.* III, 339.

[20] cf. also J.-P. Morel, *Mél. École Rome* 74, 1962, p. 31, n. 5. We cannot speak with confidence about the ' Capitolium Vetus ' (Varro, *LL* 5, 158): even if it was on the Quirinal (Mart. 5, 22; 7, 73), we do not know its date and origin (T. Hackens, *Bull. Inst. Hist. Belge Rome* 33, 1961, 69–88).

[21] G. Wissowa, *Gesamm. Abh.*, 1904, 230–52, not refuted by G. De Sanctis, *St. dei Romani* 1, 185. On L. A. Holland's theory, *TAPh. Ass.* 84, 1953, 16–34, see Gjerstad, *Legends and Facts* 23, n. 1, which also applies to J. Poucet, *Bull. Inst. Hist. Belge Rome* 32, 1960, 25–73. Poucet in a very thorough study returns, to my mind not persuasively, to a ' septimontium ' of eight ' montes '. See also M. Pallottino, *Arch. Class.* 12, 1960, 31. I refer to Wissowa for ' montani ' and ' pagani ', but cf. also the art. Montani (W. Schur) and Paganalia (G. Rohde) in P-W. We should know more about Early Rome if we knew what Nonae Caprotinae and Poplifugia were about, but see the excellent analysis by S. Weinstock in P-W s.v. Nonae Caprotinae. Other opinions in A. M. Colini, *Mem. Acc. Pont.* 3, 7, 1944, 20; A. von Gerkan, *Rh. Mus.* 96, 1953, 20–30.

into four regions but did not include the Capitoline Hill and the Aventine.[22] The Aventine did not become part of Rome, as defined by the sacred boundary called Pomerium, until the time of the Emperor Claudius. But it is worth noticing here that so far we have no clear archaeological evidence that the Capitol was occupied before the sixth century B.C. :[23] it was also reckoned to be outside the Curiae in the festival of the Fordicidia. On the present archaeological evidence, the festival of the Argei may go back to any date before the late sixth century B.C. Yet the four regions were already unified, and I therefore find it impossible to accept Gjerstad's opinion that the ceremony of the Argei ' reflects the phase of habitation when each village formed an independent community '. We do not know if and when the Capitolium was included in the Pomerium. Strictly speaking we do not even know what the Pomerium was and when it became an accepted notion. But we know that the Etruscan kings made the Capitol the religious centre of Rome : this can hardly be later than 525 B.C. To sum up, the evidence of the religious ceremonies seems to point to five stages, respectively including (1) the Palatine, (2) the Septimontium, with the reservation that the Septimontium may conceivably be earlier than the unification of the Palatine, (3) the synoecism of the two communities, one centred on the Palatine, the other centred on the Quirinal, (4) the organization of the *geminata urbs* into four regions with the exclusion of the Capitol and the Aventine, (5) the inclusion of the Capitol in the *urbs*. All these stages are well within the monarchic period. We can call stage one Romulus ; stage three Titus Tatius ; stage four Servius Tullius, leaving vague stages two and five.

The evidence from festivals also says something about the territory of the Roman State outside the Pomerium. From Strabo 5, 230, we learn that the Roman pontiffs celebrated sacrifices at selected points about 5 miles from the Forum to purify the Roman territory. The ceremony was called Ambarvalia, though there were other ceremonies called Ambarvalia which were purifications of private estates, not of the Roman territory. Paulus-Festus p. 5 L. in a difficult passage seems to connect the Ambarvalia with the Fratres Arvales, not with the Pontifices. He says ' Ambarvales hostiae appellabantur quae pro arvis a duobus fratribus sacrificabantur '. It is obvious that the Fratres to which he alludes are the Fratres Arvales. ' Duobus ' has been emended into ' duodecim ' because the Fratres Arvales were Twelve, not two. But ' pro arvis a duobus fratribus sacrificabantur ' is simply a Varronian etymology of *ambarvales*—where ' amb ' is derived from ' ambo ', not from ' ambire '. The Ambarvalia—says Festus—are called Ambarvalia because they were performed by two Arvalian brothers. This etymology is an unfortunate one, and from a passage of Macrobius, *Sat.* 3, 5, 7, it is clear that the same Festus gave an alternative and more correct explanation of Ambarvalia : ' ambarvalis hostia est, ut ait Pompeius Festus, quae rei divinae causa circum arva ducitur ab his qui pro frugibus faciunt.' Thus we can eliminate the number two simply as a bad conjecture by the source of Festus.[24] But the connection between the Fratres Arvales and the Ambarvalia is intrinsically reasonable and should not be dismissed, as Alföldi has recently done, merely because the number two is a mistake.[25] There must have been a time in which the Fratres Arvales used to perform the Ambarvalia. By Strabo's time the performance had been

[22] According to Gjerstad, *Legends and Facts* 22, the fact that the procession did not form a continuous circuit around the city ' but four separate circuits . . . around the boundaries of the pre-urban settlements ' indicates a pre-urban origin of the festival. This seems to me a doubtful argument, though it can claim the authoritative support of H. J. Rose, *The Roman Questions of Plutarch*, 1924, 98–101. The document itself (G. Rohde, *Kultsatzungen der röm. Pontifices*, 1936, 59 ; K. Latte, *RR* 412) and its content (G. Wissowa, *Ges. Abh.*, 221–3) point, *prima facie*, to a date not earlier than Servius Tullius : cf. Varro, *LL* 5, 45 ; 7, 44 ; Festus p. 14 L. ; Livy 1, 21, 5 ; Dionys. 1, 38 ; Ovid, *Fasti* 3, 791 ; 5, 621 (with the commentary by J. G. Frazer and by F. Bömer) ; Aulus Gellius 10, 15, 30. I cannot follow L. A. Holland, *Janus and the Bridge*, 313–31.

[23] The exclusion of the Capitol is a moot point. *Contra* e.g. De Sanctis, *St. d. Rom.* 1, 390, n. 3 ; H. Müller-Karpe, *Vom Anfang Roms*, 39 ; M. Pallottino, *Arch. Class.* 12, 1960, 24. But see also F. Altheim, *Hist. Rom. Relig.* 1938, 129–31 ; V. Basanoff, *Studi V. Arangio-Ruiz* 2, 1953, 323–32. The question is bound up with that of the Pomerium. Mommsen, *Staatsrecht* III, 379, n. 2, ' auf dem strenggenommen vom Pomerium ausgeschlossenen Capitol.' I cannot go into this question. To the new literature in Walde-Hofmann, *LEW* s.v. Pomerium, add the article by v. Blumenthal in P-W, J. Le Gall, *Annales de l'Est* 22, 1959, 41–54, and above all P. Grimal, *Mél. École Rome* 71, 1959, 43–64.

[24] cf. my note ' Ambarvales Hostiae ', *Maia* 15, 1963, 47–8, and G. Lugli's remarks in *Rend. Acc. Lincei* 8, 6, 1951, 371–3. Also A. Kilgour, *Mnemos.* 3, 6, 1938, 225–40.

[25] *Hermes* 90, 1962, 196, n. 1.

taken over by the pontiffs—according to a well-known trend in the religious affairs of Rome. There can be hardly any doubt that the pontiffs preserved the territorial limits of the ancient ceremony when they replaced the Fratres Arvales. This is confirmed by other ceremonies taking place about the fifth mile from Rome, for instance the *Terminalia* which were performed at the sixth mile on the Via Laurentina (Ovid, *Fasti* 2, 682), the *Robigalia* which were performed at the fifth mile of the Via Claudia (*Fasti Praenestini*), etc. Actual borders change quickly, and it would be absurd to try to find absolute consistency in all these indications. But the territory in question did not include the ager Albanus and the ager Ostiensis and therefore must be earlier than the incorporation of Alba Longa and Ostia into the Roman State: [26] it represents the age of Numa in terms of the traditional sequence of Roman kings and is later than the unification of Palatine and Quirinal.

Alföldi, most interestingly, has tried to show that this territory included the Servian tribes called Lemonia, Pollia, Pupinia, Voltinia, Camilia. According to him the tribes with names of famous *gentes*, such as Claudia and Fabia, are more recent than the tribes with (allegedly) place names.[27] I am not concerned here with the chronological consequences of this theory, but the theory itself seems to me erroneous. We have very little evidence about the distribution of the Servian tribes in their original setting but one of the few certain data is that the Pupinia tribe, selected by Alföldi as a tribe within five miles, reached at least the eighth mile from Rome. Hannibal, as Livy says (26, 9, 12), 'in Pupiniam exercitu demisso octo milia passuum ab Roma posuit castra.' The boundary represented by the Ambarvalia has probably nothing to do with the Servian tribes and is likely to be earlier than king Servius Tullius.

(iii) *Archaeological Evidence.*—There is some archaeological evidence that the Roman hills were inhabited in the second millenium B.C., but so far there is no proof of continuity between these settlements and the Early Iron age villages which can reasonably be taken as the components of early Rome.[28] The archaeological evidence which has a direct bearing on the origins and early development of Rome is collected in the first three volumes of E. Gjerstad, *Early Rome* (Lund, 1953 ff.). It may be convenient to summarize Gjerstad's evaluation of it.[29]

(1) Burial tombs on the Esquiline, Forum (Sacra Via), Quirinal and Palatine provide some essential archaeological data. The tombs on the Esquiline are numerically the most important. The overwhelming majority is of the 'fossa' type, and the burial rite seems to have been almost exclusively that of inhumation. According to Gjerstad they extend from the eighth to the early sixth century B.C. The tombs on the Sacra Via go from the eighth to the early sixth century; they include both inhumation and cremation, but cremation prevails in the early tombs. On the Quirinal the earliest tombs (eighth century) are of the 'pozzo' type for cremation; in the seventh century 'fossa' type tombs for inhumation appear. The most important tomb of the Palatine (discovered in 1954) is a cremation one, apparently of the eighth century B.C.[30]

(2) Among the archaic buildings which have been explored the following deserve special mention: (*a*) remains of huts have been found on the Palatine near the Scalae Caci

[26] The evidence about Ostia is examined judicially by R. Meiggs, *Roman Ostia*, 1960, 479. Further references in T. Hackens, *Antiq. Class.* 31, 1962, 305.

[27] 'Ager Romanus Antiquus', *Hermes* 90, 1962, 204. The anecdote on Fabius' estate 'in Pupinia' which is wrongly placed in Val. Max. 4, 8, 1 (De Sanctis, *St. d. Rom.* III, 2, 51, n. 81) may belong here (cf. Livy 26, 11, 6). Alföldi's dating of the origin of the Fabia tribe after 450 (p. 207) is also full of difficulties. It implies that the tribe was established when, according to the *Fasti*, the Fabii had lost their previous power, F. Münzer, *Röm. Adelsparteien* 53.

[28] See now the papers by R. Peroni and E. Gjerstad in *Bull. Comm. Arch. Com.* 77, 1959–60 (but published in 1963), 1–108, on the S. Omobono excavations. Gjerstad says 'già ora si può constatare che l'epoca pre-urbana di Roma si è prolungata di 700 anni, fino al 1500–1400 a.C.' cf. also Gjerstad, *Acta Archaeol.* 32, 1961, 215–18.

[29] cf. also P. Romanelli, *Bull. Paletnol. Ital.* 64, 1954, 257–60, and the reviews of Gjerstad by L. A. El'nickij, *Vestnik Drevnej Istorii*, 1960, no. 1, 150–8, and by P. Romanelli, *Gnomon* 31, 1959, 434–9. The collective volume of the Swedish Institute, *Etruscan Culture, Land and People*, New York and Malmö, 1962, is indispensable (with a chapter by Gjerstad on 'The Etruscans and Rome in archaic times', pp. 145–61).

[30] On this see G. Carettoni, *Bull. Paletnol. Ital.* 64, 1954–5, 261, and *Early Rome* III, 72. cf. S. M. Puglisi, *Bull. Paletnol. Ital.* 63, 1951–52, 45–59; 64, 1954–55, 299–322.

(eighth to seventh century B.C. ?) [31] and beneath the 'lararium' of the 'Domus Augustana' (eighth to seventh century B.C. ?); (*b*) the 'Regia' in the Forum has been shown to go back at least to the sixth century B.C. and the archaic finds from the area of the Temple of Vesta go back to about 550 B.C. or a little earlier; (*c*) the sacred area in the Forum Boarium in the region of the present church of S. Omobono contains, among others, at least one temple belonging to the sixth or the early fifth century; [32] (*d*) votive deposits of the Quirinal (Church of S. Maria della Vittoria, Villino Hüffer) include seventh and perhaps late eighth century materials and incidentally produced the Duenos vase which is dated by its form to about 525 B.C.[33]

(3) Stratigraphic exploration of the Comitium and of the Forum seems to show that (*a*) the Forum began to be occupied by huts in the seventh century B.C.; (*b*) about 575 B.C. a market place was laid out, the first real 'Forum Romanum'; (*c*) the gravelling of the open area later occupied by the Comitium was more or less contemporary with the construction of the first pebble floor of the Forum; (*d*) the votive deposit beneath the sacred place known as the *Lapis niger* (so-called tomb of Romulus) goes back to the early sixth century B.C.[34]

(4) Archaic architectural terracottas found on the Palatine and in the Forum, on the Capitol and on the Esquiline represent armed horsemen, chariot races, banquet scenes, gods or monsters and are similar to those found in Veii and Velletri.[35] They certainly belonged to sacred buildings and show the penetration of Etruscan and Greek taste in Rome in the sixth and early fifth centuries B.C.

Attic Black-Figured and Early Red-Figured pottery is a proof of direct commercial relations with Greece.[36] Moreover, the discovery in Veii of statuettes representing Aeneas carrying his father proves that even the myth of Aeneas was something that archaic Rome had in common with Etruria.[37]

(5) The remains of the city wall traditionally connected with Servius Tullius (Grotta Oscura wall) have been shown to belong to the fourth century B.C., but, according to Gjerstad, there remain earlier fortifications probably to be dated not much later than 500 B.C.[38]

So far Gjerstad. If one proceeds to discuss his conclusions one is faced by a paradoxical situation. The materials and the chronology produced by Gjerstad prima facie confirm the literary tradition in all the essential points, except the Servian wall. Tradition puts the foundation of Rome in the eighth century B.C. and makes the Palatine the earliest centre

[31] *Early Rome* III, 54–6, dates Hut A in the later part of Period II (700–625 B.C.), but adds that 'Hut A does not represent the earliest remains of habitation on this spot'. S. M. Puglisi, *Mon. Ant.* 41, 1951, 97, seemed to date the 'villaggio capannicolo sul Palatino' in the eighth century B.C., and this date is usually given by the Italian archaeologists (G. Carettoni, *JRS* L, 1960, 200; P. Romanelli, *Acta Congressus Madvigiani* I, 393). It may ultimately be correct. The excavation of the Temple of Magna Mater has produced no new decisive elements for the archaic period (P. Romanelli, *Mon. Ant.* 46, 1963, 202–330).

[32] According to Gjerstad, *Bull. Comm. Arch. Comunale* 77, 99–102, 'il primo tempio . . . ed il pavimento del Foro Boario appartenente a quel tempio si possono assegnare all'inizio del V sec. a. Cr.', but an open-air sanctuary preceded the Temple and is to be dated about 570 B.C. The literary evidence is collected by H. Lyngby, *Die Tempel der Fortuna und der Mater Matuta am Forum Boarium in Rom*, Berlin, 1939; cf. also his monograph in *Acta Instituti Romani Regni Sueciae* 7, 1954: *Beiträge zur Topographie des Forum Boarium-Gebietes*.

[33] On the Duenos Vase, *Early Rome* III, 161, is to be supplemented by Gjerstad's paper in *Septentrionalia et Orientalia: Studia B. Karlgren dedicata*, Stockholm, 1959, 133–43. cf. E. Peruzzi, *La Parola del Passato* 13, 1958, 328–46.

[34] cf. E. Paribeni, *Bull. Comm. Arch. Com.* 76, 1956–8, 3–21; A. Bartoli, 'I pozzi dell' area sacra di Vesta,' *Mon. Ant.* 45, 1961, 1–144, and in general P. Romanelli, *Bull. Fac. Lettres Strasbourg* 38, 1959–60, 235–43.

[35] The standard work remains A. Andrén, *Architectural Terracottas from Etrusco-Italic Temples* (1940): see furthermore Å. Åkerström, *Opusc. Romana* I, 1954, 191–231; E. Gjerstad, *Acta Instit. Rom. Norvegiae* I, 1962, 35–40 (on Jupiter temple).

[36] cf. also E. Paribeni, *Boll. Musei Comunali Roma* 6, 1959, 41–8, for a Greek vase of about 500 B.C.; G. Carettoni and L. Fabbrini, *Rend. Lincei* 8, 16, 1961, 55–60, for the findings of about the same date under the Basilica Iulia; and in general *Bull. Comm. Arch. Com.* 77, 1959–60, on S. Omobono.

[37] cf. G. Q. Giglioli, *Bull. Museo Impero* 12, 1941, 3–16; A. Alföldi, *Die Trojanischen Urahnen der Römer*, Basel, 1957 (reviewed in *JRS* XLIX, 1959, 170, by S. Weinstock); K. Schauenburg, *Gymnasium* 67, 1960, 176–91; G. Pugliese Carratelli, *La Parola del Passato* 82, 1962, 20–3 (with further bibl. on p. 22). I cannot understand J. Gagé, *Mél. École Rome* 73, 1961, 69–138; *Rev. Hist.* 229, 1963, 305–34; Degrassi, *Inscr. Lat. Lib. Reip.* II, 1271.

[38] E. Gjerstad, *Studies D. M. Robinson* I, 1951, 412–22; id., *Opusc. Romana* I, 1954, 50–65, and 3, 1960, 69–78. One Attic Red-Figure sherd found in the second stratum of the agger on the Quirinal is the basis for the date.

of Rome.[39] Excavations confirm the existence of tombs (and perhaps also of huts) on the Palatine belonging to the eighth century B.C. Tradition clearly marks a caesura between pre-Etruscan and Etruscan Rome. Now Gjerstad presents imposing evidence for a transformation of the city about 600 B.C., when the Forum begins to be the political and commercial centre of the *urbs* and Etruscan artists operate in Rome. Furthermore the tradition is quite explicit in indicating contacts with the Greek world for the first half of the fifth century B.C. : Greek artists are said to have decorated the plebeian Temple of Ceres ; corn is imported from Sicily ; a Roman embassy goes to Athens to study Athenian law. Now Gjerstad's chronology implies that Greek imports continued up to c. 450 B.C. and rapidly decreased only in the second half of the fifth century B.C. True enough, the chronology of the tombs according to Gjerstad does not offer those clear-cut distinctions between inhumers and cremators which are dear to the hearts of certain archaeologists. Anyone who believes with F. von Duhn that any inhumer in Rome must be of Sabine origin will look with distress at the mixture of inhumers and cremators in the Roman Forum. However, those who do not regard burial rites as racial habits will hardly consider this a disadvantage. The tombs on the Esquiline and the Quirinal confirm the existence of communities which were absorbed by the Palatine community either in the Septimontium or in the ' geminata urbs ' traditionally connected with Romulus and Titus Tatius. The Esquiline community seems to have displayed a certain amount of cultural independence both in the almost exclusive adherence to inhumation and in peculiar pottery types.[40] On the other hand, the disappearance of the tombs from the Forum in the sixth century B.C. confirms the new level of civilization reached by Rome under the Etruscan kings.

(iv) Why then has Gjerstad reached a very different conclusion from his own evidence ? He maintains that Rome was founded only about 575 B.C. by unifying pre-existing villages and that the monarchy came to an end only about 450 B.C. with the decemvirate. I must add that his idea that the Etruscan rule in Rome ended only about 450 B.C. is now widely shared, for instance by Raymond Bloch, while Alföldi is about to publish a book that may be in substantial agreement with this revised chronology.[41] The explanation, to my mind, is to a large extent extra-archaeological. Gjerstad has been impressed by the theory acutely developed by his countryman, K. Hanell, in *Das altrömische eponyme Amt*, 1946, according to which 509 B.C. was not the first year of the Republic but more modestly the first year of the new cult of Jupiter Capitolinus. The earliest part of the Fasti would be a list, not of eponymous heads of the Roman State, but of eponymous magistrates connected with the new cult of Jupiter. Hanell's theory eliminates the evidence of the Fasti for the initial year of the Republic. This elimination seems to me unacceptable : it is arbitrary and would compel us to put the struggles between patricians and plebeians under Servius Tullius and Tarquinius Superbus.[42] Further arguments seem to Gjerstad to confirm a date of about 500 B.C. for Servius Tullius. The two Temples of Fortuna and Mater Matuta attributed by tradition to Servius Tullius, if rightly identified, would appear to be archaeologically not earlier than 500 B.C. Moreover, in a very ambiguous sentence Livy (II, 21, 7) seems to date in about 500 B.C. the creation of the local tribes (which Dionysius

[39] Evidence collected in Lugli, *Fontes* I, 26 ; 73. (On Ennius 157 Vahlen, S. Timpanaro, *Maia* 3, 1950, 26.) ' Roma quadrata ' must not, however, be taken too seriously : F. Castagnoli, *Studies D. M. Robinson* I, 1951, 389–99 ; J. Ward Perkins, *Acta Congressus Madvigiani* 4, 109. See the telling evidence of Cosa, F. E. Brown, *Cosa* II, 1960, 9–18. W. Müller, *Die Heilige Stadt*, Stuttgart, 1962, 1–52 (with bibl.), discusses the question in a wide context of cosmological speculations ; cf. also G. Dumézil, *Rituels indo-européens à Rome*, 27–43. P. Grimal's notion of the Forum as the earliest centre of Rome cannot, I think, be defended (*Lettres d'humanité* 4, 1945, 15–121 ; *Mélanges* 71, 1959, 59), but see S. Ferri, *Opuscula*, Florence, 1962, 600–603.

[40] F. von Duhn's pages in *Italische Gräberkunde* I, 1924, 428 ff., have not lost their fascination, but L. Banti, *St. Ital. Fil. Class.* 7, 1929, 171–98, was quick to see their weakness. Nor was G. Devoto, *Gli Antichi Italici*, now 2nd ed. 1951 (with bibl.), more convincing. I do not know what, archaeologically, makes a Sabine in Rome. See Pallottino's good remarks in *Archeol. Class.* 12, 1960, 31, and also Gjerstad in *Etruscan Culture*, 159, n. 3. But *contra* S. M. Puglisi, *La civiltà appenninica*, Florence, 1959, 99.

[41] R. Bloch would, I think, put the end of the Etruscan rule about 475. Gjerstad challenged this in *Etruscan Culture*, 161, n. 73.

[42] The best discussion of Hanell's theories I know of is by Ernst Meyer, ' Zur Frühgeschichte Roms ', *Mus. Helv.* 9, 1952, 176–81, but there are convincing objections in the reviews by F. E. Adcock in *JRS* XXXVIII, 1948, 105–9, and L. R. Taylor, *AJPhil.* 72, 1951, 69–72. Meyer rightly emphasizes the importance of Dionys. 7, 5. J. Gagé, ' La poutre sacrée des Horatii ', *Hommages à W. Deonna*, 1957, 226–37, I am unable to understand.

attributes to Servius Tullius). Finally, Gjerstad accepts the existence of an agger, to be dated about 500 to 470 B.C. by a fragment of a Red-Figured vase: this would be Servius Tullius' wall. It is, however, obvious that such additional arguments can carry weight only if one accepts Hanell's elimination of the evidence of the Fasti for the end of the monarchy. Whatever Livy may have meant by his statement about the tribes, he was certainly writing about an event of the republican period, not about Servius Tullius. The existence of a wall earlier than that of 380 B.C. is altogether denied by an authority such as A. von Gerkan.[43] If it did exist, it could hardly be dated by one lonely sherd. And even if it were dated c. 500 B.C., it would not necessarily be the work of Servius Tullius. As for the Temples of Fortuna and Mater Matuta, it is easy to observe: (1) that it is not yet certain that they are to be identified with those now discovered in the Forum Boarium; (2) that even if the identification were certain, it would not be surprising to see later temples wrongly attributed to a famous king. One may date a temple by a king, but it is very dangerous to date a king by the date of a temple attributed to him.[44] Gjerstad has still to perfect his theory, and indeed he promises three volumes on it.[45] We must wait. At the moment there seems to be a strong conflict between his evidence and his conclusions. Taken as a whole, the evidence as presented by him confirms the traditional sequence and chronology of the monarchy.

But is Gjerstad's evidence (as distinct from his conclusions) safe enough? Here the real trouble begins. For the critical traditionalist his evidence is too good to be true. The discussion that has followed the appearance of Gjerstad's books has confirmed that both the details of classification of the archaeological evidence and the chronology are still doubtful. One could hardly expect anything else. In the last fifty years archaeologists have been divided in their classification and dating of the Villanova civilization on which the early dates for Rome depend. The old conflict between O. Montelius who dated the early Villanovan in the eleventh century B.C. and G. Karo who scaled it down to the ninth to eighth centuries B.C. has reappeared in the opposite theories of H. Müller-Karpe and Gjerstad.[46] What Gjerstad puts in the eighth century B.C. Müller-Karpe puts in the tenth century B.C., and he connects it with survivors of the Late Mycenaean civilization who

[43] *Rh. Museum* 100, 1957, 82–97; *Von antiker Architektur und Topographie*, Stuttgart, 1959, 133–8; *Rh. Museum* 104, 1961, 132–48; *Arch. Anz.* 1963, 104–18.

[44] Gjerstad's conclusions were doubted by F. Castagnoli, *Bull. Comm. Arch. Com.* 74, 1951–2, 49–51, and more recently by G. Colonna, ibid. 77, 1959–60 (1962), 125–43. cf. F. Castagnoli, ibid. 77, p. 148, n. 10. Though not directly relevant I should like also to mention the paper by M. Pallottino, 'Gli Etruschi nell' Italia del Nord', *Hommages à A. Grenier*, 1962, 1207–16, which shows the difficulties of a historical analysis of archaeological evidence. K. Kromer, *Mitt. d. Praehist. Kommission der Oesterr. Akad.* 6, 1952–3, 119–44, is too schematic.

[45] Gjerstad, *Opusc. Rom.* III, 93; *Legends and Facts*, 50 attributes much importance to the statement by Varro *ap.* Pliny, *NH* 35, 157, that Vulca was invited to Rome by Tarquinius Priscus, not by Tarquinius Superbus. I do not pretend to guess how Varro would have known that it was Tarquinius Priscus, but Pliny's text does not say that Tarquinius Priscus ordered Vulca to make . . . the terracotta quadrigae for the roof, which is essential to Gjerstad's argument. For a recent discussion of this difficult passage see A. Rumpf in P-W s.v. Vulca. I find myself also unable to accept Gjerstad's chronological argument which is part of his penetrating study of the early Roman calendar (*Acta Archaeol.* 32, 1961, 193–214; cf. *Legends and Facts*, 57). According to Iunius Gracchanus, Tarquinius (Priscus) introduced the twelve-months calendar (Censor. 20, 4) and Servius Tullius the intercalary system (Macrob. I, 13, 20). This is a construction on the pattern of the progressive increase in the number of the 'equites' during the monarchy and can hardly claim authority. Varro (*ap.* Macrob.) in discussing various opinions on the introduction of intercalation quoted 'antiquissimam legem . . . incisam in columna aerea a L. Pinario et Furio consulibus [472 B.C.] cui mensis intercalaris adscribitur'. I do not see how one can deduce from this that Servius Tullius was reigning in 472 B.C. I shall quote only one more example of the arguments which I feel unable to accept: Gjerstad in *Etruscan Culture*, 160, n. 31, 'That the fictive Romulus was added to an earlier list of kings is indicated by the fact that Numa is represented as the second founder of the city (Livy I, 19, 1: *novam urbem de integro condere parat*). This indicates that he was originally mentioned as the founder of Rome, but when this role was later assigned to Romulus, Numa had to be downgraded to a second founder.' Here A. Alföldi, *Mus. Helvet.* 8, 1951, 203, is essential.

[46] For the earlier discussions it will be enough to refer to O. Montelius, *Die vorklassische Chronologie Italiens*, 1912, 170–2, and G. Karo, *Athen. Mitteil.* 45, 1920, 106–56; cf. the centenary volume *Civiltà del Ferro*, Bologna, 1960. Pre-war research on Villanova is reviewed by G. Kaschnitz-Weinberg, *Handbuch der Archäol.* II, 1954, 364; more recent work is critically summarized by R. Pittioni in P-W, Suppl. IX, especially 261 ff. (1962). M. Pallottino's discussion of H. Müller-Karpe, 'Beiträge zur Chronologie der Urnenfelderzeit nördlich und südlich der Alpen' (1959) in *Studi Etruschi* 28, 1960, 11–47, is important and gives a bibliography. See also R. Chevallier, *Latomus* 21, 1962, 99–123. I am also indebted for information to A. M. Radmilli (ed.), *Piccola Guida della Preistoria Italiana*, Firenze, 1962.

would have reached Latium.[47] Müller-Karpe too, of course, can quote some literary tradition in his favour: Aeneas found the Arcadian Euander on the Palatine.[48] On the other hand, H. Riemann in the *Göttingische Gelehrte Anzeigen* 214, 1960, 16–42, and R. Peroni, *Per una nuova cronologia del sepolcreto arcaico del Foro*, in the collective volume *Civiltà del Ferro*, Bologna, 1960, 463–99, have suggested intermediate dates supported by a clever rearrangement of the chronology of the finds. According to Riemann the earliest cremation tombs belong to the ninth century B.C.; inhumation tombs appear on the Esquiline about 775 B.C. and belong to a different population, that is to a later immigration. Gjerstad's epoch-making date of 575 B.C. is denied by Riemann, in whose view all the changes of the Forum happened before the end of the seventh century B.C. Peroni has independently suggested a very similar classification. He too separates an earlier stage of cremators characterized by proto-Villanovan pottery from a later penetration of inhumers who have their peers in the pre-Hellenic necropolis of Cumae and elsewhere on the Tyrrhenian coast of Southern Italy. He does not give absolute dates, but he seems to be nearer to Müller-Karpe than to Gjerstad.

This conflict of dates is due only in a very small measure to disagreement about the Roman evidence. While Gjerstad includes in his second and third stages a small group of cremation tombs found near the Temple of Divus Iulius by R. Gamberini Mongenet, other archaeologists maintain that they belong to the first period—in this way they give absolute precedence to the cremators over the inhumers by some fifty years or so. But these are secondary details: a gap of fifty or a hundred years between two types of tombs, even if real, may still be filled by the next discovery. The real cause of the conflict of dates between Gjerstad and Müller-Karpe is not the evidence, but the absence of indisputable evidence. As we all know, the first absolute date in the archaeology of ancient Italy is represented by the vase which goes by the name of the Egyptian king Bockhoris and was discovered at Tarquinia in 1895.[49] By the usual chain reaction the vase serves to date the beginning of the orientalizing style in Etruria and Rome. So far no proper study has ever been published about this vase, the tomb in which it was found and other objects with the name of Bockhoris which were discovered in Italy. The date of Bockhoris is now reasonably well established by the Egyptologists towards the end of the eighth century B.C. (718 to 712). This is the least troubling part of the matter. But the vase itself is said to be a Phoenician imitation of an Egyptian vase; and apparently there is another Bockhoris vase which was discovered at Lilybaeum in Sicily and was very vaguely made known in *Notizie Scavi* 7, 2, 1942, 284, with the not encouraging information that it is now in an archaeological context of the fourth century B.C. The name of King Bockhoris, if it is true that it was used for commercial purposes by Phœnician craftsmen and still circulated in the fourth century B.C., would obviously be of no great dating value. However, a scarab with the name of Bockhoris turned up on the island of Ischia a few years ago, and though the tomb in which it was discovered still awaits publication, we are informed that the tomb is dated by Greek vases to the end of the eighth century B.C. (*Parola del Passato* 54, 1957, 225). King Bockhoris remains the symbol both of the scarcity of our evidence and of the insufficiency of our research on it. The next and less famous piece of evidence is the bronze lion's head from Veii which is taken to be a direct import from Assyria.[50] Comparison with the reliefs from Khorsabad would suggest a date not earlier than the end of the eighth century B.C. for the production of the object; the date of its arrival in the Veii tomb is another matter. And there is no need to add that there are other factors

[47] Gjerstad has replied to Müller-Karpe in *Gnomon* 33, 1961, 378, and at greater length in *Opuscula Romana* v, 1962, 1–74. Gjerstad explicitly involves in his condemnation Peroni's 'absurd results'. Peroni has been attacked also by P. G. Gierow, ibid. III, 1961, 103–122, and has replied in *Bull. Comm. Arch. Com.* 77, 1959 (1963), 19–32. Peroni also published an important monograph 'Per una definizione dell' aspetto culturale subappenninico come fase cronologica a sè stante' in *Mem. Acc. Lincei* 8, 9, 1959. cf. also P. G. Gierow, *Opusc. Rom.* IV, 1962, 83–100.

[48] On a strange suggestion in the same sense by S. Mazzarino, *Studi Romani* 8, 1960, 385, cf. M. Pallottino, *Archeol. Class.* 12, 1960, 15, n. 2.

[49] First published by E. Schiaparelli, *Mon. Ant.* 8, 1898, 89–100. cf. E. Hall Dohan, *Italic Tomb-Groups*, Philadelphia, 1942, 106–9; J. M. A. Janssen, *Varia Historica . . . aan Prof. Doct. A. W. Byvanck*, Assen 1954, 17–29 (with bibl.). It is good to know that Gjerstad is preparing a new study of the Bockhoris tomb (*Opusc. Rom.* v, 60, n. 7). See also T. J. Dunbabin, *Arch. Ephem.* 1953–54 (1958), 253.

[50] W. L. Brown, *The Etruscan Lion*, 1960, 12, and M. Pallottino, *Antiquity* 36, 1962, 204.

of uncertainty. So much depends on the precise date one is prepared to attribute to Italo-Geometric and to Italo-Protocorinthian pottery.

(v) At the moment I do not feel that it would be honest to use Gjerstad's evidence against Gjerstad's conclusions without qualification, because Gjerstad's evidence is bound still to be revised. The revision may well lead to a date for the Palatine settlement nearer to Timaeus' date of the foundation of Rome in 814 B.C. than to the Varronian date of 753. It may also involve a different relative chronology of the settlements on the various hills. It is in any event likely that there were more than seven kings of Rome (or eight including Titus Tatius), and it is certain that a great deal of the history of the monarchy is lost. The evidence of the religious customs seems to point to early stages in the evolution of Rome which had been forgotten by the literary tradition. But it still seems to me that historical tradition is not contradicted by archaeology and gives us the essential constitutional, political and territorial development from the foundation of the *urbs* to the end of the monarchy. It also seems to me that the end of the monarchy can be safely left where tradition puts it in agreement with the Fasti: before 500 B.C. Nobody in his senses would expect all the trade with Etruria or all the Roman aristocratic families with Etruscan names to disappear overnight because of the fall of the monarchy. The Roman aristocrats eliminated a tyrant in the manner of the sixth century B.C.; they did not fight a war of national liberation in the style of the nineteenth century A.D.[51]

I should like to show by an example how tradition is in fact much more reliable than many critics think: the example will demonstrate at the same time that even a good tradition cannot be accepted wholesale. I can deal with it rather quickly because I have recently written on it in the *Rendiconti dei Lincei* (XVII, 1962, fasc. 7–12). Livy (1, 45) and Dionysius (4, 26) say that Servius Tullius founded a temple of Diana on the Aventine in order to cement his relations with the Latins: the temple was in fact meant to be a Latin federal temple (Varro, *LL* 5, 43). Dionysius still saw there what he took to be the foundation charter of the league. The antiquity of the foundation is confirmed by the fact that the text of the *lex Icilia de Aventino publicando* of 456 B.C. was deposited in this sanctuary of Diana (Livy III, 31; Dionys. 10, 32). Yet we must immediately admit that tradition is at fault in one point. Later epigraphical sources allude to a *lex arae Dianae in Aventino* which was the model for other cults (e.g. Dessau 112; 4907). Thus the original regulations for the cult of Diana on the Aventine envisaged an *ara*, an altar: not a temple, *aedes*. The literary tradition forgot that the Temple on the Aventine had been preceded by an altar, an *ara*. The later replacement of an *ara* by an *aedes* has many parallels. If Servius Tullius founded a sanctuary for Diana it was a *lucus* with an *ara*, not an *aedes*. Indeed it is tempting to suggest that also in the cases of Fortuna and Mater Matuta he established *arae*, later replaced by *aedes*; and this explanation may be extended to the Temple of Dius Fidius allegedly built by Tarquinius Superbus, but dedicated in 466 B.C. (Dionys. 9, 60, 8).[52] This is, however, a secondary point. Did Servius Tullius really try to establish a federal sanctuary for the Latins on the Aventine? Professor Alföldi is the latest of the many who have denied this.[53] His argument is derived from G. Wissowa, but the conclusions he reaches are his own. We know that the Temple of Diana on the

[51] Ultimately the new theories by Gjerstad, Bloch and Alföldi will stand or fall by their ability to make satisfactory history for the period 500–450 B.C. I attribute some general importance to the fact that the pre-Caesarian calendar is at least contemporary with, but probably earlier than, the Temple of Jupiter Capitolinus (M. P. Nilsson, *Opuscula Selecta* II, 1952, 979–87; A. Brelich, *Vesta*, 1949, 19–24; Latte, *RR* 1960, 1 and 36; Van L. Johnson, *TAPh. Ass.* 91, 1960, 109–20; id., *AJPhil.* 84, 1963, 28–35). This calendar presupposes both a well-ordered political organization and a long evolution. My impression is that it can hardly be the result of less than seventy years of the life of Rome as a city. This is a different matter from 'the enormous change from primitivism to monumentality' in buildings which, contrary to Gjerstad (*Legends and Facts* 55), I take to have happened between 600 and 500 B.C. Notice that Gjerstad assigns the Regifugium to the pre-urban period (before the foundation of Rome about 575 B.C.) which I do not consider plausible (*Acta Congr. Madvig.* 1, 390). Gjerstad writes: 'I believe that there were pre-urban kings, but these, who are and will remain anonymous, should not be confounded with the kings of the unified city whose names are known to us, from Numa Pompilius to Tarquinius Superbus.' What, then, is the real difference between the pre-urban and the urban stage? And why should the Romans have lost the memory of names earlier than 575?

[52] But see Wissowa's wise remarks on Dius Fidius, *Religion und Kultus* 2nd ed., 1912, 129. And also Gjerstad's excellent paper 'The Temple of Saturn in Rome', *Hommages à A. Grenier*, 1962, 757–62.

[53] *Studi e Materiali* 32, 1961, 21; D. van Berchem, *Mus. Helv.* 17, 1960, 30–32.

Aventine celebrated its own annual festival, its own *dies natalis*, on 13th August. Wissowa deduced from a passage of Statius, *Silvae* 3, 1, 59–60, that also the sanctuary of Diana Nemorensis at Aricia had the same anniversary, 13th August. From this alleged coincidence and other less cogent arguments he concluded that the sanctuary of Diana on the Aventine was an imitation, indeed a filiation, of the sanctuary of Diana Nemorensis. A. Alföldi accepted this theory and observed that there cannot be two Latin federal sanctuaries celebrating their festivals on the same day unless one has replaced the other. As there is good evidence to show that the sanctuary of Aricia was still active as a federal sanctuary after 500 B.C. he concludes that the Roman sanctuary is later than 500 B.C. and therefore cannot be a foundation of Servius Tullius. I shall not criticize this theory in detail simply because no criticism is needed. The very starting point of both Wissowa and Alföldi is wrong. Statius does not say that 13th August is the *dies natalis* of the Temple of Diana in Aricia. He says that in his own time the whole of Italy, including Aricia, celebrated the festival of Diana on 13th August:

omnisque pudicis
Itala terra focis Hecateidas excolit idus.

This is a very different matter. We know from various inscriptions of the Imperial period (e.g. Dessau. 1561, 3253, 7212) that the festival of Diana Aventinensis had become a general festival of Diana inside and perhaps also outside Italy. This was due to the prestige of the old Roman Temple, the regulations for which were considered a model, as we have seen, for other cults. What Statius teaches us is that even the Temple of Diana in Aricia had adopted the festival of Diana Aventinensis on 13th August. If anything, this fact confirms the great antiquity and authority of the Temple on the Aventine. The tradition of its foundation by Servius Tullius has certainly not been shaken by Alföldi. The fact that Dionysius still saw the foundation charter speaks for its reliability (apart from any other considerations of political history). Here the literary tradition is supported by documentary evidence.

It is curious to speculate how much of the history of Early Rome we should know to-day if we had only the archaeological evidence to rely upon. Somehow we could guess the existence of various separate villages though we should have no way of knowing that the Palatine was the original nucleus of the *urbs*. The transformation of the Forum would probably suggest a political unification of the villages, and the sudden outburst of new decorative art in the sixth century could certainly be interpreted as foreign influence from Etruria and Greece. If we were to include the archaic inscriptions in the archaeological evidence, we should have the possibility of establishing the near-bilingualism of the society of the sixth century. The *Lapis niger* would tell us that an Indo-European language was the language of the law, and therefore of the State, and by a comparison with other Indo-European languages we should reach the correct conclusion that a Rex was the head of the State.[54] Not much else could be established with certainty. The political and social structure of Early Rome would remain to a great extent unknown; the religion, the moral ideas, the very names of the legendary kings would of course be unknown. But I fear that archaeologists and linguists would not stop at this point and would probably give us one of those fanciful pictures which are sometimes produced in other fields under the name of Indo-European civilization, Megalithic civilization, Mediterranean civilization. Admittedly the literary tradition can be, indeed normally *is*, a source of difficulties. The historian of some thousand years later is called upon to separate truth from fancy in conditions which are sometimes impossibly hard. The archaeologists may have to beware of fakes, but can feel pretty safe in a regular excavation: stones do not lie. Yet this is ultimately a cause of weakness for the archaeologist. Men lie about the past because the past is not dead, because they are still struggling with it. Livy and Dionysius still reflect the process whereby the various sections of the Roman people reacted to their past in different centuries. Through the words of Livy and

[54] On the interpretation of the *Lapis niger* add to the bibl. given by A. Degrassi, *Inscr. Lat. Liberae Reipublicae*, I, 1957, 5, and K. Latte, *RR* 3, n. 4: G. Dumézil, *Rech. Sciences Rel.* 39–40, 1951–2 (*Mélanges Lebreton*), 17–29; M. Lejeune, *REA* 54, 1952, 342; G. Dumézil, *REL* 36, 1958, 109–11; G. Marchetti-Longhi, *Arch. Class.* 11, 1959, 50–69, and Degrassi, *Inscr.* quoted II, Addenda p. 379.

Dionysius the modern historian is in direct contact with what generation after generation of Romans thought about themselves. The pure archaeologist cannot rely on the living memory: he has to guess and to infer, very often by analogy. He has to deduce the thoughts from the objects, the individuals from the collective products. This procedure is ultimately far more open to arbitrary suggestions than the analysis of a literary tradition. Where there is a literary tradition, it is a safer guide to a past civilization than archaeology alone. But of course archaeology can act as an excellent control of a literary tradition. The archaeologists can check the truth of many stories by a direct approach which by definition is denied to the critic of literary texts. The rediscovery of Babylonia has told us more about Herodotus than any amount of literary criticism. As early Rome is the ideal place to combine archaeological exploration and source criticism, the study of archaic Rome remains an ideal school of historical method.

II. CURIAE AND CENTURIAE

(i) The Romans themselves had to do research in order to form an idea of pre-Servian Roman institutions. Some of the facts had no doubt been remembered by continuous tradition in the Roman aristocratic houses. Others were still reflected in current constitutional practice and were piously preserved as survivals. But still other facts had been forgotten and were brought back from oblivion by the ordinary process of antiquarian research in the Greek way: archaic texts were examined, monuments were interpreted, analogies were suggested from other cultural contexts (ceremonies of other Latin cities, Etruscan and Greek parallels). A great deal of conjectural thinking or of plain fancy was inevitably introduced into the descriptions of archaic Roman institutions which were written in the last centuries of the Republic. Indeed the picture presented by our ancient sources is not so clear and uniform as that presented by the modern vulgate. For instance, we know from Varro (*LL* 5, 55; 89; 91), from Propertius IV, 1, and later sources that there were in Rome three tribes—Tities, Ramnes and Luceres: the Tities having precedence over the Ramnes, and the Ramnes over the Luceres (cf. also Cic., *rep.* 2, 14; 36). Dionysius of Halicarnassus (2, 7, 2) mentions the existence of the tribes, but does not give their names. In his account of Romulus, Livy mentions the names, but not as those of the tribes. Livy says that Romulus established three centuries of knights which had the names of Ramnes, Tities and Luceres. It will be noticed that the official order of the names is not respected. The change is intentional, because Livy explains that the Ramnes or Ramnenses received their name from Romulus, the Tities or Titienses from Titus Tatius, while the etymology of Luceres was, in his opinion, uncertain: 'Ramnenses ab Romulo, ab Tito Tatio Titienses appellati, Lucerum nominis et originis causa incerta est' (1, 13, 8; cf. 1, 36, 2). Obviously the Tities could not have precedence over the Ramnes if they were called after Titus Tatius. It is not until Book X, 6, 7, that Livy shows his awareness of the Romulean tribes as such, and again not in the official order. In Book I he chose to ignore the tribes. We also happen to know from a lawyer of the Tiberian age, Masurius Sabinus (*ap.* Aul. Gell., *NA* 7, 7, 8; cf. Pliny, *NH* 18, 6) that the creation of the corporation of the Fratres Arvales was ascribed to the times of Romulus. Yet there is no mention of the Fratres Arvales in the accounts of Romulus' reign by Livy and Dionysius. The Fratres Arvales may have been connected with Romulus only by antiquarians later than Livy; but, for various reasons, this is not a very likely hypothesis. The silence of Livy and Dionysius is not easy to explain. These two examples will suffice to show that what we call ancient tradition is not invariably what we find in our most authoritative sources: it may even contradict them. Ancient tradition at its best is a combination of ancient texts; at its worst it is an arbitrary selection from ancient data. We can readily admit that a proper combination of ancient sources leads to the conclusion that Rome once had a king, three tribes, thirty centuriae—ten per tribe—a senate of 100 *patres* later brought up to 300, an infantry of 3,000 and a cavalry of 300 men, one-third of whom was provided by each tribe. But, if we are honest, we must immediately add that only Varro, *LL* 5, 89, says that each tribe provided 1,000 infantrymen, and 100 knights. If we want to include the six Vestal virgins in this picture of the most archaic Roman State, we must also admit that the connection between the number of the Vestals and the

number of the tribes—two Vestals for each tribe—is indicated by only one source, Festus, p. 475 L.

Just because the so-called ancient tradition on the constitution of Early Rome is in fact a modern simplified and rationalized synthesis of what the ancient sources say, we cannot hope to learn much from it about the real nature of the most archaic Roman institutions. There is a world of difference between this synthetic product of our erudition on Early Rome and the first-hand account of Roman institutions of the second century B.C. provided by Polybius. For similar reasons we do not know much about archaic Sparta and archaic Athens. And yet our best eye-witnesses of the institutions of Sparta and Athens lived in the late fifth and in the fourth centuries B.C. and were therefore chronologically nearer to the situation of the sixth century than any student of the Roman constitution ever was.[55]

It is perhaps no exaggeration to say that we would know how the archaic Roman institutions worked if we knew what the Curiae were. A glance at the evidence is enough to show that the Curiae were the pivotal element of archaic Rome. Yet does a combination of the ancient data help us to form an idea of what they were ? [56]

Let us summarize what we are told about the thirty Curiae. The word *curia* was applied both to a section of the Roman population included in the tribes and to certain buildings. Some of the buildings called Curia had something to do with the Curiae as subdivisions of the Roman State : others had not. If it is true that *curia* comes from *co-viria* and etymologically means ' gathering of people ', one can easily understand why it could apply equally to a tenth of a tribe, and to the place where certain individuals met.[57] The Curia Pompei and even more evidently the *curia athletarum* had nothing to do with the Romulean Curiae.[58] Each Romulean Curia, however, had its official residence. In the later Republic the banqueting halls of the Curiae seem to have been concentrated in two buildings—respectively called *curiae veteres* and *novae*. The *curiae novae* had been meant to replace the *veteres*, but Festus most interestingly informs us that when the *curiae novae* were built, four (or seven) Curiae refused to leave their old residence (p. 180 L.) ; thus both buildings remained occupied. Dionysius of Halicarnassus visited some of these places and rather admired them ; but he seems to have witnessed a rustic banquet offered to the gods rather than a curial dinner party (2, 23, 5). In a little noticed article of *Studi Etruschi* Professor Minto suggested that the collective buildings of the Curiae went back to a prehistoric type, an example of which he believes to have discovered at Monte Cetona in Umbria.[59] I can do no more than refer to this pleasant speculation. The Curiae were certainly a Latin (and perhaps also an Etruscan) institution to be found in cities other than Rome. There were Curiae in Lanuvium,[60] and later the Curiae spread among colonies

[55] Ernst Meyer, *Röm. Staat u. Staatsgedanke* 2nd ed., 1961, provides an invaluable guide to Roman constitutional problems. The best introduction to historical method is contained in P. Fraccaro's discussion of Beloch's *Römische Geschichte* in *Riv. Fil. Class.* 56, 1928, 551–69 ; 57, 1929, 267–76, and in his paper ' La storia romana arcaica ', *Opuscula* 1, 1–24 (cf. *JRS* XLVII, 1957, 59). Equally exemplary Ed. Meyer, *Zur älteren römischen Geschichte*, in *Kleine Schriften* II, 1924, 286–307. Ernst Meyer, *Röm. Staat* and Fraccaro's ' Storia romana arcaica ' are here presupposed throughout. Cf. the excellent survey by W. Kunkel, *Zeitschr. Sav. Stift.*, Rom. Abt., 85, 1955, 288–325 ; 86, 1956, 307–25 ; 90, 1960, 345–82.

[56] The evidence in P-W s.v. *curia* by Chr. Huelsen. F. Altheim, *Epochen der Römischen Geschichte* 1, 1934, 70–86, is important. cf. also L. Pareti, *St. di Roma* 1, 1952, 287 and *passim* : Pareti's personal interpretation of the monarchic period deserves attention. P. de Francisci, *Primordia civitatis*, 572–91, is discussed by U. Coli, *Studi Senesi* 71, 1959, 375–423, especially 400–5 ; F. De Martino, *Storia della Costituzione Romana* 1, reprint 1958, 120–31 ; C. W. Westrup, *Rev. Int. Droits Ant.* 3, 1, 1954, 462–73 ; W. Kunkel, ' Zum römischen Königtum ', *Ius et lex. Festgabe ... M. Gutzwiller*, Basel, 1959, 1–22. P. Catalano, *Contributi allo studio del diritto inaugurale* 1, Torino, 1960, 391–585 (almost an encyclopaedia on the subject of Roman monarchy). cf. also the interesting considerations by L. Gerschel, ' La conquête du nombre ', *Annales* 17, 1962, 691–714, especially 708–10.

[57] Walde-Hofmann, *LEW* s.v. *curia*. *Covehriu* is attested in the Volscian table of Velletri with the meaning either of ' contio ', ' conventus ' (E. Vetter, *Handbuch der Italischen Dialekte* 1, 1953, 156) or of ' curia ' (F. Altheim, *Unters. zur Römischen Geschichte* 1, 1961, 85) ; cf. also J. Untermann, *Indog. Forsch.* 62, 1955, 123–35, and G. Dumézil, *REL* 31, 1953, 175–90.

[58] The evidence is collected in Platner-Ashby, *Topogr. Dictionary of Ancient Rome*, 1929, s.v. See especially Festus, s.v. Novae curiae (where I accept Veliensis for Vellensis), p. 182 L. ; Tac. *Ann.* 12, 24, 3 ; Ovid, *Fasti* 3, 140.

[59] *Studi Etruschi* 19, 1946–7, 377–81.

[60] *CIL* XIV, 2114, 2120, 2126 = Dessau, *ILS* 6201, 6199, 6202. For Tibur cf. Serv., *Aen.* 1, 17, and K. Latte, *RR* 106, note. One would like to know who were the ' meos curiae vernulas ' of Tibur. For the equivalent of Curiae in Etruscan cities, see Festus s.v. rituales, p. 358 L. ; Servius, *Aen.* 10, 202. cf. also V. Basanoff, *Rev. Hist. Rel.* 123–4, 1941, 110–41.

and municipia outside Italy, not necessarily among those of Latin rights. The names of Roman buildings or sections of buildings which can be certainly or probably connected with the Romulean Curiae are : Foriensis, Rapta, Veliensis, Velitia, Titia, Faucia, Acculeia, Tifata. Not surprisingly some Curiae take their names from a gens (Titia, Faucia, Acculeia), others from a place ; Veliensis is to be connected with Velia, a section of the Palatine, and Foriensis has presumably to do, if not with *the* Forum, at least with *a* Forum. Each Curia had its cults, which were public cults, not private ones (Dionys. 2, 23 ; Fest. pp. 54, 284 L.). Each Curia had its own leader, a Curio, its own priest, a flamen, and its own orderly, a lictor.[61] The Curio was certainly never a military leader because according to Dionysius he was a man of at least fifty years of age and was elected for life (Dionys. 2, 7 ; 21 ; 64). How he was chosen and for what precise purposes we do not know ; in later times there was little for him to do but to preside over religious ceremonies of the Curia. Even more mysterious is the head of all Curiae, the *curio maximus*, who in republican times up to 209 B.C. was a patrician. How he was chosen originally we do not know, but in 209 B.C. the assembly that elected the first plebeian *curio maximus* was perhaps the Comitia Tributa.[62] The Curiae had a special devotion for Iuno Quiritis—which poses the problem of the relation between Curia, Quirites, Quirinus.[63] But their most famous festivals, the Fornacalia and the Fordicidia, were unsophisticated agricultural rites.[64] The Fornacalia, a movable festival lasting several days in February, was said to have been established by Numa. It was in Pliny's obscure words (*NH* 18, 8) ' farris torrendi ferias et aeque religiosas terminis agrorum ', that is a feast of ovens accompanied (in the most natural interpretation of the sentence) by a purification of boundaries : which boundaries is the question. The festival was partly conducted by each Curia separately, but on the last day, coinciding with the Quirinalia, those who did not know to which Curia they belonged were given a chance to take part in the rites—hence the nickname of ' feriae stultorum '. The Curiones conducted the business, and the pontifices were conspicuous by their absence.[65] The other festival of the Curiae was the Fordicidia on 15th April. Pregnant cows were slaughtered, some on the Capitol and one in each Curia. The sacrifice was to Tellus, and the Vestals and the pontiffs took part in it.[66] Some purification of the boundaries may have gone with it, but our authority for this, John Lydus (*De mens.* 4, 72), is not reliable.

We also know of course that the Curiae were the voting units of a political assembly. During the late Republic thirty lictors represented the thirty Curiae of this assembly. Even in this form they conferred the imperium on the consuls previously elected by the ' comitia centuriata '—from which fact we may deduce that the Comitia Curiata were originally the assembly entitled to confer power on the Roman King and perhaps on other magistrates. The same symbolic Comitia Curiata, in a special form called ' comitia calata ', which was presided over by the Pontifex Maximus, met to sanction the transition of a man from one gens to another : in earlier times the Comitia Curiata had also been concerned

[61] The evidence in P-W s.v. *curio* (Kübler). I do not accept the ' decurio ' and the ' decuria ' of Dionys. 2, 7, as historical realities. I am also unable to follow Mommsen, *Staatsrecht* III, 104, n. 6, but see J. Wiesner, *Klio* 36, 1943, 98 on Festus p. 47 L. s.v. ' centuriata comitia '. On the ' curio minor ' (*CIL* II, 1262 ; VI, 2169 = Dessau, *ILS* 1320) ; cf. Mommsen, *Staatsrecht* III, 101, n. 4.

[62] Livy 27, 8, 1. cf. Mommsen, *Staatsrecht* II^3, 27, n. 4.

[63] Dion. 2, 50, 3 ; Festus p. 56 L. ; perhaps also Festus p. 302 L. The age and origin of the Roman cult of Iuno Curitis is doubtful (S. Weinstock, P-W s.v. Tibur, 832 ; K. Latte, *RR* 168. cf. Walde-Hofmann, *LEW* s.v. Quirites, and K. Latte, *RR* 59, n. 1 and 113). C. Koch, *Religio*, 1960, 17–39 ; R. Schilling, ' Janus ', *Mél. École Rome* 72, 1960, 119 ff. (with bibl.), and A. Brelich, ' Quirinus ', *Studi e Materiali* 31, 1962, 63–119, do not seem to me conclusive ; cf. also J. Paoli, *Studi U. E. Paoli*, 1956, 525–38 ; W. Burkert, *Historia* 11, 1962, 356–76, and C. J. Classen, *Philologus* 106, 1962, 174–204.

[64] Wissowa in P-W s.v. Fordicidia and Fornacalia. On Fordicidia cf. S. Weinstock, *Glotta* 22, 1934, 142–8 ; G. Dumézil, *Rituels indo-européens à Rome*, 1954, 11–26. On Fornacalia, L. Delatte, *Antiq. Class.* 5, 1936, 391–400, did not persuade me but must be consulted on the controversial interpretation of Pliny, *NH* 18, 8. With hesitation I accept W. Warde Fowler, *Roman Festivals*, 1899, 304. I cannot follow here and elsewhere A. Brelich's interesting suggestions, *Tre variazioni romane sul tema delle origini*, Rome, 1955, 113 ff.

[65] Ovid, *Fasti* 2, 519 ff. (with F. Bömer's commentary) ; Varro, *LL* 6, 13 ; Festus p. 304 and 418 L. ; Plut., *QR* 89.

[66] Ovid, *Fasti* 4, 629 ff. ; Varro, *LL* 6, 15.

with testaments.[67] Finally we must quote that surprising plebiscitum, the 'plebiscitum Ovinium', which seems to have ruled that senators were to be chosen 'curiatim', according to Curiae: 'ut censores ex omni ordine optimum quemque curiati⟨m⟩ in senatum legerent' (Festus p. 290 L.). Date and meaning of the plebiscitum are frankly mysterious. It would be surprising if it were earlier than the 'lex Hortensia' of 287 B.C., which gave full legal value to the plebiscita.[68] Granted the reading *curiatim*, the only thing one can say for certain is that when it was enacted, the Curiae were still a vital division of the Roman State.

The preceding evidence suggests the following remarks:

(1) The only certain facts about the Curiae are: (*a*) they were an intermediate group between the tribes and the gentes; (*b*) each Curia included specified gentes. Because each Curia included specified gentes, you actually belonged to a certain Curia by reason of your having been born into a certain gens.

(2) From the fact that the Curiae were made up of gentes, it does not follow that all the members of the Curiae were kinsmen. To begin with there is very little in the ancient evidence to suggest that blood relationship was at the basis of the tribes. It is true that we find the names Ramnes, Tities and Luceres connected with the names of Romulus, Titus Tatius and (with some reservation) Lucumo—an Etruscan general who was supposed to have helped Romulus.[69] But nowhere is it suggested that the Ramnes were descendants of Romulus, the Tities descendants of Titus Tatius and so on. Titus gives his name to the Tities more or less as Opiter Oppius and Caelius Vibenna are believed to have given their names to mons Oppius and mons Caelius. There is not even a clear statement in our sources that the Tities were Sabines and the Luceres Etruscans. Florus may indirectly imply as much (2, 6, 1), but our earlier and more authoritative sources never mention the tribes as representing three different populations. As for the Curiae, it was said that all took their names from the Sabine women raped by Romulus' men:[70] no ethnical distinction was noticed in their midst. When the antiquarian Laelius Felix explains in a famous passage of Aulus Gellius (15, 27, 5) 'cum ex generibus hominum suffragium feratur curiata comitia esse' he does not necessarily equate a Curia to a group of kinsmen. All that he seems to imply is that in the Comitia Curiata votes were cast according to groups of gentes, not according to centuriae or to the Servian local tribes. There remains the comparison of Dionysius of Halicarnassus between the Roman Curia and the Greek *phratria* (2, 7, 3). Without rejecting it off-hand, we may well wonder on what evidence it was based.

[67] Thirty lictors: Cic., *De lege agr.* 2, 31. Comitia calata: A. Gell. 15, 27. G. W. Botsford, *The Roman Assemblies*, 1909, 152–200, provides information on earlier views. Siber in P-W s.v. Plebs (1951), 128–33, is the most recent general survey known to me. K. Latte's attractive theory on the *lex curiata*, *Nachr. Gesell. Göttingen*, 1934, 59, cannot be considered proved (though accepted for instance by K. v. Fritz, *Studies D. Robinson* II, 896), but the Republican origin of the *lex curiata* supported by U. von Lübtow, *Zeitschr. Savigny-Stiftung*, Rom. Abt., 69, 1952, 154–71, and E. Friezer, *Mnemosyne* 12, 1959, 301–29, is quite improbable; cf. the very able discussion by E. S. Staveley, *Historia* 5, 1956, 85–90 (to Staveley's paper I should like to refer for all the problems discussed here). I shall confine myself to one remark. If the (patrician) senators could only choose the 'interrex', not the 'rex', this means that the ultimate appointment of the kings rested with the 'curiae': the *lex curiata*, whatever its interpretation, confirms this. cf. G. Tibiletti, *Studia Ghisleriana* 2, 1, 1950, 359–77, and J. Paoli, *Rev. Et. Anc.* 56, 1954, 121–49.

[68] The Farnesianus reads in Festus s.v. 'Praeteriti senatores' *curiati in senatu*: the alternative emendation is *iurati in senatum*. I take *curiatim* to be more probable (on the history of the two emendations P. Willems, *Le Sénat de la République Romaine* I, 1878, 169–71). The *lex Ovinia* is most usually dated before 312 B.C. (Mommsen, *Staatsr.* II, 418; O'Brien Moore, P-W, Suppl. VI, 686). On Curiae and Senate see also G. Bloch, *Les origines du Sénat romain*, 1883; V. Sinaïski, *La Cité Quiritaire*, Riga, 1923, a fanciful, but acute and original book, and V. Arangio Ruiz, *Le genti e la città*, 1914, with the subsequent discussion registered in P. de Francisci, *Storia del diritto romano* I, 1925 (reprint, 1943), 99 ff.; F. Cornelius, *Unters. zur früheren Römischen Geschichte*, 1940, 89; U. von Lübtow, *Das Römische Volk*, 1955. cf. A. J. Nemirovskij, *Vestnik Dr. Ist.*, 1961, fasc. 1, 178–83.

[69] The connection is already in Ennius (*ap.* Varr., *LL* 5, 55), but apparently only for Tities and Ramnes. Luceres represented a problem (cf. Livy 1, 13, 8); they were connected with Lucumo in Iunius Gracchanus (*ap.* Varr. quoted) and Cicero, *Rep.* 2, 14. Volnius (*ap.* Varr.) believed in the Etruscan origin of the three names. For other evidence and discussion P-W s.v. Luceres (Berve) and Ramnes (Rosenberg). cf. J. N. Lambert, *Studi P. De Francisci* I, 1956, 337–60, and P. de Francisci, *Primordia Civitatis*, 539. The Etruscan form of the names (W. Schulze, 'Latein. Eigennamen', *Abh. Göttingen*, 1904, 218; 263) can hardly be disputed; but we do not yet know whether the names were etruscanized under Etruscan rule or were originally Etruscan.

[70] Cicero, *Rep.* 2, 14; Livy 1, 13, 6; Festus (Paulus) 42 L., etc. Dionys. 3, 9, 6 and 3, 10, 4 presents no difficulty.

(3) There are vague indications in our sources that each Curia had its own territory. Dionysius says so explicitly (2, 7, 4), which in itself for these earlier times does not mean much. But we have seen that the Fornacalia (according to one interpretation of Pliny's passage) included a purification of boundaries and that some of the Curiae had local names. Without pressing the evidence too far, we must bear in mind the possibility that the Curiae were originally territorial divisions to which certain gentes were assigned.

(4) The regularity of the organization—three tribes, thirty Curiae—clearly points to the artificial origin of both tribes and Curiae. Whether the Curiae were meant to be a territorial division or a blood relationship, they had been introduced by a legislator as elements of the machinery of the Roman State. If tradition attributes their creation to Romulus after the struggle with Titus Tatius, it may be right in the sense that the man who created the Roman State is also most likely to have created the Curiae. More precisely, the moment to create the Curiae was that in which the hills were unified in a new political organization.

(5) The original subordination of the Curiae to the State is confirmed by the fact that though each Curia was supposed to provide a centuria for the infantry and a decuria for the cavalry, we hear nothing of the Curiae as military units, and the Curiones were explicitly excluded from the military organization. On the other hand—or rather consequently, given the nature of the Roman State—the Curiones are never presented as magistrates: they do not summon the Comitia Curiata, they do not even preside over the Comitia Calata, they serve the State by keeping the Curiae in working order, and this pre-eminently includes assuring the *pax deorum*.

(6) Finally it seems clear that when the Curiae were established, there was no rigorous distinction between patricians and plebeians, between 'patroni' and 'clientes'. There is no indication in the tradition that the Curiae were reserved for the patricians: only the position of Curio Maximus, not unnaturally, was reserved for a patrician in republican times up to 209 B.C. If each Curia had to provide a centuria for the infantry it cannot have been made up of patricians only. There is furthermore the tradition that in the first years of the plebeian agitation the Tribuni Plebis were elected in the Comitia Curiata (Cic., *Corn.* 60 Stangl; Dionys. 6, 89, 1; cf. Livy 2, 58, 1). This tradition at least proves that Roman antiquarians believed the Curiae to have been open to the plebeians. According to our evidence, the Curiae are elements of a State in which the gentes were not yet divided into two clear-cut groups, patricians and plebeians.[71]

(ii) The preceding remarks, I need hardly say, are very unsatisfactory. They certainly suggest the existence of a creator of Rome—you may call him Romulus or otherwise—but do not explain how the Curiae behaved in relation to individual gentes, how the levies of soldiers were made within the Curiae, how the Curiae co-operated with the kings and finally how the distinction between patricians and plebeians developed within the Curiae. The nature of the Roman Senate has also remained unexplained, though we have seen that even in the fourth or third century B.C. there was still some awareness of its organic connection with the Curiae. Did the king who fixed the Senate at 300 members intend to choose ten *patres* from each Curia? Traces of decuriae are still to be found in the tradition about the Roman Senate.[72]

At this point we may ask whether the comparative study of primitive institutions can supplement our knowledge of early Roman society and particularly of the Curiae. I have gone as far as I could go on the Roman evidence alone. If there is any hope for me, it is outside Rome.

[71] First 'curio maximus' known to us: Ser. Sulpicius Camerinus who is said to have died in 463 B.C. (Livy 3, 7, 6, on which Münzer, P-W 4 A, 747). cf. Livy 27, 8, 1, and generally Kübler in P-W s.v. *curio*. The existence of a 'curio maximus' as the head of the Curiae implies a certain dualism between king and Curiae (populus), which is obvious also in the case of 'regifugium'—'poplifugia'. But the social and political implications of such a dualism escape us entirely. V. Basanoff, *Regifugium*, Paris, 1943, is not sound. cf. also the interesting paper by R. Schilling on Romulus, *REL* 38, 1960, 182–99.

[72] See Kübler in P-W s.v. decuria; Mommsen, *Staatsrecht*[3] 1, 656; III, 529 and 852. cf. Livy 1, 17; Dionys. 2, 57; Serv., *Aen.* 6, 808; Cic., *Verr.* 2, 2, 32, 79, etc. For the relation between the 'decuriae' of the Roman Senate and the 'decuriones' in 'municipia' cf. Marquardt, *Staatsverw.* 1[2], 184.

I have read my Malinowski, Evans-Pritchard and Fortes, Schapera, Lévi-Strauss and so on, but I have not found in them much to shed light on the Roman Curiae. My failure must not be construed as a denial of the value of comparative studies. Comparative anthropology is more likely to indicate alternative possibilities of interpretation for the evidence we have than to supplement the evidence we have not. I shall give two examples of what comparative anthropology can do. We have the evidence for Roman clientship, though we have no valid explanation of this institution in our ancient sources. There are several African societies, for instance that of the Gusii described by P. Mayer, which can provide a model for explaining the origins of clientship.[73] The Roman tradition tells us that aliens became kings in Rome without actually conquering it. Classical historians have sometimes found it difficult to believe this story, but again there are plenty of accounts of immigrant rulers in modern anthropological literature.[74] In our case comparative anthropology can be useful only in so far as it reinforces the contention that there is no need to assume that the Roman tribes were founded upon kinship. As Professor I. Schapera says of Bantu tribes: 'the tribe is essentially a body of people all paying allegiance to the same chief. They certainly do not visualize it as a group of kin, not even in terms of residence.'[75] But what we need for the Curiae is not a model of explanation. We need the primary evidence—which no comparison can supply.

There is in truth a special type of cultural anthropologist who would be only too glad to be accepted as a help on this occasion. As is well known, Professor G. Dumézil, the eminent member of the Collège de France, has insistently claimed in the last twenty years that he can explain the nature of Roman tribes and Curiae and much besides by comparing them with Indian institutions and recovering their original meaning. Professor Dumézil is constantly improving his own system, and the later version of his theories—to be found in the Italian translation of his *Jupiter Mars Quirinus* (1955) and in the Upsala lectures *L'idéologie tripartie des Indo-Européens* (1958)—jettisons many of the previous arguments.[76] Yet even the more recent statements of Professor Dumézil's theories are to my mind unconvincing. Dumézil is certain that he has discovered a philosophy of tripartition in the Indo-European mind. According to him, the Aryan ancestors were firm believers in hierarchy. In the social order hierarchy would be expressed by the presence of three diversified tribes. In Rome the Ramnes represented the priests, the Tities the producers and more especially the agriculturalists, while of course the Luceres were the warriors. In religion Jupiter, Quirinus and Mars would look after the three functions, and the three corresponding flamines would be their symbols. The best direct evidence Professor Dumézil can produce for his tripartition in Rome is the poem by Propertius IV, 1. Why Propertius should have been endowed with a knowledge nobody else possessed we are not told.[77] But it is enough to read again that delightful poem of self-irony to see how weak the evidence is. Propertius clearly implies that Rome had warriors and peasants even before Titus Tatius and Lucumo joined in to create the tripartite state: Propertius was near enough to his Italian earth not to imagine otherwise. It is not superfluous to add that Professor Dumézil is equally unfortunate in his non-Roman evidence. In their pastoral solitude the Scythians may be supposed to have remained good Indo-Europeans uncontaminated by Oriental despotism—or Mediterranean democracy. Professor Dumézil, naturally enough, makes much of them. He therefore quotes from Herodotus (IV, 5–6) the story of how a plough, a yoke, a spear, and a bowl of gold fell from the sky, and he who took them became the king and was the ancestor of the royal tribe among the Scythians. Dumézil manages to reduce the four objects to three and to make them the symbols of the three functions. But there is no indication in Herodotus that each Scythian

[73] P. Mayer, *The Lineage Principle in Gusii Society*, London, 1949. cf., however, the critical remarks in *Man*, December 1950, no. 264.

[74] It will be enough to refer to L. Mair, *Primitive Government*, London, 1962, ch. 5, 'The Immigrant Rulers.'

[75] *Government and Politics in Tribal Societies*, London, 1956, 28.

[76] For a longer list of Dumézil's works see his *L'idéologie tripartie*, 1958, 92. Among his recent pronouncements notice his discussion of de Francisci's *Primordia* in *Rev. Belge Phil.* 39, 1961, 62–7, and his reflections in *REL* 39, 1961, 87–93; also *Rev. Hist. Rel.* 154, 1958, 1–9; 157, 1960, 141–154; *Latomus* 13, 1954, 129–139; 17, 1958, 429–446.

[77] The point is developed at length in *Jupiter Mars Quirinus*, Ital. ed., 90–107, 230–63 (391–405: Dumézil's views on the Curiae and further references on Quirinus). See also *L'idéologie tripartie*, 13.

tribe got one of the gold objects and organized its own activities in accordance with that symbol. He rather implies the contrary: all the objects ended in the hands of the king. Quintus Curtius, who retails a similar story, states definitely that the objects fallen from the sky, which are in this case five, were the common possessions of all the Scythians (7, 8, 17–18). There is no suggestion in the Scythian story of a functional division of tribes.

My objection to Dumézil's views (and I know that I am not alone in this) is not only that his evidence is weak, but that his theories are unnecessary.[78] Nothing is explained in Roman history if we believe that in a prehistoric past Roman society was governed by a rigorous separation of priests, warriors and producers. The fundamental fact of Roman society remains that warriors, producers and priests were *not* separate elements of the citizenship, though priesthoods tended to be monopolized by members of the aristocracy.

There is more reason in the less well-known attempt by Francisco Rodriguez Adrados to interpret Roman institutions in the light of a 'sistema gentilicio decimal de los Indo-Europeos occidentales' (1948). Professor Adrados believes that Greeks, Celts, Germans, Osco-Umbrians and Latins had in common a political and military system, according to which the tribe included 100 gentes, the *phratria* or Curia ten gentes. Adrados himself admits that the evidence for the Greeks and the Celts is tenuous. The evidence is certainly much stronger for the Germans among whom divisions according to thousands and hundreds were already noticed by Caesar (for instance *BG* 4, 1) and Tacitus (*Germ.* 6; 12). The question, however, is how much we can learn about Early Rome from such rather obvious similarities in structure. What do the Germans add to our knowledge of the Roman Curiae? Professor Adrados puts himself out of court when he concludes 'de aquí debemos deducir que la atribución de 100 hombres a la curia y 10 a la gens son innovaciones romanas'.[79] By admitting that Rome only had a division by Curiae corresponding to centuriae of soldiers, he deprives the parallels of any really useful function.

If a fruitful comparison is to be sought for the Roman political structure, it must be within the boundaries of ancient Italy. In Latium and Etruria, among the Oscans and the Umbrians, there were states which had in common with Rome much of the material and intellectual culture, not to speak of the military conflicts and political alliances which interconnected them. For some of these communities we can even postulate close similarity of language with Rome. It was Arthur Rosenberg's great merit in his pioneer work of 1913, *Der Staat der alten Italiker*, to concentrate on the similarities between the Roman institutions and other political institutions of ancient Italy. There are obvious difficulties in this task. Normally we are better informed about the Romans than about the others. Whereas Roman literary evidence supplements epigraphical evidence, we are left almost exclusively with short inscriptions for the other Latins, the Etruscans and the Oscans. Furthermore almost all the non-Roman evidence is later than the archaic stage of Rome with which we are here dealing. Prima facie one would argue that the Roman evidence is more likely to clarify the institutions of the other Italian groups than vice versa. Yet this is not quite so. Rome was the great centre of political innovation in Italy. The other places, by losing the initiative, either accepted the more recent Roman institutions or stuck to their own old-fashioned ones. There are archaic features in non-Roman evidence, however late, which can turn out to be useful. It is true, as I have said, that by far the greatest part of the non-Roman evidence is represented by short epigraphic texts, but there is at least one long text of institutional character, the Tabulae Iguvinae. I propose to confine myself to a brief examination of this text because I feel it can tell us something about the relation between Curiae and noble families: which is another way of putting the question of the origin of the patriciate.

[78] My views on Dumézil (which I hope to develop in an article in *Rivista Storica Italiana*) are not far from those of A. Piganiol, *Histoire de Rome*, ed. 1954, 529–30. cf. H. J. Rose, *JRS* XXXVII, 1947, 183–6; A. Brelich, *Studi Mat. St. Relig.* 28, 1957, 113–23; K. Latte, *Acta Congressus Madvigiani* 1, 1958, 216; J. Brough, *Bull. School Oriental and African Studies* 22, 1959, 69–85; J. Gonda, *Mnemosyne* 4, 13, 1960, 1–15; F. B. J. Kuiper, *Numen* 8, 1961, 34–45, for criticism of Dumézil's views by specialists in different fields. It is only fair to add that Dumézil has many and authoritative followers especially in France, has deeply influenced J. Bayet's more recent work (cf. already *Rev. Hist. Relig.* 126, 1943, 159–66) and is supported by one of the greatest living philologists, E. Benveniste (cf. *Rev. Hist. Rel.* 129, 1945, 5–16 etc.).

[79] *El sistema gentilicio decimal de los Indo-Europeos occidentales y los origenes de Roma*, Madrid, 1948, 124. But cf. G. Devoto, *Origini indoeuropee*, Firenze, 1962.

(iii) Like the laws of Gortyna, the Tabulae Iguvinae perhaps owe something of their fascination to the double fact of being preserved near the place where they originally stood and of being very difficult to understand.[80]

The date of the Tabulae themselves is uncertain. It is a mere guess that the earlier tables in the Etruscan script belong to the second century B.C. and that the tables in the Latin script were written after the Social War. Even if these dates were certain, they would not solve the problem of the date of the religious regulations contained in the tables. Fortunately for us, the question of the date is not of primary importance. Whatever the date or dates, the tables are evidence for a type of society in which something like the Roman Curiae were still in full operation, not relics of the past, but living units.

Like the Roman Fratres Arvales, the Fratres Atiedii of Iguvium in Umbria had their Acta, but instead of registering the performance of individual ceremonies, as the Fratres Arvales did, they recorded their own regulations and by-laws. Like the Fratres Arvales the Fratres Atiedii were a corporation acting on behalf of the Iguvine State in religious matters.[81] The two most important ceremonies they performed were the purification of the acropolis, called Fisian Mount, and the lustration of the people of Iguvium. The purification had its centre in the sacrifices at three gates. From the lustration foreigners were formally banished: 'Whoever is of the Tadinate people, of the Tadinate tribe, of the Tuscan, the Narcan, the Iapudic name, let him go out from this people' (transl. J. W. Poultney). But the whole male adult population of Iguvium was of course supposed to be present: 'Arrange yourselves in priestly ranks and military ranks, men of Iguvium' (table VI). The distinction between military ranks and priestly ranks, if that is the correct translation, is worth noticing. We have seen that the Curiones were past military age and had priestly functions. We are also reminded of the slender, yet respectable, evidence according to which the Fratres Arvales performed one of the most important lustrations in Rome—the Ambarvalia—before being ousted by the pontiffs.

The Fratres Atiedii were probably life members of the corporation like the Fratres Arvales, and like the Fratres Arvales they seem to have been twelve in number. Though in table VII, b, 1–2, the number XII probably refers not to the Fratres themselves, but to the victims they must sacrifice, it seems fairly obvious that each brother had to sacrifice one victim. There is some difficulty in accepting the number XII for the Atiedii because one passage, according to the most authoritative interpretation, says: 'then the brothers rising in groups of five shall elect an auctor who shall be in accord with the customs of the brothers in assembly' (III, 4–5).[82] If the Fratres Atiedii were divided into pentads, the number XII is in danger. But even accepting this translation, it could be argued that two of their most important officers, the Fratricus and the Adfertor, were outside the pentads. This brings us to the question of the names of magistrates mentioned in the tables. I can start with declaring my belief that the four officers mentioned—the Fratricus, the Auctor, the Quaestor and the Adfertor—are all officers of the corporation, not magistrates of the city of Iguvium.[83] True, both the Auctor and the Quaestor alternately appear as eponymous, but that does not yet make them city magistrates, nor does it prove

[80] Together with the old classics (among which F. Buecheler's *Umbrica*, 1883, has still a place of its own), I have used the following recent editions: G. Devoto, 3rd ed., Rome, 1962, and his ed. minor with an Italian transl., Florence, 1948; E. Vetter in *Handbuch der Italischen Dialekte* I, 1953, 170; V. Pisani, *Le lingue dell'Italia antica oltre il latino*, Torino, 1953, 121; G. Bottiglioni, *Manuale dei dialetti italici*, Bologna, 1954, 259; J. W. Poultney, 'The Bronze Tables of Iguvium', *Am. Phil. Ass. Mon.*, 1959. cf. also A. Ernout, *Le dialect ombrien*, Paris, 1961. Survey of research 1940–59 by J. Untermann, *Kratylos* 5, 1960, 113–25: cf. K. Olzscha, *Glotta* 41, 1963, 70 ff. G. Radke, P-W Suppl. IX, s.v. Umbri.

[81] On J. Gagé, *Huit recherches sur les origines italiques et romaines*, Paris, 1950, it will be enough to refer to P. Fraccaro, *Gnomon* 25, 1953, 14–18. But I found very useful G. B. Pighi, 'Umbrica', *Mem. Accad. Bologna* 4, 5, 1953; 'De Atiediorum cletra', *Studi G. Funaioli*, 1955, 373–7; 'I nomi delle divinità iguvine', *Riv. Fil. Class.* 32, 1954, 225–61.

[82] The translation quoted in the text is by J. W. Poultney. For discussion cf. U. Coli, *Il diritto pubblico degli Umbri e le tavole eugubine*, Milano, 1958, 46.

[83] On this point I agree with U. Coli, *Il diritto pubblico degli Umbri*, 37–50: cf. also J. H. Waszink in his important discussion of Coli, *Tijdschrift voor Rechtsgeschiedenis* 29, 1961, 346. For the opposite view cf. most recently G. Camporeale, *La terminologia magistratuale nelle lingue osco-umbre*, Florence (*Atti Accademia Colombaria*), 1957, 80. In general cf. I. Rosenzweig, *Ritual and Cults of Pre-Roman Iguvium*, London, 1937; G. Devoto, *Gli Antichi Italici* 2nd ed., 1951, 247–80; P. G. Scardigli, 'Studi sulla III^{a} e IV^{a} Tav. di Gubbio', *Studi Etr.* 25, 1957, 267–301, and 26, 1958, 155–84, and C. Gioffredi's discussion of Coli in *Labeo* 4, 1958, 351–8. One may compare S. Mazzarino, 'Le droit des Etrusques', *Iura* 12, 1961, 24–39.

that the Auctor and the Quaestor are identical. As a matter of fact we have seen that table III contains a rule which, though difficult to understand, seems to refer to the election of the Auctor by the Atiedii. And table V mentions the Auctor and the Quaestor as two different persons.

What then was the relation of the Fratres Atiedii to the Iguvine State, if the officers of the corporation were not magistrates of the State ? The city of Iguvium, unlike Rome, was not divided into tribes. The word *trifu*—tribus—indicated the territory of the city. This means that when the Umbri occupied the land, each tribe developed its own city centre. A territory called ' tribus Sapinia '—evidently an independent community in Umbria—is mentioned twice by Livy (31, 2 ; 33, 37).[84] The population of Iguvium, according to our tables, was apparently divided into ten units. The majority of the units was later doubled, resulting in the Clavernii and the second Clavernii, etc. One of the units was trebled (Casilas, second and third Casilas), but the Peraznanii remained single. One is of course reminded of the Roman *centuriae equitum* which were doubled into ' Ramnes, Tities, Luceres primi, secundi '. One of the units was called Atiedii and included or operated the Atiedian brotherhood. The other units were in some sort of contractual relationship with the Atiedian brotherhood in the matter of ceremonial banquets. Each unit had its own residence or territory from which messengers of the Fratres Atiedii fetched food.[85] The name of the individual units seems to have been partly local, partly gentilicial. There can be little doubt that the original ten units were subdivisions of the one city tribe, comparable with the ten Curiae of the Roman tribe. The name decuria has been attributed to these units with good reason. The words *tekuries* and *tekvias* appear in the Iguvine tables just before the list of the units. Both words seem etymologically connected with *decem*. But the truth is that no satisfactory interpretation has so far been provided of the lines in which *tekuries* and *tekvias* appear (II, b, 1–2).

seme : nies ; tekuries : sim : kaprum :
upetu : tekvias : famerias : pumperias : XII

Professor Poultney, following Devoto, translates : ' At the decurial festival of Semo choose a pig and a he-goat. The decuriae and the quincurial families are twelve.' The translation of the second sentence is, however, very doubtful. The word *pumperias* in the famous Iuvilas inscriptions of Capua [86] has been authoritatively interpreted as a day of the month, something like ' nonae '. On this analogy, ' Pumperias XII ' in our Iguvine tables looks to me, as it looked to A. von Blumenthal, like an honest date, XII being the month.[87] That leaves *tekvias* and *famerias* as subject of the verb *upetu*. But if *famerias* is the Latin *familiae*, *tekvias* remain mysterious. It is more important for our purpose to discover whether the units contained specified gentes. Here the answer is positively provided by the very composition of the corporation of the Atiedii. The tables register a sacrifice to the god Hondus Iovius specifically for the gens—or natio—Petronia of the Fratres Atiedii : *petruniaper natine fratru atiieriu* (II, a, 21) ; also a sacrifice to Iupiter for the gens ' Vucia ' of the Fratres Atiedii (II, b, 26). There was a special relationship between the Petronii and Hondus Iovius and between the ' Vucii ' and Iupiter to be compared with the special

[84] Earlier discussions in E. Täubler, ' Die umbrisch-sabellischen und die römischen Tribus ', *Sitzungsb. Ak. Heidelberg*, 1929–30, 4 ; A. Momigliano, *Bull. Comm. Arch. Com.* 60, 1933, 228. Recent discussions : J. N. Lambert, *Studi P. de Francisci* I, 1956, 339–60 ; P. de Francisci, *Primordia Civitatis* 537 ; U. Coli, *Diritto pubblico degli Umbri* 69 ; Ernst Meyer, *Römischer Staat*, 461 ; G. I. Luzzatto, *Dalla tribù allo Stato: Atti del Convegno Internazionale*, Accad. dei Lincei 1962, 193–234, with full bibl. cf. ibid. p. 191, my remark on *Syll.*[3] 167.

[85] Vb 13–15 is translated by Poultney (the translation is fairly uncontroversial ; cf. the illuminating commentary by Buecheler, *Umbrica* p. 41) : ' The (decuvia) Casilas is required to give to the Atiedian Brothers each year six pounds of choice spelt from the Ager Casilus of Picus Martius, and dinner for the two men who come to fetch the spelt or else (to give) six asses.' cf. Festus s.v. ' sex Vestae sacerdotes ', p. 468 L.

[86] E. Vetter, *Handbuch der Ital. Dialekte* I, 70. Contra J. Heurgon, *Étude sur les inscriptions osques de Capoue dites Iuvilas*, Paris, 1942, 72 : ' à Capoue la population, lors de certaines fêtes qui recevaient de là leur nom, se groupait en quincuries.' I cannot follow Coli, *Diritto pubblico degli Umbri*, 51–68. cf. also E. Vetter, *Jahresh. Oesterr. Archäol. Inst.* 39, 1952, Beiblatt, 98–102.

[87] A. von Blumenthal, *Die iguvinischen Tafeln* 1931, 38–40, substantially followed by Pisani, *Lingue dell'Italia antica*, 197, and now, changing his earlier interpretation, by Devoto, *Studi Etruschi* 22, 1952, 169–73. For the difficulties see Poultney, *Bronze Tables* 192. Another suggestion by G. Radke, P-W Suppl. IX, 1806, s.v. Umbri. Memorable the interpretation by W. Schulze, ' Lat. Eigenn.', *Abh. Göttingen*, 1904, 543–7. cf. now also A. Ernout, *Dialect ombrien* 63.

devotion of the Fabii to Hercules,[88] if not with the part of the Potitii and Pinarii in the cult of the same Hercules.

To sum up, the Fratres Atiedii were a religious corporation connected with one of the original ten divisions (perhaps decuriae) of the city of Iguvium. Two gentes had a fixed place and a special importance in the brotherhood; specific cults were connected with them. In other words, in Iguvium one of the Curiae provided the priestly corporation of Fratres Atiedii to whom the religious purity of the city was entrusted. But this corporation reflected the varying importance and the specific religious interests of its members, some of whom were regularly chosen from a particular *gens*.

The connection between religious corporations and tribes and Curiae was no longer evident in Rome. The Fratres Arvales do not appear to have been connected with any Curia. Only the number six served as a reminder of the relation between the Vestals and the three tribes. Also the relation between augurs and tribes had almost disappeared. If the Sodales Titii had been an emanation of the tribe of the Tities, as the Fratres Atiedii were an emanation of the unit of the Atiedii, only the name preserved a trace of the relationship between the Sodales and the tribe.[89] The existence of a relation between the Curia Acculeia and the goddess Angerona is only briefly mentioned by Varro (*LL* 6, 23). On the contrary, we can clearly see Roman aristocratic families developing religious corporations of their own, outside the Curiae but recognized by the city. The Luperci Fabiani and Quinctiales are the most famous case in point.

What Iguvium provides is that stage of development in which individual Curiae were still fully operating their religious corporations, while individual cults of especially powerful families began to emerge. The *Tabulae Iguvinae* teach us that the existence of units like the Curiae was no obstacle to the formation of a small aristocracy which would control religion just as it would control military power. The Petronii and the 'Vucii' of Iguvium are still contained within the decuriae. In Rome the Fabii, the Quinctii, and the Pinarii are without, fending for themselves.

(iv) After Hugh Last's famous paper [90] it has now become fashionable to say that the distinction between patricians and plebeians developed after the end of the monarchy. Some facts are plainly in favour of this theory. Four of the seven Roman kings belonged to gentes which later were considered plebeian (Pompilius, Hostilius, Marcius, Tullius). Three *montes* of the Septimontium derived their names from plebeian gentes (Oppius, Caelius, Cispius).[91] Even in the early years of the Republic there appear names of consuls belonging to plebeian gentes, for instance Iunius Brutus and Sp. Cassius. On any reckoning some of the gentes which were considered plebeian in later times had belonged to the ruling class earlier. Furthermore, the Senate seems to have been always open to the plebeians. Certainly no sharp distinction existed between patricians and plebeians when the monarchy ended. But on the evidence we have it is difficult to believe that the Roman tradition went astray when it attributed the separation of the orders to the monarchic period. As is well known, the *patrum auctoritas* and the nomination of the interrex were privileges of the patrician senators in republican times.[92] It is easy enough to believe that after the fall of the monarchy the patricians could, and did, usurp the right of veto on laws and elections made by the Comitia (which is what the *patrum auctoritas* amounted to). But it is hard to see the point of the patricians usurping in republican times the privilege of appointing the interrex—a clear survival, even in name, of the monarchic period. Objections to such a change in the name of the 'mores maiorum' would have been very strong.

The appointment of the interrex by the patrician senators looks like a privilege that

[88] A useful list of such cults in C. W. Westrup, *Rev. Int. Droits Antiq.* 3, 1, 1954, 446. For the Fabii Münzer in P-W s.v. Fabius, 1740. On the Potitii cf. now the revolutionary thesis by D. van Berchem, 'Hercule Melqart à l'Ara Maxima', *Rend. Pontif. Acc. Arch.* 32, 1959–60, 61–8, supported by A. Piganiol, *Hommages à A. Grenier*, 1261–1264.

[89] S. Weinstock in P-W s.v. Titii Sodales. cf. Mommsen, *Ges. Schriften* IV, 34 (1886). The evidence on the Luperci in Marbach, P-W s.v.

[90] *JRS* XXXV, 1945, 30–48.

[91] C. Hülsen, *Rend. Pont. Acc. Rom. Archeol.* n.s. 2, 1923–4, 83–6. cf. in general B. Kübler in P-W s.v. Patres.

[92] For the controversy on these points see bibl. in P. Catalano, *Contributi allo studio del diritto inaugurale* 455, but Cicero, *De domo* 14, 38, and Livy 6, 41, 6–10, are unambiguous evidence.

certain families managed to extract from the kings or from their fellow senators when the kings still existed. This privilege may even have been the first legal definition of the patrician group among the senators.[93] The same argument applies to the membership of certain religious institutions (notably the Salii) which were of very little importance during the Republic and yet were confined to patricians. Though there is just a chance that the patricians usurped the privilege of exclusive membership of the corporation of the Salii during the first decades of the Republic, the more reasonable explanation is that their privilege dated from the monarchic period. The same families who appropriated the right to appoint the interrex must also have monopolized certain religious institutions which prospered in the monarchic period and declined later. After the fall of the monarchy the same families had ample opportunity for consolidating their powers in more strictly political terms. If they were not actually the gentes which played the most conspicuous part in bringing down the monarchy, they were at any rate the best equipped to exploit the new situation—perhaps by eliminating agitators such as Sp. Cassius. My contention is that the patriciate emerged from the Curiae by gradually monopolizing religious and political rights until the crisis of the monarchy gave the opportunity for a wholesale taking over of the Roman State. Religious authority was the first and the easiest to monopolize because it implied some special knowledge and some leisure and required that respectability aristocrats always have. Religious authority was indeed what the Roman patricians traditionally tried to keep for themselves: 'nobis adeo propria sunt auspicia' (Livy 6, 41, 6).

It must have been very difficult for the Roman kings to keep their subjects quiet within the accurately framed Curiae. Increasingly prosperous and increasingly civilized, Rome was also increasingly restless at the crossroads between the Etruscans of Etruria and those of Campania.[94] I do not see any reason to doubt that Ostia had been reached and transformed into a harbour by Ancus Marcius. Greek and Carthaginian merchants may already have reached Rome, but it was above all the pressure from Etruscan cities that made life in Rome unstable.[95] If an Etruscan aristocrat of doubtful origins, Tarquinius Priscus, managed to have himself accepted as a king in Rome, others must have been equally enterprising, though less successful. One of them was perhaps the Cn. Tarquinius of the Vulci painting. Both Roman and Etruscan tradition remembered the brothers Aulus and Caelius Vibenna whose existence is now epigraphically confirmed.[96] Etruscan tradition remembered also their friend Mastarna who was sometimes identified with Servius Tullius. There were all the conditions for local aristocrats imitating the foreign chieftains, creating their own bands, organizing their own clientes and taking part in the political struggles. Nothing was remembered of what the Fabii had done during the monarchy.[97] Only their control of one of the Luperci groups was rightly connected with

[93] The same *patres* who monopolized the right to appoint an interrex may also have acquired the right to be succeeded automatically or almost automatically by their sons in the Senate—thence the name 'patricii' (cf. Mommsen, *Röm. Forsch.* 1, 228 n.). Alföldi has shown that the members of the aristocracy were originally members of the equestrian centuries and dressed like knights, *Der frührömische Reiteradel und seine Ehrenabzeichen*, 1952 (cf., however, the important remark by Ernst Meyer, *Gnomon* 25, 1953, 186, and the criticisms by F. Altheim, *Röm. Gesch.* II, 1953, 429–443).

The position of the 'equites' in early Rome needs further research: J. Wiesner, *Klio* 36, 1943, 45–100; and the speculative J. Gagé, *Rev. Hist. Droit Franç. Étr.* 4, 33, 1955, 20–50; 165–94. For the early traditions of the 'equites' see also S. Weinstock, *St. Mat. St. Rel.* 13, 1937, 10–24. Ernst Meyer's acute suggestion that *procum patricium* must be understood as *p. et p.* (*Röm. Staat*, 491, n. 80) is not likely. cf. Hor., *AP* 342 'celsi . . . Ramnes'.

[94] On economic life during the monarchic period see F. Tamborini, *Athenaeum* 8, 1930, 299–328 and 452–87; B. Combet-Farnoux, *Mél. Éc. Rome* 69, 1957, 7–44; R. Giunter (Günther), *Vestnik Drevnej Istorii*, 1959, fasc. 1, 52–83. Memorable is G. Pasquali, 'La grande Roma dei Tarquinii' (1936), now in *Terze Pagine Stravaganti*, Florence, 1942, 1–24. On the alleged friendship with Massalia (Iustin. 43, 3; Strabo 4, 180), see G. Nenci, *Rivista Studi Liguri* 24, 1958, and F. Benoît, *CRAI* 1961, 159–173.

[95] A. Blakeway, *JRS* XXV, 1935, 129–49. Blakeway must be quoted here to remind ourselves that his research made unreal even the best juridical definition of Roman monarchy such as that offered by U. Coli, *Regnum*, Rome, 1951. More is to be learnt from several articles by A. Bernardi, among which *Athenaeum* 31, 1953, 273–87; *Riv. Storica Ital.* 66, 1954, 5–20. cf. also M. Pallottino, *La Parola del Passato* 47, 1956, 81–8; J. Bayet, *St. Etruschi* 24, 1955, 3–17.

[96] G. Radke, P-W s.v. Vibenna (VIII A, 2454–7). cf. L. Pareti, *Studi Etruschi* 5, 1931, 154–61; S. Mazzarino, *Dalla monarchia allo stato repubblicano*, 1945; U. von Lübtow, *Das Römische Volk*, 1955, 166–231; J. Heurgon, *La vie quotidienne chez les Étrusques*, 1961, 63–8.

[97] This is perhaps not irrelevant if one tries to assess Fabius Pictor's honesty as a historian: to the best of our knowledge he did not attribute any role to his family in the monarchic period, though he must have known (from the Luperci Fabiani) that the Fabii were as old as Romulus.

the origins of Rome. But they emerged at the beginning of the Republic as the most powerful gens with the best retinue of clientes. They must have reached this position under the last kings. Arriving in Rome at the beginning of the Republic with all their clientes, the Claudii knew they would be received by a society of peers.[98]

The impending formation of the patriciate (which in military terms meant a strong cavalry) was bound to disrupt the Curiae and to fill the military centuriae with clients of the leaders. Had the process gone unchecked, the ultimate result would have been the formation of permanent private armies.

All we know for certain is that this process was somehow arrested and that the Roman State emerged from the crisis with a new military and political organization which we call the Comitia Centuriata. One of the most conspicuous consequences of this reform was to give Rome an army on the lines of the Greek phalanx. I venture to suggest that Servius Tullius, to whom tradition attributes this reform, did in fact introduce hoplite tactics in Rome while curbing the anarchic tendencies of the Romano-Etruscan aristocracy. But before I proceed, I must point out what I think is one of the least noticed difficulties in any interpretation of the origins of the Comitia Centuriata. It was the considered opinion of the Romans themselves (as testified by the *Ineditum Vaticanum* 3 and by Diodorus 23, 2) that they had imitated the Etruscans in fighting χαλκάσπιδες καὶ φαλαγγηδόν.[99] How the Etruscans ever managed to combine an army of hoplites with their social structure founded upon a sharp distinction between nobles and clientes I cannot imagine.[100] Nor can I say when and how the Etruscans influenced the Roman military organization. The history of the diffusion of the phalanx in archaic Italy has still to be written, if it can be written at all. But if we are prepared to believe that the Etruscans transmitted the phalanx to the Rome of Servius Tullius, we must also be prepared to believe that what Servius Tullius did in Rome other kings tried to do in Tarquinia or Vulci or Caere—with less permanent success or perhaps only with less influence on later historical tradition.

Features of the Servian organization were: (1) the partial—or rather progressive—replacement of the Curiae in the popular assembly by the straight military unit, the centuria: this was presumably meant to give greater power to the soldiers than to the priests and altogether contributed towards greater discipline; (2) more important, the grouping of the centuriae into classes according to census: this was certainly meant to give greater power to the middle class hoplites in comparison with the aristocrats filling the cavalry. We may add that the centuriate organization, if combined, as we are told it was, with the creation of new local tribes, was ideally suited to introduce new men into the Roman citizenship or rather to transform ambiguous clients of individual aristocrats into clearly classified members of the Roman *civitas*. Seen in these terms, the reform attributed to Servius Tullius was both an attempt to modernize the Roman army under Greek and Etruscan influence and to re-establish order in the Roman State, at a time when Etruscan examples and natural economic developments had made the Curiae ineffectual against the rise of what in later times was to be known as the patriciate.

It has often been argued that the Servian reform must be either more ancient or more recent than the rule of the patriciate because it does not refer to it.[101] But a third

[98] Münzer, P-W s.v. Claudius, 2663. cf. Mommsen, *Röm. Forsch.* I, 293; A. Alföldi, *Antidoron E. Salin*, 127 (with whom I do not agree).

[99] E. S. McCartney, 'The military indebtedness of early Rome to Etruria', *MAAR* I, 1917, 121–67; E. Meyer, *Kleine Schriften* II, 231; F. Altheim, *Röm. Geschichte* II, 1953, 157–69. H. R. W. Smith, 'Votive Religion at Caere', *Univ. California Public. Class. Arch.* 4, 1, 1959, 13, speaks of 'Roman army etruscanized by Servius Tullius'.

[100] This is a point which I should like to see discussed in the books about Etruria. Did the decline of monarchy precipitate a far more serious crisis of the hoplitic organization in Etruria than in Rome? On the decline itself J. Heurgon, *Historia* 6, 1957, 63–97; R. Lambrechts, *Essai sur les magistratures des républiques étrusques*, 1959, 22. cf. O.-W. v. Vacano, *Die Etrusker in der Welt der Antike*, 1957, 154–6, and G. Camporeale, *Parola del Passato* 13, 1958, 5–25, which I do not find convincing. On the Etruscans in Rome F. Schachermeyr, P-W s.v. Tarquinius (1932), is still the starting point for further research.

[101] So for instance A. I. Nemirovskij, *Vestnik Drevnej Istorii*, 1959, fasc. 2, 153–65, now summarized in *Bibl. Class. Orient.* 8, 1963, 171–6. For an authoritative statement of the case for a date around 450 B.C., M. P. Nilsson, *JRS* XIX, 1929, 1–11. cf. E. Schönbauer, *Historia* 2, 1953, 21–49; U. Coli, *Studia et documenta historiae et iuris* 21, 1955, 181–222. The Roman tradition, for what it is worth, presupposes the presence of the phalanx at the well remembered battle of Lake Regillus: Dionys. 6, 10, 2. Other literature in L. R. Taylor, *AJPhil.* 78, 1957, 337–54.

possibility is worth considering, namely, that it was an attempt to stop the separation of the orders. Such an interpretation is the most natural if one attributes the Servian reform to Servius, which is after all what the tradition says. Those who want to make the centuriate organization contemporary with the creation of the censorship about 440 B.C. have to explain why tradition remembered the creation of the censorship at that date, but not the far more spectacular ' Servian ' reform. Fraccaro has proved that the Servian reform presupposes a legion of sixty centuriae, that is 6,000 men.[102] Later the original legion was divided into two legions, each with sixty centuries of sixty (or thirty) men. The oddity of a century of sixty men can only be explained if the original was divided into two without a serious increase in the total number of soldiers. As was seen long ago, the most propitious moment for the division of the legion into two was the creation of two consuls instead of one king. Thus the most natural date for the Servian reform is in the monarchic period—which is an independent confirmation of the tradition. But if the original curiate legion was 3,000 infantrymen and the new centuriate legion was 6,000, there must have been a conspicuous increase in the available men at the time of the reform. This confirms that the Servian reform was accompanied by an extension of citizenship. We are reminded of the traditional datum that King Servius was particularly friendly towards the Latins and anxious to attract them to the newly-built sanctuary of Diana on the Aventine : the Aventine was bound to remain in the years to come the anti-patriciate centre of Rome.

It would be foolish to maintain that Servius organized the classes exactly as they appeared centuries later, when Rome had a money economy and a much more complex social structure. Tradition itself implies a more simplified form of centuriate organization when it tells us that the first Servian class was called *classis*, and the four others were collectively called *infra classem*.[103] The ancients themselves were puzzled by the expression. Its most likely explanation is that Servius Tullius originally put all the sixty centuries of the legion into a group called *classis*—and all the centuries of the lighter troops and of the non-combatants into another group, the *infra classem*. The knights remained of course above the centuries of the *classis*. In this view, which is entirely hypothetical, the original Servian organization contemplated a simple division between ' equites ', ' classis ' and ' infra classem '. In the same view nothing prevents us from believing that the ' infra classem ' were already subdivided by Servius Tullius into several groups, one of which was composed of ' proletarii '. But we are *ex hypothesi* bound to admit that in the early republican period some major changes occurred in the Servian organization. These changes can be formulated as follows : (*a*) one legion having been split into two, the *classis* in the Comitia became something different from the *classis* in the field ; (*b*) it was considered expedient to reduce the *classis* in the Comitia to forty centuries and to transfer twenty centuries, divided into two groups, to the *infra classem* ; (*c*) sooner or later, the centuries were subdivided into ' iuniores ' and ' seniores '—which may have extended the franchise in the Comitia Centuriata to people past military age.

Even in this hypothetical form of the original Servian constitution—*equites*, *classis*, *infra classem* (with an additional section of *proletarii* ?)—there remains enough similarity between the Solonian and the Servian order to make it legitimate to ask the obvious question : did King Servius know of Archon Solon ? Unlike Solon, King Servius apparently did not write autobiographical poems, and we shall never know. His problems and purposes were of course not identical with those of Solon. His main task was rather to give greater voting power to the hoplites than to put the magistracies in the hands of the two upper classes, as Solon did. But both fought against the arrogance of the aristocrats and the decline of the lower classes into a position of clientes without rights. The traditional date of Servius is only twenty or thirty years later than Solon. Greek vases had already

[102] *Opuscula* II, 287–306 (the two papers of 1931 and 1934). Cf. H. H. Scullard, *A History of the Roman World 753–146 B.C.*, 3rd ed., 1961, 423–427.

[103] Festus s.v. *infra classem* p. 100 L. ; Gellius, *NA* 6, 13, ' Classici dicebantur non omnes qui in quinque classibus erant, sed primae tantum classis homines, etc.' My earlier hypothesis on these passages (1938) is justly criticized by P. de Francisci, *Primordia Civitatis*, 691–98. But the passages themselves cannot be dismissed, as Hugh Last did, *JRS*, 1945, 43. cf. A. Bernardi, *Athenaeum* 30, 1952, 3–38, and Ernst Meyer, *Röm. Staat*, 52, who gives the literature in full. I shall only mention E. Friezer, *De ordening van Servius Tullius*, diss. Amsterdam, 1957, and the discussion of it by E. van 't Dack, *Rev. Belge Phil. Hist.* 36, 1958, 103–17.

begun to arrive in Rome. The words *poena* and *triumphus* and other Greek loans may by then already have penetrated into the Latin language: *triump(h)us* appears in the Carmen Arvale; *poena* was a current Latin word when the Twelve Tables were written. Archaeology has taught us that Graeco-Etruscan ideas about town-planning and architecture were current in Rome in the sixth century B.C.[104] It would be very strange indeed if the name and the work of Solon had remained unknown to the subtle king of Rome whom tradition associated with Fortuna.[105]

Fortuna was a less propitious goddess than Justice, dear to Solon. Servius Tullius was killed. His impious successor was thrown out. The Servian reform had not consolidated the monarchy. The Servian order must have fallen into abeyance in the obscure years of civil war and military struggles which followed the end of the monarchy. One of the most precise recollections of late tradition was the war that the Fabii and their clients fought against the Etruscans about 475 B.C.[106] A war of this type would not have been possible if the centuriate organization had been in working order. It was a return to the days of Aulus Vibenna and other leaders. The great clan of the Claudii was probably attached to Rome in those years in which the patricians celebrated their liberation from the hoplite discipline. But the very disaster of the Fabii at the Cremera must have taught the patricians that it was dangerous to fight with an obsolete military technique. Perhaps it was the Cremera disaster that saved the Servian organization and later made possible the decemvirate legislation.[107]

[104] cf. for instance E. Sjöqvist, 'Pnyx and Comitium', *Studies D. M. Robinson* I, 1951, 400–11. There are interesting, if not persuasive, observations in V. Pisani, 'Roma e Sparta', *Saggi di linguistica storica*, Torino, 1959, 220–31. In general, A. Böethius, *The Golden House of Nero*, 1960, 11–25. On the Twelve Tables, F. Wieacker, *Vom Römischen Recht* 2 ed., 1961, 46.

[105] Ovid, *Fasti* 6, 569 (with J. G. Frazer's note); Val. Max. 1, 8, 11; Pliny, *NH* 8, 194 and 197; Plut., *Quaest. Rom.* 74, and *De fort. Rom.* 10. cf. M. Guarducci, 'La fortuna di Servio Tullio in un' antichissima sors', *Rend. Pont. Acc. Rom. Arch.* 25–6, 1949–51, 23–32, and E. Peruzzi-Sc. Mariotti, *La Parola del Passato* 14, 1959, 212–20. I cannot accept anything in J. Gagé, *Matronalia*, 1963, 24–39, and 'La mort de Servius Tullius et le char de Tullia', *Rev. Belge Phil. Hist.* 41, 1963, 25–62; but there are some very good points in G. Dumézil, *Hommages à J. Bidez et à Fr. Cumont*, 1949, 77–84. Cf. M. Guarducci, *La Parola del Passato* 15, 1960, 50–53; A. Degrassi, *Inscriptiones* II, 1963, 1070.

[106] Notice P. Frezza, 'Intorno alla leggenda dei Fabii al Cremera', *Studi C. Ferrini*, Pavia-Milano 1946, 295–306; J. Heurgon, *Latomus* 18, 1959, 713–723.

[107] M. Pallottino, 'Fatti e leggende (moderne) sulla più antica storia di Roma', *Studi Etruschi* 31, 1963, reached me when this article was in proof. It must be consulted especially for Pallottino's evaluation of the materials of S. Omobono. I am grateful to him, E. Gabba and F. Castagnoli for information.