

## THE PROBLEM OF THE ORIGINS OF THE ARCHAIC CULTURES IN ETRURIA AND ITS MAIN DIFFICULTIES

(*SKETCH FOR AN ADDRESS*) (\*)

To review these problems in their entirety belongs not to an inaugural address, but to the Meeting itself... or to the meetings of many years. Also, as must be evident, my command of the literature of the subject, and of the archaeological material which to-day plays so large a part in posing the problems, is that of a scholar in a foreign library and a visitor only occasionally to the museums and monuments of Italy. However, since interest in ancient Italy and especially Etruria is so widespread, and since naturally therefore the literature of these problems is not Italian only, but international, perhaps I may be permitted to ask the Meeting to hear for a few minutes some thoughts which have been brought into my mind by three recent contributions to that literature, which in the last three years have been published in the English language. One is by an Englishman, Mr. David Trump; one by an Englishwoman, Mrs Rachel Maxwell-Hyslop; and the third by the American scholar well known throughout this field of study, Dr. Hugh Hencken. It is possible that the three articles which I refer to, having appeared in England and the United States, are not all familiar throughout the Convegno; and I think that from each of them there is something that invites a comment,

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(\*) Il Prof. C. F. C. Hawkes, trattenuto da imprevisti impegni, non ebbe la possibilità di partecipare al Convegno. La sua relazione fu ampiamente riassunta in italiano dal Prof. M. Pallottino, secondo il desiderio espresso dal Relatore, nella seduta del 25 maggio a Chiusi; e successivamente discussa insieme con la relazione del Prof. H. Hencken. Il testo che qui si pubblica integralmente è quello originario inglese del Prof. Hawkes, il quale ha tenuto a precisare che si tratta piuttosto di un « abbozzo discorsivo » che non di un saggio criticamente compiuto e perfezionato.

which may illustrate significant aspects of these problems and of their difficulties, in any case as seen by your English-speaking colleagues.

Mr Trump will be known to you as the excavator of La Starza at Ariano Irpino, and as the holder in recent years of a Scholarship at the British Academy in Rome; he is a Fellow of Pembroke College in the University of Cambridge, and his article has been devoted to the *Apennine Culture* of Italy, the culture of most of the peninsula in the Middle and (as he shows) the Later Bronze Age. It has appeared in *The Proceedings of the Prehistoric Society* for 1958 (Vol. XXIV), pp. 165-200, edited at the Museum of Archaeology and Ethnology at Cambridge. With this subject, naturally, it interests us here in regard to what in the 1st millennium before Christ was already *the old population* of peninsular Italy: the inception of the Apennine Culture is referred by him approximately to 1600 before Christ. There are signs that it did not fail entirely to incorporate elements from older populations, even though primarily it seems to have been introduced by immigrants from across the Adriatic, landing most probably on the Apulian coast. While its first phase, Phase A, is still not well known, the pottery of the next, Phase B, is present all over South-East Italy, and in Campania... whither it will have been brought by the Pass of Ariano, and where it is dated on both sides of 1400 by the association, proved by Dr Buchner at Castiglione on Ischia, with Late Helladic II and IIIA pottery, studied lately by Lord William Taylour, brought by Mycenaean navigators. Its relations with the Milazzese culture of Lipari gives it similar associations as far as Late Helladic III C of a date just before 1200; and at the Scoglio del Tonno of Taranto its pottery appears with a seemingly continuous Late Helladic series. In its Northern form, the culture was not so old. In the Marche, where the work of Rellini did so much to make it recognized, its introduction from the direction of Apulia had been accomplished no doubt before 1200, but obscurely; and only from that point onwards do its ceramics indicate its history. Mr. Trump shows that it was from the Marche, and not Campania, that it was taken to Lipari in the form of Bernabò Brea's 'First Ausonian'; and from the Marche about the same time, around 1200, that its bearers entered eastern Emilia. These things were done in Trump's apparently very short Phase C; and only after that, within the 12th century, though

probably not later than 1150, did the consequences ensue with his Phase D. For now, at last, the people of the Apennine culture in Emilia made contact with the Terramaricoli, long established there and so now their neighbours; and it was from this contact that the commerce in bronzes soon began, which carried bronze goods of Terramara types — those of Säflund's Terramara Phase IIB — down to Navigata, to Taranto, and to Pertosa. It is just at this point, within Trump's D, that began the well-known occupation at Belverde: not before! and this synchronism with the new bronze commerce seems to signify the discovery now — or rather, the re-discovery — of the copper of the Monte Amiata, so near to Belverde, and presumably the tin of the Monte Bradoni. Lastly, it was after that again, not probably before 1100, that true Apennine pottery is apparent in what will be Etruria: in the Lazio, where there were connexions with Campania, and in Tuscany, by spread from the Belverde region. Anterior populations in these regions seem to have been scanty, and could easily have been absorbed. This, then, is the first part of Mr Trump's contribution to our problems: a Bronze Age people of Apennine culture, arriving here late, not before about 1100, in the Roman region perhaps partly from Campania, but otherwise, and principally, from the mountainous interior — with the Marche and the Adriatic coast beyond, where, along the *east* of the peninsula now and *not* the *west*, ran the chief currents of this people's commerce and activity. Thus far, the soil of Etruria has not achieved much importance...

Next comes the second part: the Urnfield or 'Proto-Villanovan' contribution. The essential facts are known to all: the appearance of Urnfields or cremation cemeteries, with pottery suggesting rather vaguely, and not unequivocally, that of East-Alpine Europe or beyond, associated with fibulas now seldom of the arco di violino type and normally ad arco semplice; with a relation to the Terramara cremation cemeteries that is again equivocal; and with a sparse but very extended geographical distribution, from north to far south. Fontanella Mantovano; Bismantova; Pianello di Genga; Timmari and Monte Castelluccia in the south-east; Milazzo on its Sicilian promontory — while Apennine contemporaries from Campania sail to Lipari to form Bernabò Brea's 'Second Ausonian' (this time, perhaps the Ausonians of tradition?); and lastly, in our region of Tuscany-Lazio, Ponte S. Pietro, Sticciano

Scalo which cannot be omitted from this category, and Tolfa-Allumiere which cannot be dissociated either. The cremators of these cemeteries have had a big vogue as invaders from the north-east. Alternatively, they can figure as forth-faring Terramaricoli. But in any case, as Mr Trump has faithfully recorded and insisted, the Apennine culture continues through all this time: it is his Phase E. The Pianello cemetery itself adjoins continuing Apennine settlements; and Ponte S. Pietro and Torre Castelluccia are only the two most obvious instances of the fact, that the settlement corresponding to such cremations is in its ceramics an Apennine settlement still. Further, the cremation-urn ceramic may, and for the most part simply must, be recognized as Apennine ceramic, specialized for the tomb either partly or not at all. Mr. Trump therefore says: this people is in itself unchanged. The meaning of such cemeteries is a 'conversion' (as I may call it) to cremating habits — under influence, cultural or specifically religious, from the north, and more probably from the Terramaricoli (who can have been cremators already) than from invaders across Alps or Adriatic. In the south and east, too, this influence was before long almost entirely exhausted: by the time that we can begin to date the southern tombe a fossa or their counterparts in Picenum, inhumation has gained its victory. — But here I feel myself personally inclined to intervene. The south and east, Mr. Trump has shown, were the oldest regions of the Apennine culture's domain. One expects its traditions there to be at their strongest: that the novelty of cremation should be finally rejected, is easy to understand. But in Tuscany-Lazio, he has also shown, this was not so. The fibulas of the cremation cemeteries in general date them from not later than 1000: thus, even if Ponte S. Pietro and the others in our region are not quite so old, this province of the Apennine culture was still very young when they were started. And here, cremation remained strong: throughout the communities of the Iron-Age Villanovans, to Rome, to the Alban Hills... I am inclined to believe, therefore, that while Mr. Trump's argument from the ceramics is in itself correct, and while his ethnical inference from it appears correct for the basic populations of the inhuming south, yet in Picenum it is correct only with the reservations required by the historical distinctness of the people; and in Etruria-Lazio it is again correct with this reservation: that the events which introduced cremation were here, to this extent anyhow, indelible.

There is of course the further question, whether here was super-added, after the Ponte S. Pietro stage and before the classic Villanovan, some further external influence or influx, from some eastward Continental region. Of this I will only say, that if there was, its demonstration by comparative archaeology needs to extend a long way east, before finding anything that may be *more* cogent *than* a sort of 'Italo-Balkan parallelism'. The great pots of Donja Dolina are *not* closely similar to Villanovan ossuaries: they are just vaguely members of the same widely-extended family, that is all. Dr. Hugh Hencken, to whose article I shall end this essay by referring, has seen this; so too, in her own way, has Pia Laviosa-Zambotti; the region best comparable is beyond the mountains and beyond the Save, with the urns of the Dubovac-Zutobrd group and their cognates beyond the border of Rumania. Thus, Villanovan cremation may be not simply a persistence from that of Ponte S. Pietro; it may be due further to a reinforcement, applied by some prestige-bearing group who would then be veritable immigrants from East-Central Europe, received into the matrix of the existing culture. That this is a thing to be seriously reckoned with, moreover, I find is agreed by Professor Pallottino, in the latest edition of his *Etruscologia*. (p. 91). But what does all this amount to? Only this: that whereas the Fossa-grave peoples of the south were peoples of the old Apennine culture who had undergone 'Urnfield' influence and had absorbed it *without* being greatly affected at all, the Villanovans were Apennine people who had received it quite possibly twice, and who anyhow *were* considerably affected. You may reply, that all this has been said before, and said more effectively... Yet even so, I think that Mr. Trump's careful demonstration of the universality, and the continuity, of the Apennine culture in its various provinces, from Lipari to the Po, is a fine example of what can be done by essentially ceramic studies. For how can these provinces not, when all is said, be provinces all of a culture and of peoples basically and in an inclusive sense *Italic*? If the Terramaricoli were a different people, and if the institutors of cremation at Fontanella or the Pianello or Ponte S. Pietro were a different people, and if those responsible for the standardizing of Villanovan ossuaries or Alban or Palatine hut-urns were different peoples, I do not know; I do not know who *any* of these people were, in so far as they were each distinct. But I *do* know that the situation to-day, after 30 years of study

of the Apennine culture, from Rellini to Trump, insists that all those « peoples, or influences », must be *ethnically secondary*, in the composition of the Italic peoples, and that the peoples of the *Apennine* culture that are *primary*. And if there are Neolithic or Chalcolithic groups that ought to be called 'pre-primary', as no doubt there are, their contributions were made *through* the Apennine culture and not in contradistinction from it. The archaeological stature of the Apennine culture to-day, in fact, seems to me a thing that has grown, from the insignificance of the 'Extraterramaricoli' of former days, to be the biggest single factor in the estimation of Italic origins.

I turn now to Mrs. Rachel Maxwell-Hyslop. She is of the Institute of Archaeology in the University of London; she is the assistant of Mallowan, and the pupil of Sidney Smith, as well a disciple of Gordon Childe. That is, she is an archaeologist with European but more especially Mediterranean interests, whose particular studies are in the field of the Middle and Near East. Also, she has paid attention not only to the 'ceramic archaeology' which naturally plays so large a rôle there, but especially to metalwork: the gold and the silver, the multifarious bronze, and finally the iron. And the contribution by her which I want to call before you is not the one published in the journal *Iraq* in 1957 on the question of Etruria and Urartu, for Professor Pallottino has already discussed it in his writings, as he has discussed each and all aspects of that captivating problem, and of course it belongs to the Orientalizing period of Etruria, which I have not yet begun to reach. Instead, it is her article of 1956, which arose from the same series of visits to Italy by her in the middle years of the current decennium, and was published in *The Proceedings of the Prehistoric Society* vol. XXII, pp. 126-142. It bears the modest title, 'Notes on some distinctive types of Bronzes from Populonia'; and it is concerned with that westerly portion of Etruria which the Convegno is going to visit, and also with Sicily and with the Eastern Mediterranean. Its period is not the classic Orientalizing, but the problematic time of its remoter antecedents. It is addressed to the question: is there anything in Etruria *before* the classic Orientalizing, back in the Arcaico Primo or even in the time of its earliest inception, which is not Italic but is *already Oriental*?

There has been a theory which you all will know, most prominent in the older modern literature of Etruscology, but still held

by some writers to-day, which holds that the Etruscans, a people living somewhere about or near the Eastern Mediterranean, took an aggressive part in the great tumults and raids launched by the so-called Sea Peoples, in the 13th and 12th centuries before Christ and especially close before and after 1200, and then, more or less immediately, and in great or less great numbers, sailed away to Etruria and settled there, to make themselves ultimately the overlords of the Villanovans or other natives, and so attain their historical position in Italy. I think that this theory would never have been proposed, if the Egyptian records of the Sea Peoples had not contained, in their enumeration of them, not only the name *Shklsh* which does so make one think of Sikels and Sicily, and not only the name *Skrdn* which does so make one think of Sardinia, but also the name *Trsh*, which likewise will make one think of Etruscans. And, for those who believe that the historical Etruscans contained an Oriental element, or an Oriental determinant, in their ethnic composition, and who ascribe to this their ethnic name, it is very natural to believe that this element was really, all or in part, the same thing as the *Trsh* among the Sea Peoples. But unfortunately, the supposed 12th-century Orientals in Etruria have always been quite absent in the record of archaeology...

Mrs Maxwell-Hyslop, with her keen eyes trained in study of the Oriental material which might be relevant, again found nothing here that is certainly as early as the 12th century. But she did notice that the bronze grip-tongue swords, which in the East Mediterranean and Aegean appear so suddenly just in the Sea-Peoples period, have in Italy some apparently direct derivatives. Here, I must clear away a misconception. At the time when she wrote, it was still possible to credit the proposition of 1948 by Gordon Childe, that these grip-tongue swords had been developed in the Orient itself — in parallel, therefore, with the well-known development of the type in Central and Northern Europe. But in the same volume of the *Proceedings of the Prehistoric Society* in 1956, a full study of the grip-tongue swords of this period in the East Mediterranean and Aegean was published by Mr. Hector Catling, of the Department of Antiquities in Cyprus, which proved that this theory was wrong. For these are swords of the so-called 'Type II': the 14th-century weapons of Ras Shamra have nothing at all directly to do with them; and if a place of first appearance in those regions must be chosen, that place is the mainland of

Mycenean Greece, about the middle 13th century. And so — while Catling did not consider Central Europe — we must to-day return to the long-maintained German belief, of von Merhart, of Sprockhoff, and of Milojević (though we need not agree with them in all theoretic details), that these swords appeared in the middle 13th century in Aegean and East-Mediterranean lands by derivation from Central Europe, where alone the type had had its primary development. This conclusion, as summarized by the English sword-specialist Mr. J. D. Cowen, was presented in 1958 to the International Congress of Prehistoric and Protohistoric Sciences at Hamburg, in an applauded communication which will be published in its *Acta*. Now grip-tongue swords have a long history in Central Europe also after the 13th-12th centuries. And if they appear in those centuries in Greece, by introduction of their type somehow through the Balkans or down the Adriatic, they can appear in Italy likewise, again either by land (the Alps) or the Adriatic, at any time from that period onwards, according to the chronology acceptable for the individual examples. And I believe that most of the examples found in Italy represent this process of derivation from Central Europe. Yet, notwithstanding that, it remains *also* true, that this type of sword *also* played a part in the Eastern Mediterranean, once it had been introduced there in the 13th century, so that to recognize derivatives of it *also there* is quite legitimate. I do not include here, indeed, the so-called Peschiera daggers, which I think are certainly of the same Central-European origin, primarily, as the swords — whether at Peschiera itself, or in isolated examples (cited by Mrs Maxwell-Hyslop) at Knossos, at the Grotta di Pertosa, or at the Scoglio del Tonno of Taranto. But those *do not exhaust the possibilities*. For example, the extension of the flanges of the grip-tongue to include a T-shaped projection of its top, as in the daggers of the Modica hoard or of the Italian Iron Age, is a matter of Aegean and Oriental development. Central Europe did not share in this: it only contributed to its origins, in the Sea-Peoples period around 1200. I say therefore that Mrs Maxwell-Hyslop is right, that Italy can have *not only* grip-tongue weapons of Central-European derivation, *but also* some that are of East-Mediterranean derivation. And their period need not be the period around 1200 itself: it could more intelligibly be *after* it. Immediately after? How long after? That is a matter for the individual specimens, and their associations if

any. On the Gargano, the extraordinary series of swords and daggers from the funeral cleft in the Grotta di Manaccora includes grip-tongue weapons that could be of the 12th century already, as Mrs. Baumgärtel claimed they all were, as well as one with the projecting top, prototypic to the T-shaped type already noticed. The pottery in the cave, in so far as it is exotic fluted ware, is no doubt trans-Adriatic, and difficult to date; but it includes also Apennine pottery, and this for Mr. Trump is predominantly of his Phase E, and at the earliest not before the 11th century (his pp. 178-9 and Table, 195). The mysterious painted sherds here resemble the few found by Mosso at Coppa Nevigata; and for Mrs. Maxwell-Hyslop these suggest the outgoing Late Mycenaean of Cyprus, again of the 11th century (her p. 141, note 1). Oriental origins seem also possible to her for Manaccora bronze pins and for Nevigata bronze spearheads. Lastly, a few grip-tongue swords without provenience in the Museo Pigorini (her fig. 5) and one from Sicily near Gela and perhaps from the cemetery of Monte Dessueri (*BPI* LXVIII, Tav. III) suggest East-Mediterranean origins about the same time (not Central-European without difficulty, anyhow), while her best parallel for the Manaccora projecting-top sword (though an iron one) comes from Idalion, again in Cyprus. Thus the conclusion seems to be, so far, that in the aftermath of the Sea-Peoples period, in perhaps the later 12th and in the 11th century, the South-East coasts of Italy and Sicily were visited by East-Mediterranean navigators of some kind, just as Taranto was by Sub-Mycenaean Greeks, who brought in East-Mediterranean things, and who may here and there have settled.

We reach Etruria next, with Mrs Maxwell-Hyslop, after further considering Sicily. She brings together further material from Sicily, still mainly of the period Pantalica I and so again not later than the immediate sequels of the Sea-Peoples period, which strengthens the notion of East-Mediterranean incomings. The comparisons offered extend even to the Talysh in Transcaucasia; and from all this follows her final proposition. This is, that at Populonia, and elsewhere near it, there is sufficient material of the same exotic character, to indicate about 1000 a further move, of a group of the same Oriental background, coming either from those in Sicily or anyhow in close connexion with them, to this part of the Etrurian coast-lands. It may easily be agreed that the thing is possible. The question is, if so: was it important? Was it important? or

need we only say that a connexion between Populonia and Sicily at that time, in the beginnings or the local prelude to the recognized Arcaico Primo, is admissible only as a thing of evanescent and subordinate significance?

It is well known that in the cemetery of the Piano delle Granate at Populonia, studied by Minto in his book of 1922, there were tombe a fossa with inhumations, chamber tombs beginning typologically early, and also cremation tombs, all three types belonging to the same Archaic period. Elsewhere in Etruria and not far away, we know already that cremation was introduced about 1000, from the north or east, amongst a not-long-established population of late Apennine culture. How early chamber tombs may be, when neither Greek nor Orientalizing contents are present to date them, is perhaps still an open question. At this cemetery, certainly, Minto was sure that some of the earliest sort had been made while both cremation tombs and also tombe a fossa were still being made. And the presence of tombe a fossa at all seems rather curious. Now, Mrs Maxwell-Hyslop has insisted that the bronze dagger in Tombe a fossa no. 7, associated with simple fibulas and minor bronze things only, is an immediate derivative of the grip-tongue series already here considered, being quite close, though not identical, with most of the weapons in the Grotta di Manaccora — and like them, apparently, with a knobbed rhomboidal pommel made separately (these at Manaccora are Baumgärtel's so-called 'tutuli') and affixed presumably by mastic — her fig 1, 1. A date about 1000 would quite well suit this dagger. In itself, both it and the Manaccora weapons could perhaps be not of East-Mediterranean, but of trans-Adriatic origin (indirectly then Central-European), like the Manaccora fluted pottery. In that case, they might be connected not with Sicily but with the cremators who can be invoked — theoretically, as we have recalled — to account most immediately for Villanovan ossuary-practice. But the same cemetery, and some of the same tombe a fossa (though not no. 7), produced fibule a gomito, of the ad occhio type with a coil inside the gomito, which occur in Sicily in addition to the primary type a gomito semplice. The chronology of both these types of fibula is more easy to date later, than at its beginnings. Nevertheless, it is universally agreed that they connect Sicily and this part of Etruria somehow with the Cypriote and Syrian cultures of the 11th or 10th to the 8th century. And moreover, it is recognized,

and has lately been emphasized exhaustively by Professor Martín Almagro, that the Sicilian and Populonia types with the bow smooth, or else engraved, must be distinguished from those with moulded bow, which form a range of series in Syria and Cyprus and were carried thence to Spain. The starting-point of the Sicilian series is earlier than those developments, and can be dated again in the 11th-10th centuries, with the aid of a stratified example at Megiddo (Maxwell-Hyslop fig. 4, 1: the gomito already slightly more specialized than in Sicily). In addition, one may mention the bronze sword from Chamber Tomb no. 2 in the same Populonia cemetery (her Pl. XI, 3), where the Italian T-projection is present in what seems to her still an Oriental-looking form, and also perhaps the definitely Oriental type, noted in the Talysh, of a sword from Quercianella, near Livorno (Montelius Pl. 169, figs. 12, 22: perhaps with a Villanovan ossuary).

I suggest that this list is genuinely more significant than a mere assemblage of 'bits and pieces'. It seems to me attractive to attach significance to their exotic character in this part of Etruria and to attach the same significance to the Populonia phenomenon of *tombe a fossa*, which is in any case anomalous. Can we refer this to a persistence, after the appearance of cremation as at Ponte S. Pietro, of the old Apennine rite of inhumation, as seen for example in the Marche? If one takes Professor Puglisi's view of the 'dolmenic' tomb at Pian Sultano, in relation to Etruscan chamber tombs, one could then take these *tombe a fossa* as an analogous legacy from the Apennine culture. But it is remarkable that in Sicily, one of the proveniences of the simple fibule *a gomito* is the cemetery of *tombe a fossa* at Molino della Badia, which is there likewise anomalous, and indeed unique. And I cannot see any other sign of Apennine traditions at Populonia.... Accordingly, if we do make the hypothesis that East-Mediterranean visits to south-east Italian and Sicilian shores in the 12th-11th centuries had led, before 1000, to some settlement of such folk in Sicily, and that these began, about 1000, to sail to this part of Etruria, what then? — They could have come just as traders, to obtain a share in the output of the copper and tin of the interior, around Belverde, which we have seen was now fully flourishing. Certainly; and more than that. For they will have recognized, soon or before long, that close by, and in Elba, there was iron. Very gradually then, through the 10th and 9th centuries, one could imagine them

growing and consolidating, and as laying the foundation of that intimate inter-relation with what was now the main Villanovan population, to which the archaeology of the 8th and 7th centuries bears such testimony. — Such an idea is hypothetical in the extreme — perhaps to the point of absurdity? The correct archaeology of Etruria should take its bearings not from Populonia, not even from Vetulonia, but from the south, and above all from Tarquinia. Yet I should not be wholly surprised if the future it were to be found worth further considering. For, with the Apennine culture now appearing 'pan-Italic', and the proto-Villanovan providing modifications of it indeed, but only modifications and not full replacements, the Villanovans of the 8th century must seem an extraordinarily strong Italic mass — strengthened indeed by the modifications themselves — to support a society of predominantly Etruscan colour emanating *either* from a basically *pre*-Apennine element, in a country which archaeologically has so little to show for it, *or* from Etruscans immigrating in the 8th century with *no* predecessors entering the land before them.

And at that, I will leave so elusive a possibility, and turn to Dr. Hugh Hencken, the scholar of Harvard University whose work you all know best of the modern English-speaking students of these problems. His most recent short article, reprinted from the Acts of the *Ciba Foundation Symposium* on Medical Biology and Etruscan Origins, published in the USA in 1958 (pp. 29-47), is entitled 'Archaeological Evidence for the Origin of the Etruscans'. He begins with a summary outline of the modern presentation of the subject by Professor Pallottino. The development, as we know, was long and complex. The Chalcolithic people, and the people of the Apennine culture of the Bronze Age, had come in part at least from beyond the Adriatic. Then came the formation of the Villanovan culture with its cremation, arising from the foregoing elements in Etruria with the addition of a nucleus of cremating people from Central Europe. And from that composition, with no more ethnic elements added, issued the protohistoric and historic Etruscans. Hencken's own suggestions, for modifying this, are two. First, he would like the cremating immigrants who standardized the Villanovan culture to be more numerous and more important. Well, I have already said that there must be a real difference between the east and south, where cremation was brought in and was soon extinguished again by inhumation, and the Lazio

and Etruria where it flourished through the Early Iron Age. I think that this well-recognized position has to be maintained, in spite of Mr Trump's demonstration that the Apennine element is in all these regions fundamental. And I have no doubt that a greater number of early cremation cemeteres still wait to be discovered: especially cemeteries like Tolfa-Allumiere, and connecting it with the better-known Selciatello or Poggio dell'Impiccato. The strength of the forces here which changed Apennine to Villanovan could then be better measured. For, as Dr. Hencken rightly reminds us, those forces were anyhow strong enough to bring in connexions with Central Europe which endured through both the first and the second Villanovan periods. A common origin for the *idea* of the hut-urn, in Italy and in the Transalpine North, is surely difficult to refute, even though all arguments at present must be *e silentio*; then there are the helmets and the knives and the antenna swords, the bronze girdles, the embossed decoration of sun-disc and bird-boat and the numerous bird-protoms and the zoomorphic wheeled vessel from Tarquinia, and the Hallstatt situlae of the type of the Tomba del Duce of Vetulonia... All this stands beside and among the Italian and the Orientalizing phenomena of these periods, and — apart from what can be found in North-East Italy and in Picenum — it is only in Villanovan Italy that these things appear. We do well to remember the article of Georg Kossack, in the *Atti del Congresso Mediterraneo di Firenze* of 1950: it is the best modern treatment in Italian of this Transalpine aspect. The thing is surely not intelligible unless *some* group or groups came into the region in the 11th century or the 10th, introducing the cremation and giving the potentiality for this long subsequent story of Transalpine connexions. Yet I am not so certain, that the new group or groups of people *themselves* came genuinely out of Central Europe. That is always still not proved. And really, it is *only* necessary that they should have *the habit* and the *tradition* of Transalpine connexions *already*. Having *that*, they have the potentiality of developing it further in Etruria! Thus, I agree with Hencken in stressing the big importance of this cremating element in the formation of the Villanovans. But I think on the other hand, that they do not *need* to have had an immediate physical Transalpine origin. *Perhaps* they had! But perhaps not; and *if* not, may they not have been people only from across the Apennines, not the Alps, and not from Rumania but

from the Romagna, not from Hungary or Jugoslavia but from Emilia?

We are back again, in this case, almost to the good old Epic Cycle of Italian prehistory, the Epic Cycle of the Terramaricoli... Not, indeed, to the Emilio-Roman Aeneid of Luigi Pigorini *exactly*. But to a modern recension which is authentically related to it. Mr. Trump has shown that in the Late Bronze Age, his Apennine phases D and E, from the 12th century onwards, people of the Apennine culture from the Marche had settled in Eastern Emilia; had thus become the neighbours of the Terramaricoli; had engaged in commerce with them; and had thus enabled a big trade to be developed, using the metals of the mountains round Belverde, where a parallel group of Apennine people now established themselves — the trade which from the Marche went down the Adriatic coast, and brought the bronzes of Terramara types to Nevigata and to Taranto. Into this situation came the rite of cremation, as we see it at the Pianello, and with it came the beginning of our long story of the appearance, in peninsular Italy, of Transalpine types of metalwork. But the Terramaricoli, now at least, and perhaps before now, *were* cremators. And they *had* got Transalpine connexions, which are apparent in their metal types; they had had these connexions already in the Early Bronze Age, the time of the flanged axes and triangular daggers; and, as Sjöflund's analysis of their bronzes has well demonstrated, they continued with these connexions from the Early Bronze Age to the Late. And now in the Late Bronze Age, we see them passing into what Sjöflund called their 'Adriatic phase', which Mr Trump has demonstrated was a sort of Terramara-Apennine *consortium*. He thus can argue, comparing the more rotund of the Pianello cremation-urns with the Terramara urns equally rotund from Copezzato or Casinalbo, that the whole cremation phenomenon of these times in Italy is of Terramara origin. It will have had its effect *not* through a heroic march-out of Terramaricoli to the South, *but* through an extension of their same contributions to the *consortium*, which was giving to the South their types of bronzes. All that we require to propose for Etruria and the Lazio, then, would be a province of this same culture with the Terramara elements particularly strong. And we could add, going further than Trump, and perhaps bringing satisfaction to Kossack or to Hencken, that the subsequent strength of Transalpine influences in Villanovan Etruria and Lazio is sim-

ply an aspect of that same strength. For it was the Terramaricoli in Emilia, in contrast with all Peninsular Italy, who from the Early Bronze Age onwards had metal-work, and specifically metal-work already with Transalpine connexions! Their contribution to the Villanovan culture will thus be not cremation only, but the habit and tradition of those connexions, which had only to be continued and tended in this new land, to bring in all the Transalpine types which meet us in full array at last in the 8th and 7th centuries. — In the title for this essay I alluded to the *difficulties* of these problems. But I suggest that the best solutions for difficulties are those which are *most economical*. For all the aspects of the Early Iron Age in our regions which are not Oriental and not Hellenic, that is, for the Italic and the Transalpine aspects, this suggested solution *is* economical: it uses only the elements known already to exist, the Apennine and Terramara cultures.

I admit that I find it still *rather* difficult to dispense with *all* recourse to Transalpine immigrants. In particular, I cannot forget that the Terramaricoli had *no hut-urns!* But here too, perhaps, there may be a solution that is economical. For where, in all Italy, is there a hut-urn which is *not* Villanovan? The reply, of course, is *Este*. And I think that if we require authentic immigrants, we must find them where we already know that they exist — in the Atestine Venetic region of North-east Italy beyond the Po. There, in the Middle Bronze Age, instead of the Apennine culture, we have the Polada culture; in the Early Iron Age, instead of the Villanovan culture, we have the culture of Este... with Golasecca to the west of it. And in Pia Laviosa-Zambotti's doctrine of Paleo-Veneti and Veneti, we have already something similar to Mr. Trump's doctrine for peninsular Italy, *yet* we know that the historical product is not Italic, but Venetic. I should like to hold that what changed Paleo-Veneti to Veneti, and Polada to Este, was a genuine immigration of Urnfield cremators here, in the Late Bronze Age — this *is*, after all, usually believed... And as we have the Venetic name in North Europe, close to the hut-urns of the Late Bronze Age there, and a hut-urn equally in Venetic Este, I should like Laviosa to be correct in her inference, that both the name and the idea of the hut-urn had their home in the world of the Central-European Urnfields, whence they travelled both north and south. Furthermore: our earliest cremation cemeteries, of the Pianello sort, have one representative that is in fact beyond

the Po — Fontanella Mantovano. And in it we see already the association of Mediterranean with Transalpine bronze types: the fibula ad arco with lunate knife or razor. Also it lies very near to the region of Este — so that in the Iron Age, the Mantovano becomes included in the Atestine. If the responsibility for proto-Villanovan cremation were *not completely* and *exclusively* assignable to the Terramaricoli — in spite of Italian tradition, and in spite of Mr. Trump! — *then* I should assign what remains, again with economy, to the cremating immigrants of North-East Italy who were the authentic Veneti. I should say that at first, in the 12th and 11th centuries, Mr. Trump's *consortium* was veritably of Apennine people with Terramara people. But then, for his Phase E, there came in across the Eastern Alps new cremators who were Veneti. They inserted themselves into the consortium. They are already present at Fontanella, which remained in their own Venetic territory. But they are present also, perhaps at Bismantova, which is right down in the Emilian province of the *consortium*; and I believe they are present also at the Pianello. They would there account for the more *biconical* shape of many of the urns which are *not rotund* like those of the Terramara cemeteries; and their tradition, absorbed into the Apennine ambience of the Marche, could explain the cultural and perhaps the linguistic distinctiveness of the protohistoric and historic Picenes. In the influence which made for cremation, they will have reinforced the Terramara element. In its diffusion in the South, their influence was in general slight; and cremation itself was of course soon generally renounced there. But in the Lazio and in Villanovan Etruria, they will have participated strongly enough, in the development of our culture of the Iron Age, to introduce the tradition of the hut-urn. Thus, the Transalpine elements in Villanovan culture will be due *not only* to the fact that it contained a Terramara element with old Transalpine connexions, *but also*, and *in reinforcement* of that, to a *Venetic* element which was indeed of no ethnical significance, here in Middle Italy, but which remained always active as the North-East neighbour of the Villanovan culture, and consolidated its hold in the Atestine region opposite to Villanovan Emilia, so that most or all of the routes from the Alpine Passes, where the Central-European traders came over and down into the Val Padana, were in Atestine hands — the hands of the Venetic middle-men between Central Europe and Italy, whom both sides knew, and with whom

both sides had elements now in common. But finally — and of course! — what I must here desiderate, is the discovery of half-a-dozen new and good proto-Venetic cemeteries, of the period from Fontanella to Este I, distributed from beyond the Piave to the Po. In the actual situation, what I have suggested perhaps exceeds the material evidence too far. I can only hope, and I re-affirm my belief, that the story of Venetian contributions to the life of Italy is not yet thoroughly unfolded...

I am left now with the second of the two suggestions offered in his article by Dr. Hencken; and this will take us once more, in conclusion, to the Mediterranean or Oriental side of our problems. It is simply, that he pleads for a further lease of life to be accorded to the notion of Oriental immigrants at the beginning of the Orientalizing phases in Etruria. For he points out, as I also have, the extreme poverty any pre-*Apennine* elements living in Etruria, as against the strength of the *Apennine Bronze Age* elements, which we must regard to-day, I feel sure, as quite indubitably Indo-European, and generically as Proto-Italic or Italic. He prefers not to consider, in this connexion, the proposal of Mrs. Maxwell-Hyslop: that there was an *Orientially-connected* colony already at *Populonia* from about 1000 before Christ. Therefore he has remaining only immigrants at the beginning of the Orientalizing phases, to explain the special character of the Orientalization, and — in the last resort — to explain the scarcely Indo-European Etruscan language. This position, of course, has been attained by many people before! and the concluding sentences of his article are as pacific and as inclusive as we should expect from such an immensely reasonable man. In the course of the article he has emphasized, most valuably, the difference between the Greek sort of colonization, where an 'island' of exclusive Greek life is planted on a foreign coast, and the prehistoric sort, where old and new people soon are intermingled in most cases. The formation of the historical people of Etruria suggests almost wholly this latter kind of movement, and not the abrupt and self-conscious Greek kind. Yet, notwithstanding that, he finds himself still feeling that such phenomena as the great chamber tombs, filled with rich Oriental objects, side by side with the things that show the Greek influence and with the *Transalpine* and the authentic *Villanovan* — this sort of phenomenon must, in spite of all that can be said for purely cultural and commercial connexions with the Orient, mean

that there had arrived, perhaps!- perhaps!- 'a new, wealthy, and dominant element from much further afield'. It must be noticed that this contention of Hencken's relates not so much to the Second period, from about the middle of the 8th century to the very early 7th, as to the Third, beginning (as he propounds afresh for the Bocchoris Tomb at Tarquinia) within the 1st quarter of the 7th century and continuing till near its end. It is at this point, as he says, that « we find ourselves in a new world ». But of course, there are already Oriental elements in the Second period. Thus, we have to think of a *crescendo* in this Orientalism, commencing perhaps with commerce only — like that of the same time with the Greeks — and culminating in the « new world » only after some two generations. But if the big Chamber Tombs of the Third period are exotic, yet there are *some* chamber tombs already in the Second period; thus, if we are to think of immigrants, we have got some of them already then. And then there is Populonia, with its primitive small chamber tombs on the Piano delle Granate side by side both with cremations and with the tombe a fossa of Mrs Maxwell-Hyslop's early fibulae and dagger. It is of course only by inference, and not from datable contents, that any chamber tomb could be thought as early as the First period... And the tombe a fossa at Populonia of course form a temporal series, with the dagger the earliest object, and the material in general extending onwards until later in the Archaic sequence. We have it now from our colleague Vighi that at Cerveteri too, there was a sequence of tombe a fossa alongside Villanovan cremation. At what point in the sequence were the first Chamber tombs constructed? This is a familiar and often debated question. But in any case, it is clear that what marks the start of the « new world » of the Third period in that regard is *not* the start of chamber tombs *altogether*, but the start of *grandiose* ones. Thus, *if* chamber tombs were really exotic, and not an indigenous advance on a 'dolmenic' tradition of prehistoric roots (as thought possible by Puglisi), their sequence makes us think of a *continual infiltration* of exotic settlers, with the *wealthy* and dominating ones *supervening after 700*. Lastly, as everybody knows, the Oriental and Orientalizing material does not indicate any one Oriental quarter, from which such people will all have come. It is rather like a connoisseur's collection, of pieces assembled by purchase from dealers quite internationally... Now it seems to me very possible, that with the rise of Etruria's reputation as a

country 'good for business', in the 8th and on into the 7th century, there were attracted to it some numbers of enterprising Orientals — and Greeks too sometimes, if we wish to believe in Demaratos. Not only the United States but South America, or South Africa, all can furnish modern analogies. But, for as long as the evidence for their *origins* remains so many-sided and so imprecise, it is impossible to think of them as a compact group, such as could introduce a political and religious system and — above all — a language. Clearly, many or most participants in the Convegno will agree with me: the introduction of the language at this point is not very much desired... and not even necessarily by those who believe in immigrants now from the Orient, *because they cannot* be made to seem compact.

Well, and what then? The question I would ask is this: *how* was it that in the 8th century Etruria became reputed «good for business»? It is a country good for horses, and for not a few other things; but surely good *conspicuously because* of the metallic wealth of the north, and that of Elba. In the middle of the 8th century, when the Greeks were coming up the coast, and settling first on Ischia and then at Cumae, one would expect them to be interested in North Etruria too: but no! They could make no establishment farther up than Cumae; and I am sure that Dunbabin was right, in his book *The Western Greeks*, that not in the north only, but over the whole length of Etruria, the Etruscan power was already then great enough to keep them out. An individual Demaratos might be admitted: an oikist with his colonists, never! Such an Etruscan power, surely, implies already the Etruscan language. Just so, too, does an Etruria so highly reputed as to attract Oriental entrepreneurs (and a Demaratos with them), to help to build up the cities and improve the organization and exploitation of the country and its resources. And I think that this reputation was not easily achieved. At what time can we place the incessant fighting with the *Ombri*, which is vouched for by Herodotus? Must we not connect it with the fact that whereas we must think of the native elements and the Apennine people everywhere, as Indo-Europeans and as having proto-Italic languages — what else is possible? — the epigraphic language is exclusively Etruscan, until Latin comes? I am ready to credit Dr. Hencken's entrepreneurs, and even Demaratos, *if*, and essentially *if*, they had a rising and pro-

fitable Etruria, *already Etruscan*, to attract them as individual immigrants. Accordingly, we are left, for the language and whatever must accompany it, *either*, with the idea of descent from a pre-Indo-European substratum — and this, to me, looks now a very weak chance, now that the Apennine culture has increased so greatly in importance, and is revealed as the essential matrix for Proto-Villanovan — *or else*, we can enquire of ourselves whether it might be really possible, that Mrs Maxwell-Hyslop's idea of immigrants already at 1000 in North-West Etruria, making a *west-coast* contact with the Belverde metal-trade, which had flourished primarily till then on the *Adriatic* coast, and then finding *iron* still nearer to their hand — whether that idea, in itself sufficiently commendable, might be used as she would use it, to give *that* time, and *that* place, to a first landing in Etruria of the lineal forefathers of the Etruscan language? *After* they had used Sicily as a halting-place on the way, and that again *after* their East-Mediterranean performance among the Sea Peoples? But *then*, through the 10th and 9th centuries, gradually winning control in the land, through the Villanovan Stage, for the nascent Etruscan polity? Truly our studies are difficult! But difficult, too, was the process of our Etruria's formation.

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