

Colchester Before the Romans or Who Were Our Belgae?

A lecture of 1950 re-appraised

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Thirty years ago a great occasion was approaching at Colchester: the nineteen-hundredth anniversary of its Roman foundation. Mr. Hull determined on a Conference to mark it, and a great Exhibition in his Museum. In my obituary tribute to his life, I have touched on both: they were publicly its peak, for they drew a big concourse of admirers—to appreciate his work, view the monuments and sites, and the newest excavations (Mrs. Cotton's and Miss Richardson's), and hear him and others, from Britain and abroad, give lectures. The subject for mine was 'Colchester Before the Romans: the Kingdom of Cunobelin'. The date corresponded to the summer of A.D. 50, when the Roman citizens' Colonia, founded by decree the previous year, with its title from Claudius, had come to be thoroughly established. My lecture was on 6th July in the evening, at the Moot Hall. David Clarke, who edits this volume, was there. He has asked me now to 'up-date' it. The result, here following, I offer in Rex Hull's memory.

1. The setting in 1950 and the present situation

It was the second such evening lecture, open to the public; the first had been given by I. A. Richmond, 'Colchester under the Romans'. Morning lectures altogether were nine; three afternoon talks were on sites, that at Bradwell with a further lecture on the spot; and the standard they were setting me was high. The names of the speakers show it: Hull himself, Eric Birley, Ian Richmond, John Brinson, Alex Hall; Paul-Marie Duval on Paris, Elisabeth Ettlinger (for the Swiss) on Vindonissa, Mme. Faider on Roman Belgium, and Professor van Giffen on Valkenburg in Holland; on their new excavations Molly Cotton and Katherine Richardson; and at Bradwell, on the Saxon-Shore fort, Robert Appleby and Nowell Myres. Professor Donald Atkinson (my chairman), Miss M. V. Taylor and Mortimer Wheeler, Gerald Dunning and Kathleen Kenyon, all by their presence enhanced that week. It had been opened with a Civic Reception; would be ended with a fitting presentation to Hull; and fully earned its long report by Brinson (his 1950). The account of my lecture there includes, of its four main topics, the principal three. The fourth, on the Camulodunum site at Sheepen and the Dykes away around, was briefer, as Hall had had the Dykes in his lecture that morning, and Sheepen and its excavated story could be treated as familiar to a good many hearers, from the *Camulodunum* by Hull and myself which had appeared three years before. So Brinson's omitting it was sensible; but all of his report on the Conference is excellent, as any will agree who have access to its numbers of the old *Archaeological News Letter*. (My Bibliography here gives references all under author's name and year of publication.)

Besides his column on the lecture I still have the six sheets of notes that I spoke from. To what was up-to-date then, it is a far cry back from today. What was welcomed by all those eminent persons would now arouse titters and pretestings. So I am setting out here to attempt an up-dating, of at any rate its first chief topic. I shall only glance at the second and third; and the fourth, which made its conclusion, was a rapid re-hash of what was relevant from *Camulodunum*—Sheepen from Cunobelin to Boudicca and her aftermath, and sketch of introduction to the Dykes. On these, as already had Hall, I looked forward to what only now is just becoming feasible: a topographical survey, using selective excavation, of their sequence (visible or buried), with relations to some earliest roads and works that are Roman. For my briefness on all this now there is a further good reason: work is in progress for a *Camulodunum II*. Philip Crummy is handling this, with myself and with Rosalind Dunnnett and others.

Pre-Roman Colchester fell in **A.D.** 43, to the Romans under Claudius. It was the previous hundred years that nearly all of my lecture was to cover. This would start me just before Essex, at its north-west corner, had its visit from Caesar, 54 **B.C.** About Essex in the prehistoric Iron Age till then, there was in 1950 little to say. True, Miss Anne Welsford of Newnham College, Cambridge, had in the 1930s tried research on it, and from the Museum at Colchester was aided by E. J. Rudsdale; but the map she had produced, which I showed in a slide, had too few sites to make sense. But I bravely said that Caesar's Essex friends, the Trinovantes—whatever had been their past—were not Belgic. 'Belgic' culture (explained here below) came later to them, palpably after Caesar; as for a prior home in Gaul, any time when they would cross from one (I thought) would be too early for the name. Five years ago, in her book on these Britons of Essex, Rosalind Dunnnett could make an advance beyond that. Her Trinovantes, indeed non-Belgic in the main, had a small but wealthier element, like a Belgic aristocracy, already in the phase which followed after Caesar directly (Dunnnett, 1975, 9-12). This elite can have crossed from Belgic Gaul; but when? She suggested an answer from the new gold coins—blank on the front face, horse on the reverse—introduced here around the earlier 50s **B.C.** Warwick Rodwell, 1976 (from lecture when Dunnnett was in press), made bolder use of these coins (Rodwell, 1976, 194 ff.) But both, like my essay 'New Thoughts on the Belgae' (Hawkes, 1968, 11-12), used the master-work on Gallo-Belgic and early British coins by the late Derek Allen, published 1961 (from lecture of December 1958). So before I can proceed, this matter has to have a new look.

2. The question of Belgae in Britain: Gallo-Belgic coins

There was nothing of it yet when I lectured in 1950. So I could pass by the early (that is, uninscribed) coins, whether British or from Gaul, without apology. I held the subject over for the later coins, inscribed with names. On these there was a master-work already, by Allen: his study printed 1944. When his work on the early ones appeared (1961), with its appendices including, in their 180 pages, a gazetteer of them all and all inscribed coins too, and when, with coloured maps, he abridged it (1962) for the *Map of Southern Britain in the Iron Age* (Ordnance Survey), the result was most rightly an impression that was deep and wide. His classification system sorted the coins having origins in Gaul—Gallo-Belgic A, B, C, D, E and F—from the insular coinages, British A and B to Q and R, with the inscribed ones next. The 'uniface' (obverse blank) gold coins, which Miss Dunnnett guessed for Belgae in northern Essex, are spread more widely: to the Fens and Upper Thames from there, and into Surrey out of Kent, besides coastal Sussex, and even up west of the Wash (Rodwell, 1976, 197, fig. 7); in Allen's system, they are Gallo-Belgic E.

His general thesis was of movements of peoples from abroad; the E-coin invasion, he suggested, had at least three 'prongs' (Allen, 1961, 113-15). Welcome (1964) to R. P. Mack, and to myself (Hawkes, 1968) again, this belief in a 'major surge of invaders or refugees into Britain', bringing E coins which Caesar would be finding here in use, was a factor affecting in all our minds the old problem of identifying 'Belgae'—or of recognising these from their archaeological material. In 1950, before we had Allen, I repeated what I had written in 1930: the people said by Caesar to have

crossed 'out of *Belgium*' were those whose material included wheel-turned pottery, metal-work with it, and the funeral rite of cremation. All represented the third of the conventional divisions of the European Later-Celtic Iron Age, named from La Tene in Switzerland, and thus La Tene III. Allen, presenting our Gallo-Belgic coinages as six, of which E (and F) should somehow have a link with Caesar, thus multiplied the movements, and shook what in 1930 could be offered as a fixture: Hawkes and Dunning on 'The Belgae': publication date 1931. If the E coins did suit La Tene III material as Belgic, all its distribution that was outside theirs must mean it had extensions, made without them. So this meant studying the material afresh, for itself and independently of coins; and the first big study of the kind, Ann Birchall's, was published in 1965. Rodwell, with the second (1976), combined a further study of the coins. I return then to Allen and his Gallo-Belgic system; how do numismatic scholars view it now? A principal viewer, through the last dozen years, has been in Belgium at Louvain: Dr. Simone Scheers. Over here, comparing and extending her work with his own, Dr. John Kent of the British Museum is another. From their work I can only draw a sketch, repeating what appeared (rather much of it in notes) in my *Britain and Julius Caesar* (British Academy), 1978. (The pages there are 142-5, 164-5, 177, 184: Scheers in bibliography, 190-1.)

Gallo-Belgic E is not to be explained by invasion. It is part of a coinage which Belgic Gauls, in the winter 58—57 B.C., when the first year of victories by Caesar had shown them his intention of assailing them next, struck in conformity and issued as a symbol of alliance. And a twofold cause can be advanced for its carrying to Britain. Missions, in aid of fighting Caesar, will have brought it here in quantities to further their appeals; and when he conquered, in summer 57, refugees would bring more. (He records some of these: *De Bello Gallico* ii. 14, 2.) Gallo-Belgic D and the preceding C are in Essex too sparse to affect us directly. But C starts just after 70, not 'about 100' (Dunnett, 7-8, optimistically taking after Allen); it was not, as I thought in 1968, issued for the 'high-king' Diviciacus, a Belgic ruler both sides of the Channel (so Caesar, ii. 4, 6-7), for even in his home in Gaul (round Soissons) all coinage was later than his reign. Yet he had, nonetheless, so ruled. For Caesar was told of him, in spring 57, by senior Gaulish nobles who remembered him themselves. His reign was therefore in the century's earlier years. His likeliest adherents in Britain would be tribes that were coin-less then, like his own tribe; and our earliest coinages, before his time, leave room outside their distributions, where tribes still coin-less could be those that acknowledged his supremacy. When they did start coinage, it was after Gallo-Belgic C, and thus subsequent to 70. But the earliest, which leave them aside, are here much more important. Not so much Gallo-Belgic B, which has its centres in Surrey and to west of London, as Gallo-Belgic A—it is the earliest of all—of which Essex has very many more. On this, Scheers confirms and improves on Allen (Allen, 1961, 100—2): its starting-date is as early as about 150.

The western portion of Belgic Gaul, toward the Channel, not inland as on the Marne, was the portion that is named by Caesar distinctively as *Belgium*. This is in my 1968, 6-9, with the authorities. And that was the part from which in Caesar's book v. 12, 2, invaders had previously crossed into 'maritime' Britain. Its nearest people to us here were the Ambiani. Centred round Amiens, they commanded the mouth of the Somme. They were ahead of all Belgic Gaul in the adoption of coinage, and what Allen called Gallo-Belgic A began from them. So too (at its much later date) did our C, and finally our big share of E. The A coins, big gold staters and their quarter-coins, are often found clipped and very worn: their time in circulation here was exceptionally long. They divide into an early set (with two rare variants), and a later, distributed more widely. Scheers sees them brought by arrival in successive instalments; Rodwell has defined the extensions attested by the later set (Rodwell, 1976, 183-7: pair of maps). Slight in East Kent, that in West Kent passes into Surrey and across the Thames Estuary also into south-east Essex; a group appears newly in western Hertfordshire and Bucks. Essex from Chelmsford north and east has little expansion to show, as the earlier set was in use in North Essex already. Here, besides E coins, are most of the county's few and scattered B, C and D coins; through to E from early A it seems a constant coin-using area. So it could have been Belgic if Belgae had introduced A. But where are the archaeological kinds of material matching with that, and distinguishing this North

Essex from the areas around it? Material 'Belgic' in the sense of 1930/1950 is evidently later: it is clear La Tene III—even if some pottery within it has a start before Caesar. No wonder Dr. Ian Stead of the British Museum (1976) has called on us to stop using 'Belgic' in any sense at all.

3. The 'Belgic' and earlier phases of the region's Iron Age

'Belgic', from 1930 on, had meant an archaeological 'culture', the derivative La Tene III culture of the British South-East, brought here in the middle of its time in Belgic Gaul, or roughly '75 B.C.', by Caesar's people 'out of *Belgium*', so that history and 'culture' could be seen to coincide. Thirty years on, so having Allen's new system for the coins, I could change that '75' to match his date, soon after 100, for the arrival of his Gallo-Belgic C (my 1959/1961, chart fig. 4). Professor Frere, from the war-chariots used against Caesar here, which in Gaul had become extinct soon after 100, could suggest a date barely before (Frere, 1961, 84—5). But this, for the chariots, gives a lower limit only; the C date given by Allen, on the contrary, is now too high. So in 1968, though still repeating his errors on the C and his later ones, I was happier in harking back to his B and his A. Both were out of *Belgium*, and the A were of Ambiani; both were here before 100 (though the B by not very much). And the starting-date of A, confirmed by Scheers, stands now about 150. Second-century folk with them must have the culture then prevailing, not any first-century La Tene III 'Belgic': what was prevalent prior to III was La Tene II culture. (My 1968, 13 col. 2—14 col. 1 and onward to end of paragraph.) This is in Belgic Gaul known chiefly from graves, as amongst the Ambiani at Port-le-Grand (Leman, 1976); but of those, in South-East Britain, we have scarcely any. There is distinctive metal-work, imported or adapted over here; associations, however, are rare—most finds are from rivers. On pottery, foreign La Tene II influence has not been generally admitted. It is early La Tene I influence, previous again, that is recognised to give fresh styles, from before 400.

One of those, in central Suffolk—thus named from Darmsden—and western Norfolk, has a Surrey—Thames region that perhaps is really distinct but extends into Essex, in the neighbourhood of Mucking, where it first was published from Linford (1962: Barton, including Hawkes). It can truly represent, at its outset, invaders who impinge on an older population. It certainly reflects the angular forms of La Tene I Belgic Gaul—which are not to be taken as confined to the 'Marnian' region where they first became famous. When Harding re-stated the ideas of such invasion, using metal-work evidence besides (1974, 155—76 and 230), he had in Essex's centre and north no more for it than Cunliffe (1968, 178-83; whence 1974, 36 map, 'Darmsden-Linton', 39-40)—whom Miss Dunnett nonetheless well quotes for a martial immigration (1975, 7), expecting it in Essex as in south-east Britain altogether. Invading newcomers, far from driving an older population all out, must at once make sure that it will stay and keep working on the land. If their elite's own followers, as soon as there is peace, will themselves start labour in the fields, old and new can fit together in the rural economy. In central Essex (east and south of it in places), those fields can be recognised still, from their pattern in the later landscape (Drury, 1979b). Features of the pattern are not only of the Late, but of at least the Middle Iron Age already; this seems to start with economy stable. Amongst its settlements, some continue later (Rodwell, 1979a). But a sequence wholly Middle (till its end), from round 250, is seen at Little Waltham: Drury, 1973, 1978, 1979a. First an open settlement, with many round houses, in the early 2nd century it was changed to an enclosed one, directly to the north, in which the two round houses found were structurally novel (one with porch). The old site was turned over to stock-yards; and these, with the enclosing of the new, imply more cattle. Pottery, however, is of Middle Iron types all through. Some of them have matches from the hill-fort recognised at Witham (Rodwell, 1979b, c: long before the Saxons of Miss Dunnett's p. 5). Near Colchester at Ardleigh (I have more on it below) is an enclosure likewise ditched but smaller, with a single house. Its pottery is Middle Iron Age almost entirely. Has either site anything to answer to a start, at 150, for those A gold coins? Might the Middle Iron Age pot-forms, jars with scratching and combing on their sides (finger-printing only on the top of flat rims) and smooth bowls often with a foot-ring, come in—as may the coins (and the cattle)—as result of an invasion?

But the talk has been not of foreign forms of pot, as at Linford or Darmsden before. What is novel now is coinage. It is taken for a new-style invasion. For Cunliffe (1974, 59-60), though hardly touching native folk-culture, this began 'the reformation of tribal society', resulting in what Caesar found complete. Yet before we come to that, there is more we should observe about our oversea connections altogether.

4. The relation of links with Gaul to links with Germany

Middle Iron Age pottery does, for me at least, include rustic reflections of La Tène. But they cannot be insisted on as following the A coins' advent, about 150: if not just locally developed from those that were carried in the Darmsden style, they should still mean an influence at work from at least 250. And all connections then need not have been with Gaul. Over the sea to the east, in modern Belgium, the Netherlands and Germany, are lands just within or else out on the periphery of full La Tène Iron-Age culture, having some of its features yet also resemblances to others characteristically British. Round houses, though indeed expectable in northern France, as are also deep store-pits, have eastward distributions that lead on right into Westphalia; so have four- or six-poster emplacements for so-called 'granaries'; so have Iron Age sling-bullets; also triangular loom-weights—pre-300 at e.g. Linford (Barton, 1962, fig. I, 11). To Champion, 1975, where much of this evidence is summarised, additions for the weights have been mapped and published by Wilhelmi (1977a). And although these features' similarities to British may have partly common origins behind them, it is when our Middle Iron Age had started that they seem most widespread. Invasion can hardly be held responsible for that.

The impulse spreading them then (which excludes North Europe) in the first place looks like an outward one from Britain; but influences also are apparent in the opposite direction. Already in the Thames-side pottery grouped with his Darmsden style by Cunliffe, as at Linford (Barton, 1962, figs. II—III; date from 5th century now: Jones, 1968, 214), the finger-printed coarse-ware is closely matched in West Germany, in the Hunsrück-Eifel area beside the Rhine as it flows north-westwards (Hawkes, 1962, 86; Neuffer, 1938/39). And amongst the finer wares there, 4th century at earliest, there are vessels with designs impressed on the clay with stamps. Mediterranean in an inspiration that also worked on coasts of the Atlantic, in inland Europe's La Tène they are an eastern feature; so the nearest to Britain this way are the Hunsrück-Eifel ones. They occur in the 4th but more often in the 3rd-2nd centuries: Dehn, 1951, bowls; 1938, 1, 123 Abb. 75, 1, with roulette-arcs, 130-1 others, 133 dating; Joachim, 1968, 103-5 early, 130-1, 137 as Taf. 34, 41, 42; dates 151—3: end 100 B.C. or just after. Need it surprise us that in eastern England, inclusive of coastal Kent and Essex, there was stamp-impressed pottery before the Middle Iron Age was out?

This Eastern English style on pottery, with arc designs and with stamps, has been researched most thoroughly by Sheila Elsdon (Elsdon, 1975). As against the Continent's western La Tène, with its development of floral and vegetal designs, she finds (44) its affinities closer with the eastern. Though the eastern is earlier, the Hunsrück-Eifel might well have passed the stamped style on to us, in a context of other Middle Iron Age relations, to develop as the style that followed here. Its starting-date is prior to 100; thus it fits with this new-suggested derivation. Here would then be a further sign of our contact with western Germany.

Still another, again East English at an outset that certainly appears 2nd century, is the starting of our celebrated currency in bars of iron (Allen, 1967). The notion of this most probably was brought from Westphalia—mentioned for the other signs already, and an iron-rich region. After Champion, 1975, and Allen's considering Germany, we have now Wilhelmi, 1977b; he augments the Westphalian evidence, and maps the whole range. Nowhere on the Continent, west of the Ardennes, are these currency-bars known at all. Our trend altogether to relations farther east stood away from the Gaulish connection, as established in the 5th century and round 400. I would see that Gaulish connection's renewal, in the movement that brought us the A coins, as a purposely launched reassertion, keeping its hold through political ascendancy. For only through this can a

coinage, however imposed, be expected to endure. And the A coins did so endure, it seems (when the later ones are added to the early), in our Essex regions at least, for up to a century. The ascendancy they show was of course no hindrance to the pottery with arc designs and stamps; but the currency-bars, apart from six Thames finds and one within an East Kent hill-fort, are absent from South-East Britain, being scattered from the Humber away south-westward. What kept them away should then be the power with the A coins. How much is there to show that this power made a forcible invasion?

5. The record in Caesar of a movement 'out of *Belgium*'

My suggestion of local conquests, in each of the early A-coin areas, of natives little altered otherwise (1968), was from scattered signs only. Not much was yet known about pot-forms: no Little Waltham. It was stiffened, we have seen, by Cunliffe and by Rodwell; they both have agreed with it as fitting, better at least than anything later, with the movement 'out of *Belgium*' recalled by Caesar. For *Archaeology in Essex*, from the Clacton Conference of March 1978, I re-stated it (1979a, 55). And I added, since among the signs there are La Tene II swords, our first really long swords in the whole of the period, that their scabbards on the Continent are regularly made in iron, like the blades themselves. There are six in iron from the Thames and the Walthamstow Lea. The bronze scabbards, stressed by Piggott (1950), will be British adaptations, as he said; the iron ones, primary and foreign, can be signs of an invasion. This can have effected the ascendancy implied, in their regions of occurrence, by the A coins. But was it that movement 'out of *Belgium*'—which Caesar never dates?

The coins indeed will suit it within their areas, in Kent and North Essex. Yet in the record as given by Caesar it populates the 'maritime part' of all the island. The tradition of the folk in the 'interior' pronounced them native. Most of them sowed no corn, but subsisted on milk and meat, and wore skins (Caesar, v. 12, 1–2, with 14, 2). That is Highland-Zone Britain, contrasted with a 'maritime' conceived as stretching through the Lowland, not stopping at the Thames where Caesar crossed it (in the west of our Greater London). He had indeed found 'maritime states' stopping there, and Trinovantes stopping (though he hides it) on the Lea. But directly beyond, in the kingdom of Cassivellaunus, were fields—which his own troops ravaged (v. 19, 2–3); this was never the 'interior'. It is not Caesar's narrative that tells of the invasion 'out of *Belgium*'; its launching, into undefined 'maritime' Lowland, is related in his 'British Excursus': three chapters (v. 12–14) that interrupt that narrative, and stitch together notes including borrowed material that is older. This is explained in my 1978, at 165–70; the double sense of 'maritime' was missed by Harding, 1974 (223–6, with map) and already by Avery—summarised 1976, 142, note 103, from unpublished original, 1969. Doubtless it was played on purposely by Caesar; today it can be seen exposed. The invasion 'out of *Belgium*', and the two-part Britain, are amongst the material that he borrowed.

The source can only be Greek, and of a time when Britain was inadequately known. The chief Greek ethnographer whom Caesar would be drawing on was certainly Posidonius, who was in Gaul in the 90s B.C. (With my 1978, 130–1, 144, 165, 167, compare Frere, 1961, 84–5 here again: the Gauls' war-chariots that he knew are not datable later.) Though there possibly might be other Greeks, none can be fancied before 100, on account of long wars which had ended just then; Posidonius took the lead in writing them up. So the invasion into Britain is at latest 2nd century in date. And this just suits our La Tene II traces, and the coins. But was it really the single event that the record in Caesar is making it? If moves began earlier, would the memories in *Belgium*, which interpreters would have to put in Greek for a Posidonius, make the story sufficiently plain for sorting them out? The 'on-going sequence', which I offered at Clacton (1979a, 55), may be right as I gave it, but does it extend back far enough? The political ascendancy imposed by the Ambiani with their swords from *Belgium*, expressed from about 150 B.C. by the A coins, might really have

been an imposition on people who themselves had come from there—with the angular pottery, Darmsden and the rest, which had started from before 400. Once make the record in Caesar's *Excursus* represent an accumulative story, telescoped there into a picture of a single event, and one finds that a beginning so early would fit the archaeology surprisingly well—which may not be always the case with its subsequent stages. The idea would suit former essays (my 1972 and 1973).

Yet the best we can start by doing is to stick to what we have of it. The movement as given us starts with plundering and warfare. The invaders next settle, and begin to cultivate fields. And the tribal names by which almost all are called (present tense) were the names of the tribal states that they came from in *Belgium*. What names? When I lectured in 1950 I was taken to be right (as in 1930), that one was the name of the people of Cassivellaunus, just over the Thames: they were anyhow believed, at that time, to be Belgic invaders. The name, spelt *Catuvellauni* (Greek *Katou-*) in Romano-British times, was thought the same as *Catalauni*, the name of a people back in Belgic Gaul. But these, around Chalons, were inland, beyond Caesar's *Belgium*: Hachmann, 1962, put me on to that, for my 1965; thence 1968, 6-9 with my maps; 1978, 142 with note 2, 168 with own note 2. Cassivellaunus is seen now as not an invader at all.

The two good Belgic names South Britain has got lie away from the A coins: Atrebates (Thames—N. Hants—N. Wilts., with presumed second group in W. Sussex); and from central Hants out westward, Belgae itself. The evidence for this as a (nfe-name is Roman only; the Atrebates evidence, Roman too, can be stretched back guessably from coins. Opinion today is not clear about either's introduction. The old explanation (my own, from Bushe-Fox) was a late 'Second Belgic' invasion; but this was put in Caesar's time only by presuming the *Excursus* to be totally his own; nothing similar can come into a source at least forty years older. No tribe-names, therefore, can at present be explained by an invasion with the A coins dating it. And there is worse: although A, and by a narrow margin B, do suit that source in their beginning-dates, A was issued, in *Belgium* and for Britain, by the Ambiani alone. It begins here, in East and West Kent and North Essex all alike, without local variation. B, though less consistent and from different departure-points, is centred here only on the lower-middle Thames and in Surrey. But the *Excursus*'s invading tribes, nearly all with own names, are quite evidently many. They overspread a 'maritime' Britain that is left undefined; the coin-regions, even in the later A period, appear too restricted and few for them. So the recent swing towards fitting them wholly to the coins may be judged to be excessive. Were the A coins, perhaps, though in issue Ambianic, yet accepted amongst the many other tribes? Or did these, in every case bar the later one with B, stay coin-less? Is either suggestion more than the merest guess? And if all were just an elite having little effect, in a class society, on a peasant population with our settled Middle Iron Age culture, why are we told that they soon began cultivating fields? Is 'accommodation with natives' enough for an answer? Or have we a better, perhaps, in the Colchester region? We ought to return there.

6. Colchester archaeology: the sites and earthworks

The Colchester region had a Bronze Age ending with a Late phase (towards 9th century). At Sheepen, part of the site that was afterwards Cunobelin's was occupied then; besides the great cauldron (Hawkes and Smith, 1957, 160-5), and some lesser bronze finds in the excavations there, from 1931 on, most of the top and upper slopes of the hill produced pottery. At the time thought Earliest Iron Age and thence called 'Hallstatt', it has its true Late Bronze Age date now affirmed by John Barrett: publication shortly forthcoming. The range, barely touching Early Iron Age, leaves thenceforward no trace of occupation, till the early 1st century A.D. or shortly before, from whence it lasts through Cunobelin's time into Roman. Miss Dunnett's excavations—to be published soon too—will enhance and help sharpen the picture. But the gap at Sheepen is long; compensation must be elsewhere. Mr. Felix Erith has provided us with some of it at Ardleigh. From his Bronze Age cremation-urn cemetery there, of a phase before the Late one at Sheepen, a lucky air-photograph gave him an Iron Age dwelling-site: an oblong ditched enclosure with a

house inside it (Erith and Holbert, 1970). Round houses sited in enclosures have examples elsewhere, but his, being single, is not so easy to match; to Harding (1974, 30-2) it was still unique. In advance of publication, he kindly showed me his pottery; one can see it now as essentially Middle Iron Age, though lacking some forms got at Witham in the 1930s, now to be published with his own by Rodwell shortly. Fuller runs of Middle Iron Age pottery seem absent from the Colchester neighbourhood till now. Yet its period is thought to run on towards an end about 50. In Gaul, what ends about 50 is of course the Late phase; what runs on then into Roman is its rite of cremation, but its own La Tene III culture can show this everywhere. Over here, 50 is the date when cremations are beginning. It opens the phase which Stead has named 'Welwyn', from the place in East Hertfordshire renowned for them. The phase lasts on till nearly the century's end. Leaving Middle behind, we now have our own Late Iron Age.

This period gives us pottery regularly wheel-turned. Was our use of the wheel introduced about 50, therefore, and nowhere sooner? For Rodwell, wheel-turned pottery did start sooner: relatively coarse, but remarked on sundry habitation-sites. He would line this up with what, in Kent, Ann Birchall proposed, from cremations at Aylesford, as 'Early' and 'Earliest' forms of our so-called 'Belgic' (Rodwell, 1976, 221—37, with map and nearly forty vessels figured). For Stead, on the other hand (1976), being later than 50 when occurring with cremations, it need not start at all sooner on any habitation-sites. These rather coarse wheel-turned forms could be quite long-lived: some of them are certainly attested at Colchester from Sheepen, yet sherds of them occur at Little Waltham already at the end of its Middle Iron sequence. So when were they first introduced? Was it really before their occurrence with cremations? What is anyhow clear is that wheel-turned pottery does not, by itself, mean invasion. The idea that it did (1930, 1950) was a consequence of thinking that a 'culture', a package with numerous manifestations, must introduce all of them together. Remoter prehistory may see it like that, because the time-scale there is more relaxed; the margins of history, as here, need notions less crude.

At the Essex Archaeology Conference, I rather made a point of this (my 1979a, 57); with the 'Earliest' and 'Early' pots here I compared what Port-le-Grand could use for a cremation: very like those, yet in outset still La Tene II. (Rodwell's mention is confirmed by Leman: see Leman, 1976.) We know from the coins that the Belgae affecting us the most were the Ambiani. Port-le-Grand is a cemetery right inside their country. The political connection implied by our coins could have cultural consequences further. Knowledge of the potter's technique of the wheel, and belief in the virtues of cremation, could each have been passed from them' here, yet not—through any necessity—together. They are different in kind. The connection came only to an end with the fugitives from Caesar. If nobody cremated over here till these came, then to that extent Stead will be right; yet wheel-turned pottery for use in habitations might still be introduced here sooner. Rodwell has claimed it. But where is a site that will clinch it?

Colchester might have the answer: at the place called Gosbecks. Just beyond the boundary of the Borough on the Maldon Road, a little past Shrub End, the fields of old Gosbeck's Farm gave the site where Hull dug, and also John Brinson (Hull, 1958, 259-71). It had in Roman times a theatre, probably baths, and a temple. This was in a triple-walled garth with inside it a ditched one; and the ditch's first pottery and coin were both pre-Roman. Air-photographs have marks of enclosures crowding the area. Such a religious and festal centre, away from the Colonia of Roman citizens, must point to a special importance for the British Trinovantes. Arid the summary by Rosalind Dunnett (1975, 16-22, 108, 114—15) declaring this rightly and comparing sites in Gaul, not only remarks that the finds include some that can be older than Cunobelin's time, but recalls that around the brows of the slope falling south from the site, and then west, an earthwork has long been perceived, which bends into line with the oldest of the Dykes. This is Heath Farm Dyke, which my own excavations (farther off, on old Prettygate Farm) show as older than Lexden Dyke, a principal member of the main dyke-system. *Camulodunum* // will make this clear; but it reflects on Gosbecks. Lexden Dyke itself may be just a little older than Cunobelin's time; how much older again Heath Farm Dyke is, we as yet can only guess: before 50? Then is the earthwork at its end round Gosbecks

earlier still? And has the Gosbecks site a beginning in the full Middle Iron Age? The slopes that the earthwork crowns make it something like a hill-fort; the fort above the river at Witham is of just that time. A date before 50, anyhow, could mean before Caesar. Caesar in 54, well-informed already on our Essex Trinovantes, calls them 'almost the strongest tribal state of those regions' (Caesar, v. 20, 1). Cassivellaunus's alone could be stronger. He had killed their king, whose son had fled for his life to Caesar in Gaul—the prince Mandubracius, whom the people now asked for back again. This was no tribe having only Middle Iron Age farmers: it had Cunliffe's 'reformation of tribal society' complete.

In my Academy and recent Essex lectures I have shown how Caesar had intended to employ it: for a landing farther than Kent, to let him fall upon Cassivellaunus by a march assailing Hertfordshire right through Essex (1978, 158-61, 171-2; 1979a, 55). And though the wrecking of his fleet off Kent by a gale made him march to the Thames instead, he from there did at last reach Essex, at its north-west corner, restoring Mandubracius and securing him from any repetition of the harm he had suffered (v. 20 followed by 22, 5). The sequel, the elite's prosperity shown by the contents of the 'Welwyn phase' graves, which include fine vessels for wine and the amphoras it came in, brought from Italy as Peacock effectively showed (1971, 173-7)—whether East Herts lords were Trinovantes or only allied to them—has been duly underlined by Stead (1976 and refs.). No such graves, until late in the century at soonest, are to hand round Colchester. Perhaps Mandubracius and his tribal elite stayed near to their western borders, the better to ward off harm if it eventually came; or perhaps near Colchester are Welwyn-phase graves still unknown. But the modern development west of the old-time town, towards Lexden especially, where graves of the next phase cluster, would almost certainly have come on any earlier than those, if they had been within its extent and not outside it. Supposing a centre at Gosbecks, and not yet Sheepen where the later phase has it, we could guess some findable still, in locations farther. Putting guesses aside and focusing on Gosbecks itself, as at least part-known, we might find courage for renewed exploration of the site. (Philip Crummy has a forthcoming article in *Aerial Archaeology*.) It was one of Hull's dearest wishes—and is likelier than anything, so I believe, to bring some filling of our Colchester Iron Age gap.

7. Colchester a capital: problems of the passage into history

The graves and habitations known commonly as 'Belgic', at Colchester and close round about it, are at present—if we use the terminology of Stead—all put into his 'Lexden phase'. The transition into this, from the Welwyn phase before, needs datings more exact than we yet possess. We can only expect them from discoveries on well-dug sites. Those listed by Miss Dunnnett (1975, 14) for continuity into the Late from the Middle Iron Age—expectable or apparent—are in southern or central Essex. But less far off, we shall soon know more about Kelvedon (Rodwell, forthcoming); and dating-points gained on any one site ought to offer more clarity to all. Coins, though as yet not usable exactly as site-finds, at least do now introduce us to rulers' names. Passing by the British ones not so inscribed, of which the newest treatment known to me is Rodwell's (1976, 243-8), we come to the twenty-five years (or thereabouts) which Cunobelin's arrival will end about A.D. 10. Behind all recent treatments is the basic one of Allen, *Archaeologia* for 1944. On coins of these years we have Dunnnett (1975, 12-15, 18-20, 27-9) and Rodwell (1976, 249-63). They give us first a king Addedomarus; after him a king Dumnovellaunus: some obscurer names at the time of the first of Cunobelin's; and finally, from 10 or very near it, Cunobelin alone. (His gold was treated by Allen, 1975.) But his father, Tasciovanus, who minted in the Hertfordshire realm at Verulamium, has rare coins designated CAMV, for Camulodunum. Granted that they show he had a short-lived extension of rule to a centre at Colchester, one has to ask where. Perhaps Gosbecks? Or Sheepen already?

In my lecture of 1950 just as in the *Camulodunum* volume, the starting-date for Sheepen was equated with Cunobelin's obtaining sole rule, about A.D. 10. This was questioned by Peacock

(1971, 178); the wine-amphoras, Hull's form 181, from Italy, where they are the form called I by Dressel, make a good 10 per cent of all the early amphoras at Sheepen, yet their last Italian date (among those that are inscribed) is 13 **B.C.** Rather than suppose a longer life, or an earlier arrival for Cunobelin, he preferred to resolve the discrepancy by starting Sheepen before Cunobelin. He can be right, for Hull finally saw that Sheepen's other Roman imports, as proved now in Germany, must start before **A.D.** 10 by some 10-12 years. That also affects the Tumulus and near-by cemetery at Lexden. I will only recall, since Philip Crummy has it in hand, that in the cemetery's grave with the mirror, done in handsome Celtic style (Fox and Hull, 1948; Fox, 1958), this had a date for its making, from Fox, prior to Hull's for the associated pottery, **A.D.** 10-25, to-day itself over-late. Not to me of course in 1950, when the work was new-published, and mirrors and their art made the brief second topic of my lecture.

My third topic, briefly treated also, was trade: at Camulodunum with the Roman world oversea. But this, as including wine-trade and amphoras, brings me now back again to Peacock. One can share his aversion from extending, very far, the Dressel I amphoras' production-life. Yet I would mention the fact that amphora-bodies—retaining necks or not—could have a longer-lived utility after they were emptied. Latin literature shows them serving at Rome as street urinals; more relevant here is their employment for fresh-food storage. They were coolest when buried, upright to be readily got at; I have seen this found in Spain and at Chateaumeillant in central Gaul. It must be elsewhere too, though of course they could always be placed more movably—and anyhow already have been moved from the place where the wine they had held had been drunk. Inevitably broken (unless deep-buried) in the end, their fragments would often get mingled into later deposits. Yet if this occurred at Sheepen, the drinkers of their wine wait still to be located somewhere. At Sheepen already? Perhaps near the river, where there must have been wharfage for the wine-ships? Or perhaps elsewhere. If so, still not far off. Abodes of the elite within the twenty-five years before 10 need not have been here; yet certainly they ought to have been within a quite short distance.

Meantime, I have done what I can to up-date that lecture of 1950. I have here looked back on the progress since then, remembering Hull. To advance it means looking ahead. He would always do that.

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