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Camulodunum 2

by  
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To the memory of  
C F C Hawkes,  
M R Hull, and A F Hall

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## Camulodunum 2: a lifelong commitment fulfilled

In 1930, at the age of 24, Christopher Hawkes readily accepted an invitation to carry out an excavation on the site of the intended by-pass at Colchester. Little did he know what he was taking on. The excavation at Sheepen was to turn into a major project taking ten years, and his fieldwork in Colchester was to last considerably longer. He already had a full-time job at the British Museum, so he could only find time to excavate by giving up his annual leave. Later that year, sensing that it might be a long-term commitment, Christopher wrote in a letter to his mother

*'...it means sticking to it pretty hard — in this case throughout the winter writing up results — and then probably next year more work here: heaven knows how much longer I shall be committed to working in this place!'*

Christopher's association with Colchester very nearly ended up being much briefer than anybody expected. The following year a trench across the Sheepen Dyke collapsed, burying him and J P Bushe-Fox (the Chief Inspector of Ancient Monuments) for at least quarter of an hour. Mercifully Christopher survived the ordeal unhurt but sadly poor Bushe-Fox never fully recovered.

In 1932 Christopher turned his attention to the dykes and, with the help of Thalassa Cruso (later Hencken), carried out the first excavation of those earthworks. He was to become great friends with Rex Hull and A F Hall for whom he had both affection and respect, and together they were to account for a campaign of exploration of the Iron Age settlement which was to span almost thirty years. Hall felt himself inadequate when it came to publishing but was happy to feed Christopher with information. Rex Hull's great strength lay in finds rather than excavation, so they made a great team.

The digging at Sheepen finally finished in 1939 and publication in the form of *Camulodunum* followed in 1947. This was a considerable achievement considering the intervention of the war and the volume of finds which needed to be processed (forty tons). The book became a classic. For me, Christopher's treatment of the residual pottery (pp 174-80) shows a sharp and perceptive appreciation of the limitations of stratified assemblages which was ahead of its time and which signals to the reader a carefully-considered book of real quality.

In 1946 Christopher was faced with new challenges when he was appointed to the newly-created chair of European Archaeology at Oxford. Throughout the subsequent years until his retirement in 1972, he was a busy professor, founding the Research Laboratory and the Institute there. But for Colchester he had no funding and no research assistance, and yet it was to Christopher that the Ministry of Works turned when there was a need for excavation of the dyke system. The publication of the Borough's development plan in

1951 was a particularly important event since it gave the go-ahead for the development of large open areas in the western suburbs of Colchester, and prompted an appeal to Christopher from John Hamilton at the Ministry. As a result Christopher returned repeatedly to Colchester to carry out various investigations which culminated with his spectacular section across the Triple Dyke in 1961 (p 58).

Two years later, in 1963, he told the Ministry with considerable relief that he felt there was no further need for him in Colchester. The occasion was the foundation of the Colchester Excavation Committee (now the Colchester Archaeological Trust) and the appointment of Ros Dunnett (now Niblett) as its first full-time archaeologist.

Following our excavation at Pitchbury Ramparts in 1973, I wrote to Christopher wondering how we could publish our work in the absence of a report on the 1933 excavation. This led to him readily accepting an offer of practical help with the publication of all his Colchester material: and what you see here is the result. He worked on the book intermittently over a period of fifteen years or so, during which time I assembled a large number of letters and texts all written out in his distinctive large multi-coloured handwriting. His debt to Rex Hull, paid in the form of editing part of his brooch corpus, took far longer than he wanted or planned, with the result that he left the completion of the dykes almost until too late. Nevertheless, despite failing eyesight and even after a heart attack, he struggled on to within two years of his death in 1992 to fulfil his long-standing obligation and love-affair with Camulodunum. Sixty years on, it was to be the last piece of archaeological work he was to do: his archaeo-historical essay on Camulodunum and its rulers (pp 88-94) is vintage Hawkes and is a masterly demonstration of how to combine scholarship with dirt archaeology. There will be many (myself included) who will disagree with some of his conclusions, but our great debt to him will no doubt be evident in the way his work will underpin research on Camulodunum and its dyke system for many years to come.

Christopher, although always quick to acknowledge the help of others, never wrote any acknowledgement as such for the book. Given time, there would undoubtedly have been a long and generous testimonial. But we can be sure that his enormous drive to complete the work stemmed from his desire to discharge his debt to that generation of archaeologists, of which he was the last, who worked on this grand project so many years ago.

Philip Crummy  
March 1995

[Sources include: Sonia Hawkes; *Hawkeseye: the early life of Christopher Hawkes* by Diana Bonakis Webster (1992); and D W Harding's memoir of CFCH's life in *Proc Brit Acad*, 84 (1993). 323-44]



*Above and left:  
Christopher Hawkes with Joan Blomfield at the Sheepen  
excavations in 1931.*



*Below:  
Christopher Hawkes (right) with Colonel R J M Appleby in April  
1952 on the excavations at the rear of Gryme's Dyke.*



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*Philip Crummy*

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# 1: Introduction

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## ***Camulodunum 2***

Publication of a *Camulodunum 2* had been planned for many years. In the mid 1970s, CFCH and myself decided that we would produce the book jointly since much of the more recent work at the time affected that of CFCH, and the Trust could provide him with drawing and typing assistance. Most of CFCH's original texts were entered on to a word processor by Pat Brown, and his draft plans were redrawn for publication by the late Terry Cook. Throughout the 1980s CFCH edited and saw published the pre-Roman section of Rex Hull's corpus of brooches (Hull & Hawkes 1987). He also deliberated much over three other Colchester topics, namely Jennifer Foster's re-appraisal of the Lexden Tumulus (Foster 1986), the controversy about the start-date for the occupation at Sheepen, and Colin Haselgrove's interpretation of it (Haselgrove 1987a, 163-71); these all led to substantial sections in this book.

CFCH was working hard on *Camulodunum 2* even when he became very ill, and he died certain that he had finished it. However there seem to be a few gaps. He never gave us a finished account of his Triple Dyke excavation, and he does not seem to have completed the section he planned on Roman roads. I found in his papers a near-complete first draft of sections about the Triple Dyke, the 'palisaded earthwork', and Prettygate Dyke, and I have used them here to fill in gaps (pp 55-61). He may well have held these papers back, intending to revise them, but there is no way of knowing. Otherwise the text which appears here is all more or less as he intended.

In general I have interfered with the text as little as possible. I have only made alterations where absolutely necessary, in some cases to accommodate new information. The footnotes and captions are mine, and they are mainly used to indicate those parts of the text which are out of date and which I felt I could not alter. Footnotes followed by '(C Haselgrove)' are based on some of the comments made by Colin Haselgrove on the parts of the text which he kindly read. I have compiled the bibliography and completed most of CFCH's references. CFCH was able to check and revise most of the typed version of his texts, and he examined and corrected most of the illustrations after they had been redrawn for publication, although some have since been updated. Where possible, I have added the extra illustrations he wanted and I have also included some photographs from his archive.

The structure of the book is close to how CFCH wanted it, apart from a few minor changes of order in Chapter 2.

The last chapter was never part of the plan and was written after his death.

CFCH's Colchester archive is now in the Colchester Museum. It contains much of interest, including the comparatively small quantity of finds from his work. It is hoped that one day resources will be found to enable the archive to be properly ordered, indexed and conserved.

## **Other views**

When it comes to writing history based on archaeology, there can be few better places in Britain to do it than Colchester. During a relatively brief but critical period in Britain's past, Colchester was closely linked with major characters whose names, and in some cases deeds, are known to us through ancient histories and coins. Claudius, Plautius, Cunobelin, Caratacus, Togodumnus, Tasciovanus, and Boudica were all associated with the place and so too presumably were Addedomaros and Dubnovellaunus(-in-Essex). And of course the material remains which can provide these people with physical contexts in Colchester are rich and extensive. CFCH was a widely-acclaimed master of this approach although some today find the results rather overdone.

There have been important challenges to CFCH's views on archaeological interpretation, particularly by Colin Haselgrove (pp 77-82), and the question of the high proportion of Dressel 1B amphoras in the Sheepen assemblage has caused much discussion among specialists (pp 76-7). The dating and sequencing of the dykes is no easy matter, and CFCH's views have in part been overtaken by new information which he was not aware of and thus could not take into account. CFCH's contention that there were two different systems of dyke centred on the Sheepen and the Gosbecks sites is not as plausible as it once was, now that the Shrub End Dyke can be seen not to have cut off Gosbecks from Sheepen in the way he believed it did (pp 170-71). He equated his two systems of dyke with the Trinovantes and the Catuvellauni and thus saw them as a physical expression of Cunobelin's acquisition of what was formerly supposedly the Trinovantian 'capital' (pp 7 & 53-4). In the last chapter, I have put forward a less demanding explanation which benefits from recent fieldwork, namely that Camulodunum was from the start a Catuvellaunian settlement, and in effect a colony planted in Trinovantian territory (pp 172-4). Such a solution does not need a dual system of dyke or require that the succession of kings at Camulodunum was bound up with inter-tribal conflict.

Interestingly, it also prompts us to consider in the future whether other sites (the most obvious of which is Chichester) could be interpreted in the same way. Unfortunately we cannot say what CFCH would have made of it all.

## Summary of the Camulodunum excavations 1930-63

Although Christopher Hawkes dominated the study of the Colchester dykes for over thirty years, there were (as mentioned on page vi) important contributions by others. Rex Hull was excavating in the 1930s and again just after World War II, particularly at Gosbecks where his main discovery was the Roman theatre. A F Hall was a schoolteacher at the Royal Grammar School who was a dedicated fieldworker. Although he never published much, he corresponded with CFCH and Rex Hull endlessly, sending them many letters and plans of his work. The Colchester Museum has a large Hall archive, and CFCH's papers contain much valuable material from the same source. No doubt CFCH sifted out the most significant of Hall's discoveries, but nevertheless the Hall archive would still repay further study.

The following list represents an attempt to identify the main excavations and other items of fieldwork relating to the Iron Age settlement during the "Hawkes' years". The Roman town and its cemetery areas have been excluded (but see CAR 9). The list has been compiled to help clarify the sequence and nature of the fieldwork.

### M R Hull (MRH)

- 1930-39 Sheepen (with CFCH) (*Camulodunum*)
- 1929 Ditches west of Berechurch Dyke (p 137)
- 1936 Peartree Junction (pp 46-8)
- 1936 Trench dug diagonally across Gosbecks temple (*Rom Col*, 261-4) (p 101)
- 1936 Section across the Gosbecks-Colchester road (*Rom Col*, 9-10) (p 68 & 104)
- 1936 Section at Iron Latch Lane across the Roman road (*Rom Col*, 12) (p 68)
- 1939 Altnacealgach and Heath Farm Dyke North (pp 62-3)
- 1948-9 Gosbecks, theatre, temple enclosures, and others (*Rom Col*, 259-70) (p 95)

### Christopher Hawkes (CFCH)

- 1930-39 Sheepen (with MRH) (*Camulodunum*)
- 1932 Survey of dykes (really by T Cruso and H W Poulter) (P 22)
- 1932 Lexden Dyke (pp 35-45)
- 1933 Pitchbury (pp 139-43)

- 1947, 1948, 1952, 1956-7 Gryme's Dyke (following on from Hall 1946 and followed by Appleby 1958) (pp 27-9)
- 1956-7 Prettygate Junction (pp 48-50)
- 1956 Section across Gryme's Dyke south of Dugard Avenue (pp 28-9)
- 1957 Section across Dugard Dyke (pp 26 & 108)
- 1957 Section across Kidman's Dyke (p 31)
- 1957 Heath Farm Dyke, Co-operative Society's sports ground (p 31)
- 1959 Section across Lexden Dyke South (p 45)
- 1961 Triple Dyke, Hunter's Rough (pp 55-9)
- 1959 Trench across Lexden Dyke South (pp 45 & 50)
- 1961 Section across Heath Farm Dyke, New House Farm (p 31)

### Royal Air Force

- 1933 Aerial photographic survey by the Royal Air Force (P 22)

### A F Hall

- Shortly before 1939 Roman roads and burials around the Royal Grammar School (roads summarised *Rom Col*, 4-8)
- 1943 Cleaned section across tank-trap across Lexden Dyke to reveal Heath Farm Dyke (p 32)
- 1944 Section across Berechurch Dyke (pp 26 & 160)
- 1945 Three sections across Barnhall Dyke (p 24).
- 1945-6 Section across Berechurch Dyke (p 26)
- 1945-6 Berechurch and Barnhall Dykes in Borough cemetery (p 24-5)
- 1945-6 Occupation site west of Berechurch Dyke (p 137)
- 1946 Gryme's Dyke entrance, continued by CFCH 1947 & 1948 (p 27)
- 1950s Three transverse cuts in Field 1338 across Kidman's Dyke North, one continued across Heath Farm Dyke Middle (p 33)
- 1959 'Search trenches' to find Lexden Dyke South (pp 45 & 50)
- 1961 Section across Dugard Dyke (pp 26 & 109)

### R J M Appleby

- 1952 Cropmark complex on north bank of river Colne: probably post-Roman (p 131)
- 1952 Enclosure at West House Farm (p 137)
- 1958 Gryme's Dyke (Dugard Avenue, to finish CFCH 1956-7) (p 27)

### B Blake

- 1961 Sections across Layer Dyke (p 34)
- 1963 Section across ditch of Shrub End Dyke (p 51)

### Roman Essex Society

- (presumably directed by R J M Appleby)
- 1948 & 1949 Gosbecks (*Rom Col*, 261-70) (pp 95 & 97-8)

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## 2: Camulodunum

by C F C Hawkes

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### Introduction

#### Natural environment

Since the appearance of *Camulodunum* in 1947, the Iron Age human topography is better known, and beliefs about Iron Age cultures have in part been modified, but the account of the natural landscape, in its essentials, requires no change from that given on pages 1-4 in that publication, with frontispiece map (pl 1). That frontispiece map, though contoured only at 100 ft up from the Ordnance Datum for sea-level, showed how sharply the central plateau is bounded by the two rivers (the Colne and its Roman River confluent) in the valleys that both (with their feeders) have cut steeply down through the plateau's deep gravel to the underlying London Clay. Thus the plateau, with access to the water along its edges, north and east and south, and with an open heathy surface (light woodland only where the gravel has spreads of loam), is ideal for human habitation — ideal too, in Late Iron Age terms, for defence on the west, facing inland. And in the opposite direction, south-eastwards, where the Colne becomes tidal, it soon passes on, with the Roman River joining it, into the estuary that leads it to the sea.

#### Iron Age background

In *Camulodunum*, at the foot of page 4, introducing the excavations at Sheepen, it was stated that the early pottery (occasionally also implements of flint and bronze) that they came upon, widely scattered over the top of Sheepen Hill, was 'assignable to the Bronze Age-Iron Age transition about the 5th century BC. Ever since the 1950s, that transition has been rightly dated to several centuries earlier, and that pottery has now been recognised as earlier still. Mr John Barrett has firmly assigned the latter to his class of Late Bronze Age coarse-ware, prevalent in SE Britain from around 1000 BC through the next two centuries, and setting a tradition for the further 200 years, within which came the authentic transition to the Iron Age. In his work on the material from Orsett Camp in southern Essex (1978), Barrett cites his wider previous studies of the subject, and publication of that from Sheepen by him may be expected.

Secondly, the large ring-handled bronze cauldron (in the Colchester Museum) found 1932 in the same excavations, on Sheepen Hill's north brow, still close to the pottery-scatter and in a pit dug purposely to fit it, was on the same page in *Camulodunum* (p 4) said to

have contained in its filling (of sandy gravel) an 'iron nail'. I emptied the filling, and found the little object so described. Wholly of oxide, it disintegrated quickly after exposure; and lying in ferruginous gravel as it was, I have for some years now become sure it was a natural concretion. My 'iron nail' belief had sprung from misdating the cauldron too late. When the cauldron came to be published, with the others of its kind in 1957 (Hawkes & Smith 1957, 161-3 with pi 30), the rather earlier dating then proposed still let me believe in the 'nail'. But it still was not early enough. For the cauldron today, and that from Feltwell Fen in Norfolk (Norwich Castle Museum) containing a bronze flesh-hook with the early single prong, must be seen, from that and from their handles and rims, to be the oldest of their kind yet known (Gerloff 1987). So the people of the Sheepen pottery, through its dating now no less early, will still have been the possessors of the Sheepen cauldron. Most probably they buried it — when (whatever the cause) they abandoned their site. It has nothing to do with the Iron Age. (See Gerloff 1987, 88-92, and 107-8 for the dates within 20 or 25 years before 1000.)

The Early and Middle phases of the Iron Age in Essex (respectively the 6th to the 4th and the 3rd to the later 1st centuries) were in 1980 surveyed by Paul Drury, himself their principal explorer (1970 and onwards). His distribution-map of settlements and hillforts (1980, 48, fig 18) shows them flanking the Thames, the Crouch, and the Chelmer and Blackwater, while the NE has only three settlements (all beyond the Colne, with none nearer to it than Ardleigh), while the Pitchbury site, on the west of Horkesley Heath, became a hillfort seemingly not before the late phase (p 151). Thus the natural advantages offered by the plateau, south between the Colne and the Roman River, were still apparently neglected in the Early and Middle phases. At the Ardleigh settlement, however, where the single round house was enclosed, after a time, within an oblong ditch (Erith & Holbert 1970; Drury 1980, 52), both the house and the ditch are of the Middle phase. Pottery had now become simpler than the better-shaped and finer forms of the Early phase (Drury 1980, with references, 52-3). (Pots of the Middle phase from the hillfort explored at Witham in 1934 still wait to be published.)

For this and especially for a settlement-plan, the excavations giving the nearest approach to a type-site are Drury's, at Little Waltham, close to Chelmsford (Drury 1978a, 43-76 & 1978b). His summary in Drury 1980 (pp 50-52 with plan fig 20) of the two Middle Iron Age periods, II and III, shows how the two periods' houses could differ in construction, and how only the area inhabited in III was enclosed within a ditch — as in NE Essex at Mistley (known from aerial photography)

and like the single-house area at Ardleigh. The beginning of Little Waltham's Period III has been put about 100 BC. Little Waltham's Period IV, though represented more scantily, is of interest to what now becomes our principal subject, the Late phase, through having pottery distinctive of that, giving one of the still rare cases of a site with continuity from Middle to Late. For western Essex and for southern (where Mucking has much material still to be published), for the Essex coastal salt-works sites (some earlier than the well-known Late ones), and for any further Early or Middle-phase matters, Drury 1980 must here suffice.

## The Late Iron Age

### Period and culture; the Pitchbury hillfort; the Lexden Tumulus and the Lexden cemetery

The cultural progression, Middle Iron Age to Late, is in the NE quarter of Essex seldom explicit on any one site. In the Chelmsford district, its beginning has been perceived at Little Waltham (see above); but sites nearer to Colchester that represent the one, most notably that at Ardleigh, show no direct continuity with the other — or show it seldom. The basic Late Iron Age pottery forms usually first appear on new sites. Already quite frequently wheel-made, although less often in fine ware than coarse, they are in both fabrics tempered with the ground ceramic particles known as 'grog'. Isobel Thompson (1982) has accordingly used 'grog-tempered', a strictly descriptive term, instead of the ethnical 'Belgic' (from Hawkes & Dunning 1930) which connoted the belief — in a modified form still tenable — that intruders from Belgic (north-eastern) Gaul had introduced this pottery. Latterly, following on two more general articles (Hawkes 1972 & 1973), I have twice offered outline sketches of what that modified form should be: in 1977 (Hawkes 1980b) at the Essex Archaeology Conference in Clacton, and in 1982 (Hawkes 1983) in the Essex Archaeological Society's MR Hull memorial volume.

The replacement of the dagger by the sword as the standard personal weapon of the Continent's 'La Tene II' type is attested in the earlier 2nd century BC, when from the lower Thames and Lea there are some with the Continent's iron scabbard. When adopted by British smiths their scabbards are bronze, just as most of the dagger-sheaths had been. The breaking of the tradition of the dagger was made more likely by the introduction, at a date soon ensuing, of Britain's first coinage. In gold and known as Gallo-Belgic A, it was shown to have come from the Ambiani (Scheers 1969 & 1977), the Belgic people in the region of Amiens. What is implied is no wholesale invasion (as Allen 1961), but a relationship which, if they alone were supplying the gold, should imply an ascendancy won by political action. That force might still have played a part, as here suggested already from the swords, could be

supported by the story in Caesar's *Gallic War* (v. 12, 1-2), plainly borrowed from some earlier source, inevitably Greek. In a past that he nowhere dates, Caesar tells us that people came across from 'Belgium' into 'maritime' Britain, and that the intruders kept their tribal names nearly everywhere. Yet 'maritime' might mean anywhere or everywhere, from Wessex along and round to Essex. As for the names, since 'Belgae' in Caesar's Gaul had spread to become a supra-tribal name, its bringing into Wessex, still as tribal, may well have been early. Essex has no such aid for even a guess at when it was that the tribal name Trinovantes first came in. Its meaning, 'very vigorous' (Rivet & Smith 1979), is of no help.

Anyhow, on from within the later 2nd century BC, Essex has the Gallo-Belgic A gold coins, as has Kent, in what are regarded as the earlier of their sub-types, and more of the later, which are found spreading farther inland (Cunliffe 1981, 62-3). The political ascendancy apparently implied (and later perhaps merged in that ascribed to Diviciacus, king around Soissons (Caesar, *Bell Gall* ii. 4, 7)) tells us nothing of the internal implications of the coinage's adoption; anyhow it lasted long in circulation. Gallo-Belgic B's distribution is strongest on the Lower Thames and south of it. Gallo-Belgic C, strongest on the Medway in Kent, attests renewed Ambianic prestige, but exerts it, now farther on within the earlier 1st century, by influencing new gold coinages, wholly British. Essex has these only sparsely at first — so too Gallo-Belgic D, most often south-coastal — but its northern parts share with nearly all the SE (even places farther out to the NW) a flooding-in of Gallo-Belgic E: uniface gold staters, minted in Belgic Gaul for promoting its peoples' resistance against Caesar, from 58 BC through the next five years (Scheers 1972).

There were Belgae, already that year, taking refuge in Britain (Caesar, *Bell Gall*, ii. 14, 3). When he himself, in 55, made his first expedition there, he knew that Britons had been aiding the Gauls in almost all his campaigns (*Bell Gall*, iv. 20, 1). Moreover, Cassivellaunus, the Hertfordshire king who was to be head of the resistance to his second campaign, in 54, had lately assailed the Trinovantes and killed their own king (*Bell Gall*, v. 20, 1). Mandubracius, the dead king's son, having fled to join Caesar in Gaul, was restored by him to rule the Trinovantes later in 54 when Caesar reached their border. This act was done at the request of their ambassadors, who promised him the tribe's submission (*Bell Gall*, v. 20, 2-4). Others then submitted, and told him of the hillfort (likeliest Ravensburgh, some 4 miles (nearly 7 km) west of Hitchin), where were hidden, with their cattle, many of Cassivellaunus' men. His storming of this (*Bell Gall*, v. 21, 2-6), and the failure, in Kent, of an attempt to capture Caesar's base-camp, led Cassivellaunus to surrender. Caesar exacted hostages, imposed an annual tribute, and so departed, with Cassivellaunus forbidden to assail the Trinovantes ever again (*Bell Gall*, v. 22, with 23, 1-2). (On the whole year's doings in Britain, Hawkes 1978, 157-76; on the hillfort, 173-5 with 1980, *pace* Branigan 1985, 3).

Mandubracius and his people thus were left in a new situation: their protection by Roman ordinance started attracting Roman trade, bringing luxury imports — known from those that some ensuing rich burials have disclosed amongst their grave-goods. Yet the rite of cremation was here, a rite which was doubtless already coming in from Belgic Gaul where it had prevailed for some time past. And the pottery, though imports appear now and later in the richer graves, was from the start well-recognised Belgic Gaulish forms in a native fabric which was fine but tempered with 'grog', like the pottery from habitation-sites (see above). How early this domestic ware began to be adopted has been a matter for conflicting opinions. Rodwell (1976, 221-37), in his wide-ranging essay on the period altogether, extended Ann Birchall's recognition of an 'earliest' group (Birchall 1965, 248, 288) to a broader spread in Kent and broader still to the north of the Thames. A pre-Caesarian date for some of this material has to be very tentative for the forms in some places can still have been produced at later dates, as were certainly some at Colchester (eg Rodwell 19 = Cam 264B and Rodwell 36 = Cam 229B). On the other hand, adoption of the ware need not be down-dated by the rich post-Caesarian graves which have imports. For these graves, Stead 1967 has been augmented most notably by Stead & Rigby 1986 on that at Baldock. This group of graves dates to c 50-10 BC, which is Phase I of Stead's 'Welwyn-type' burials in NE Hertfordshire but not in Essex (Stead 1967, 51). And the Welwyn-type burials north of the Thames, put after c 50 BC by Stead, have now one at least no later than 50 BC, near Baldock in Hertfordshire (Stead & Rigby 1986). Thus it might be put just after Caesar's departure in autumn 54; yet the range allowed for comparing its contents does run farther back within the century's first half.

Welwyn-type burials are unknown in the rest of Hertfordshire, including Verulamium. Roman goods would be kept away because it was the county of Cassivellaunus. His submission to Caesar and the consequent promise of a tribute must have left him with a bitter grudge against his Roman-favoured neighbours which, as we shall soon be seeing, was passed on to his successor. Then who were those in the area of Welwyn-type burials? Clive Partridge in his monograph on the important settlement not very far beyond them, at Skeleton Green (close to Braughing), has suggested that they were the people who in 54 had submitted to Caesar and told him of the hillfort, which was full of Cassivellaunus' men and which he consequently captured (Partridge 1981, 353-4, with Roger Goodburn on p 127). This would earn them a sequel of Roman commercial favour. Yet an alternative suggestion would be that the Trinovantes, who alone have Caesar's own record of his favour and protection, took advantage by stretching their domain across the Lea as far as Welwyn — perhaps with a 'client-tribe' status for the folk there, who might have included some of those claimed by Partridge. In any case, the presence there of Roman goods, attested by the burials, must show them brought

in by the mouth of the Thames and up the Lea. Why the earliest dates for such goods across Essex to the east seem not before 25 BC, nor are firm before c 15 BC, may or may not be a matter of chance; but when they do appear (see p 88 on the Lexden Tumulus), they certainly were landed on either the coast or the lowermost Colne. The two contrasting distributions primarily start from two different sea-routes — political contexts being nonetheless justly inferred. However, in the later Iron Age, the agricultural landscape was altogether more basic to Essex than these two distributions, just as it was in earlier Iron Age times.

Close study of much of the ground, led most of all by Drury and Rodwell (1980), with use of modern and of earlier maps, has yielded systems of boundaries and fields, often proved, through priority to Roman roads or other features, to have been pre-Roman, at any rate in primary layout.<sup>1</sup> Their article demonstrates the long persistence of the Iron Age systems. That this is in no real conflict with what was observed here above, a break between sites of Late Iron Age settlement and Middle, may anywhere be because both would have used the same fields; and we still know only a very few sites of either. Yet there need be no denying some amount of immigration to help explain the undeniable changes, foreign-inspired, in both pottery and cremation as the grave-rite. Refugees from Caesar's wars in Belgic Gaul, and all who throughout them were bringing Gallo-Belgic E coins, can only have strengthened what Caesar found: the Trinovantes, amongst the regions' tribal states, were *prope firmissima* (*Bell Gall*, v. 20,1), 'the firmest' meaning 'the most strong-and-solid'. Thus the existing people's receptiveness towards the influences from Belgic Gaul and its migrants, who were arriving during the period leading up to Caesar and a little later, led to solidarity and increased strength.

In the Colchester area, the matters that remain to be touched on here are more particular. The first is that of its only recognisable hillfort: Pitchbury, called more traditionally Pitchbury Rings. The two sets of cuttings through its defences, in 1933 and 1973 (pp 138-154), showed traces of some earlier resorts to the place. The evidence was scanty for all periods, including the Late Iron Age, which was the latest. But as one of few Late Iron Age pottery fragments found on the site (in 1933) came from the inner ditch's primary silt, the sherd must give a date for this work — which was the main defensive ditch, reinforced by the outer one only partly. Yet plainly there was never any regular occupation: rather, the purpose will have simply been occasional refuge. The Camulodunum defences were permanent dykes; once the protection they would give became secure, it would supersede any such purpose. The Pitchbury works should thus have been prior to the early 1st century AD, and so be dated presumably late within the century before.

What needs to be touched on next, very different, are

1 The postulated early origin of these systems has been questioned by Stephen Rippon (1992).

graves for the cremated dead. They are described and fully treated in our two chapters on the Lexden Tumulus (pp 85-94) and the Lexden cemetery of 'flat' graves (pp 164-9), so-called because no surface-marks remain. The tumulus, standing to the south of the graves, has had its excavation, done in 1924, re-assessed by Dr Jennifer Foster (1986). It had far the most richly furnished of all the Late Iron Age burials in Britain, and with her acute interpretation of the findings, Jennifer Foster has descriptions of all the material, both native and introduced from the Roman Empire — a work of archaeology that leads her to a date for it about, or in the next years after, 15 BC. Summarising first her results, I have gone on further, into an essay on their placing in the period's history (pp 88-94). It resumes the brief account given here above, of what history and SE British archaeology can say of the hundred years or so before about 25 BC, by using the modern studies of British coins bearing names of kings, and/or of mints, and further historical texts, with aspects again of archaeology, down to c AD 10. Nothing more of all that need thus be anticipated here; but the events inferred will bear, not only on the Trinovantes altogether and at Camulodunum, but also on the people whose centre lay westward in Hertfordshire. For the grudge held against them, first by Cassivellaunus, was to issue, after him, in actions bringing them changes of ruler and increasing prosperity.

### **The name 'Camulodunum'**

This is simply the Latin for a Celtic *Camulodunon*. Its first element *Camulos* was the name of a Celtic god, brought into Britain at whatever time, certainly from Gaul. The Romans equated him with Mars, a deity with several aspects but pre-eminently god of war, so that *Camulos* (also an element in several Celtic personal names) will himself, in Celtic religion, have been a war-god. The name's own meaning (whatever its root-etymology) must thus have been appropriate to that; in the latest study of Celtic gods, it is 'powerful' (Green 1986, 110-11). (For some older modern literature, see Rivet & Smith 1979, 294-5, with the texts and Roman inscriptions (and inscribed British coins) amongst which is found the authentic spelling of the name: *Camulodunum*.)

Its second element, *-dunum*, Latin out of Celtic *dunos*, *dunon*, is widespread in Britain as in Gaul, sparser only farther east, and in Celtic Spain, where the commoner equivalent is *-briga*, 'hill' — the distinctive sense of *-dun* being 'fortified place', appropriate therefore to sites protected by earthworks (Rivet & Smith 1979, 274-5 & 277-8). Thus our *-dun* name for Iron Age Colchester, or even for any one part of it, should have been first bestowed when such earthworks started to be built. Only one pre-Roman major dyke stands apart from the system of the others; and, at the only accessible place where any one of them crosses its line, has been proved by excavation to have been cut earlier. This is Heath Farm Dyke (Fig 2.1), at 'Prettygate Junction' (pp 48-50) where it is crossed by Lexden Dyke Middle, with the rampart of the latter overlying its filled-in ditch.

At its start, ie as Heath Farm Dyke South some 2.5 km farther SW, it curves to protect the western edge of the Gosbecks site (p 29), for which it thus helps to declare a pre-Roman occupation. The dyke starting just in front of it, curving also around that edge, our Kidman's Dyke South, bends NE, but reaches an ending (as Kidman's Dyke North) that will be found to prove it significantly later. The minor dyke that starts in front of that as Gosbecks Dyke South, and thence runs on nearly straight (becoming Gosbecks Dyke North), has its dating in relation to the rest still unknown. Yet Heath Farm Dyke, on from a time prior at least to the rest of the major dykes, was protecting Gosbecks along with an open expanse so very much larger that its building might well be thought the earliest occasion fit for bestowing a *-dun* name, and a step towards extending this 'Camulodunum' over all the eventual dyke-protected area.

That is a guess, but there is still a date which might be admitted to bear on it: that of the name's very first occurrences, inscribed on coins as a mint-name, coins not yet of the well-known Cunobelinus, but of the king he was to name as his father, Tasciovanus, and amongst the types agreed to be early in the reign. On anyhow two of these types it is combined with his own name: the gold one (stater) numbered 186 by Mack (1953; pp 92-3), and the bronze one recognised first from a find at Great Canfield in Essex (Eddy & Davies 1982). All are far rarer than those from his capital mint, Verulamium, yet they attest his power here also, for a very few years, about 10 BC. There is still no positive evidence of any location for his centre here; nor of any pre-Roman cult on the site of the Roman temple at Gosbecks. Only these coins imply, already so early, some place protected by earthwork, with the patronage of *Camulos* the war-god, through a name soon extended throughout the area.

### **British coins**

The *Camulodunum* coins of Tasciovanus, just noticed as the earliest to carry this mint-name, have their date about 10 BC for amply-recommended reasons. During a time that can only be prior to that, without any mint-name but occurring hereabouts, though with a likely slight priority farther inland (and an added distribution, seen as later, up the Thames), coins were struck in the name of another king, Addedomaros; he is the first to be connectable thus with anything in Colchester archaeology, and with the historical people in Essex, the Trinovantes. After Tasciovanus comes (whether or not first briefly before him) a Dubnovellaunus. After them both, and after a likely but quite short interval, the mint-name of *Camulodunum* reappears: on coins in the lengthy series struck by the great king Cunobelinus, whose name, along with the place-name, enters recorded history too, and is familiar more in its English form, Cunobelin. While all the three will have various mentions below, the sequence of coins before him is treated more fully at the end of my



essay on the Lexden Tumulus (pp 90-94). Of Cunobelin's coins, gold, silver and bronze (Mack 1953, nos 201-260), the gold are most thoroughly presented, being serially classified by the late D F Allen (1975).

Since this section was first written, Dr Colin Haselgrove has published his work on Iron Age coinage in SE England (Haselgrove 1987a). He uses all the provenanced coins, some 15,000 of them, newly classified into nearly 700 types, and presents them in their settings in modern archaeology. Only for the Colchester Sheepen site does this need to be considered here in detail (pp 77-84). But it everywhere surpasses previous works on the subject in fullness: its Part 1 has ten main chapters, Part 2 six appendices and a long bibliography: 525 pages altogether. His system stands in the line of Allen's and Mack's, but of course it transcends them. Thus our previous paragraph here, referring to both, needs no withdrawing, though it might have been expanded; and our various mentions of coins and their dates seem nowhere discrepant with his findings.

Not to be forgotten, however, remains the fullest memorial to Allen: the edited and up-dated presentation of his 24-year-old Rhind Lectures, *The coins of the ancient Celts*, by Dr Daphne Nash (1980); also her own European survey, historical, social and political: *Coinage in the Celtic World* (1987).

Lastly, the longest-ever book on the British coins has appeared — R D Van Arsdell's *Celtic coinage of Britain*. This is a fully-illustrated catalogue, in which is classified nearly 800 types, with 54 plates repeating all its illustrations and 80 maps, devised in a quite new manner. He deploys the coins in sequences of date, obtained from declining weight and purity but measured with exceedingly high precision, and declares the resulting chronologies to have validity surpassing every other consideration, any conflicting historical data included. Among his various interpretations of coin-distributions, that of the pre-Cunobelin relations of Trinovantes and Catuvellauni, not as hostile but as intimately friendly, defies the considered opinion of both numismatists and historians, especially those who resort to C E Stevens (1951, 336-42) for the period. Thus our treatment of the history and coin-datings here will remain unaltered by Van Arsdell's; Colin Haselgrove's review of Van Arsdell's book (Haselgrove 1990) should be trusted to reflect majority specialist opinion that this hugely ambitious venture falls short of success.

### Points in chronology

Dubnovellaunus, shown by the coins noticed above to have preceded Cunobelin in Essex, directly or nearly so, may claim a closer chronological value.<sup>2</sup> The inscription reproducing Augustus' own long record as Roman emperor — the *Monumentum Ancyranum*

(Brunt & Moore 1987) — records that two kings fled to him out of Britain, Dubnovellaunus being one (the other being Tin(commius) from the southern British kingdom), in a year not after AD 7. Usually given as the actual date, it might allow of raising by a little, so be guessable as AD 6 (pers comm Dr Barbara Levick, St Hilda's College, Oxford), and would lead to Cunobelin making Sheepen his capital c AD 10. Earlier dates for the beginning of the Sheepen site, certainly like Hull's final choice, c 5 BC, fit only a part of the evidence. Sheepen would not have been Dubnovellaunus' capital because none of his coins have a Camulodunum mint-mark, and the few from the site, which are all bronze and ill-preserved, can have been dropped at any time later within the occupation.

Cunobelin reigned from c AD 5 or 10 until his death. This is recorded as being shortly before AD 43, which is the historical date of the Roman invasion under Claudius which reached Camulodunum within that year. Near the end of Cunobelin's life, a son of his, named Adminius, had fled in exile to the previous emperor, Gaius (Caligula), who reigned from 37 to 41. Events at Colchester in 43 and on, through the years of the place's legionary fortress and its replacement in 49 by the first *colonia*, are treated on pp 55-69. The revolt led by Boudica, queen among the Iceni, the next people to the north of the Trinovantes, who immediately joined it themselves, broke out in 61, according to Tacitus; that the date should be 60, as recent writers have mostly held, we now believe less likely (see p 70). The destruction it brought to the *colonia*, which was thereupon rebuilt, fell also on the primarily native site at Sheepen, a little farther west. But our discussion of Sheepen and its dyke is most concerned with dates for its beginning (pp 77-84). For the general occupation and the dyke, the date c AD 10 still appears, as in *Camulodunum*, to be allowably the best. Cunobelin's accession might perhaps, as seen just above, have slightly preceded his making his capital here; but anyhow a partial occupation will have begun about 5 BC on Sheepen Hill. And down by the Colne for a time, from before 15 BC, if visiting ships started landing wine-amphoras so early, there would always be people ready to take them inland. Such amphoras were discovered in the Lexden Tumulus (p 88). The relative chronology of all dykes as presented in successive phases in the Sheepen dyke is fully considered on pages 78-83 and summarised on page 83.

### The Gosbecks site: an introduction

This site, named Gosbecks after the farm that owns the fields in which it lies (and includes the old big 'Cheshunt Field'), has its centre slightly more than 2.6 miles (4 km) SW of the nearer edge of Colchester's old town. South

2 CFCH did not consider anywhere in this book whether there were might have been two rulers called Dubnovellaunus (Rodwell 1976, 261-4) although he does refer to the Kent and Essex series of coins (p 94).

from it, the ground slopes down to the Roman River, to which a small stream, from its northern part, runs beyond it to the SSW. Roman stone buildings in it, confirmed by excavation, were a theatre and a temple of Romano-Celtic plan, built in the corner of a square stone-built triple-walled *temenos*, having an earlier enclosure-ditch of similar plan within it and containing, in its primary silt, a well-preserved coin of Cunobelin and part of a contemporary pottery vessel (Rom Co/, 260-70). The extent of the site as a whole is however far larger and the plan of all its features known at present has been revealed by aerial photography, notably by Dr St Joseph (*Rom Col*, 261) and by the Royal Commission on the Historical Monuments of England. The full account of Gosbecks (pp 95-105) shows the major importance of the ditched quadrilateral enclosure, directly SW of the temple's. This 'farmstead', expected to be early as a principal pre-Roman residence, awaits a thorough excavation.

## The dykes

### Names for the dykes

[Fig 2.1]

These are set out on the key map, Figure 2.1. Those already used in the *Camulodunum* (pp 8-16, and pi 1) are Sheepen Dyke, Lexden Dyke, Moat Farm Dyke, Berechurch Dyke, Gryme's Dyke, and Triple Dyke (then still supposed pre-Roman in origin, like the rest, but recognised now as an early Roman triplication of Shrub End Dyke North, the northern part of the dyke named here 'Shrub End' after the Colchester suburb of that name). In addition to the latter, the other new names used here are the following eight. 'Gosbecks Dyke' is explained by the position of both its stretches, the South and the North, facing west to protect the occupation-site now called Gosbecks which lies within the old property of Gosbecks Farm (in the part with the traditional field-name 'Cheshunt Field', still used on the Ordnance Survey maps of 1956 and 1980, but no longer in general use). Recognition of the Gosbecks site as a whole, and of both the parts of its dyke, has been due to aerial photography. Protecting it also on the west, but inside of Gosbecks Dyke and running on north-eastwards far beyond it, are Heath Farm Dyke and Kidman's Dyke. 'Heath Farm Dyke' is so-called from its running under the buildings of the old Heath Farm; but its line as now explored is both very much longer, and important as proving it earlier than Lexden Dyke (pp 48-50). 'Kidman's Dyke', which at Gosbecks runs directly in front of Heath Farm Dyke and beyond extends so far that it ends by abutting on Shrub End Dyke, was so-called (first by the late A F Hall) to commemorate the Mr Kidman who gave permission for

its being explored where it crossed his land. 'Prettygate Dyke' — with subsequent early Roman modification — running in from Shrub End Dyke to meet Lexden Dyke South at the southern end of Bluebottle Grove, has its name from the former Prettygate Farm, into a field of which it ran (the name itself was taken from the farm's old gate, adorned in its early 19th-century ironwork with large-size models of the period's farming implements). Of the dykes that run to the south of Gosbecks/Oliver's Dyke' has been named from Oliver's Thicks, the wood through which it passes, and 'Layer Dyke' from the parish of Layer-de-la-Haye, where it ends near the vicarage. The dyke parallel with Gryme's Dyke Middle and behind it is here named 'Dugard Dyke' from Dugard Avenue (the former Pear Tree Lane) which cuts across it. Lastly, continuing the line of Berechurch Dyke, and like it in facing east, but running south from the Roman River to end at the churchyard of Abberton village, is thence named 'Abberton Dyke'.

### The dykes in topography

Into the area's natural setting as above portrayed, the dykes inject a pattern that declares their builders' defensive purpose. Where embodied in the design of their outermost lines, Gryme's Dyke facing the inland west, Berechurch and Abberton east towards the sea, has to be seen as the culmination of a sequence of phases. What appears most crucial for elucidating this is the position of the Gosbecks site. Far down in the SW quarter of the whole expanse, Gosbecks lies quite close above the valley of the Roman River, into which flows the little stream that had its source within the site and gave it water. The Sheepen site, on the other hand, has its northern edge watered by the Colne, and is defended on the west and south by Sheepen Dyke, which is the innermost earthwork of a system of three, all alike with the Colne along the north of them. And this topographical contrast allows, with support from some excavated evidence, the inference that Gosbecks was the site established first, and the Colne-based system only later. While the Gosbecks site's maze of internal division-lines, and the large four-sided 'farmstead' enclosure in its middle, can only be interpreted fully from excavations, yet the fact of its possessing what have already been explored — a Romano-Celtic temple and a Roman theatre — should mean eventual Roman recognition of its status as an older Trinovantian centre. Also the presence at Gosbecks of a Roman fort (this too known only from aerial photography) must imply that at the conquest the area was seen to require supervision, by an auxiliary unit in garrison — which confirms its importance as a centre already established. Moreover the system of Colne-based dykes, as is shown below on pp 52-55, left it excluded (till a subsequent phase)<sup>3</sup>; and since that system's author was undoubtedly Cunobelin, who belonged to the

3 The apparent exclusion of Gosbecks from a Sheepen-centred system of defences is perhaps not as clear cut as it might appear (pp 170-71).

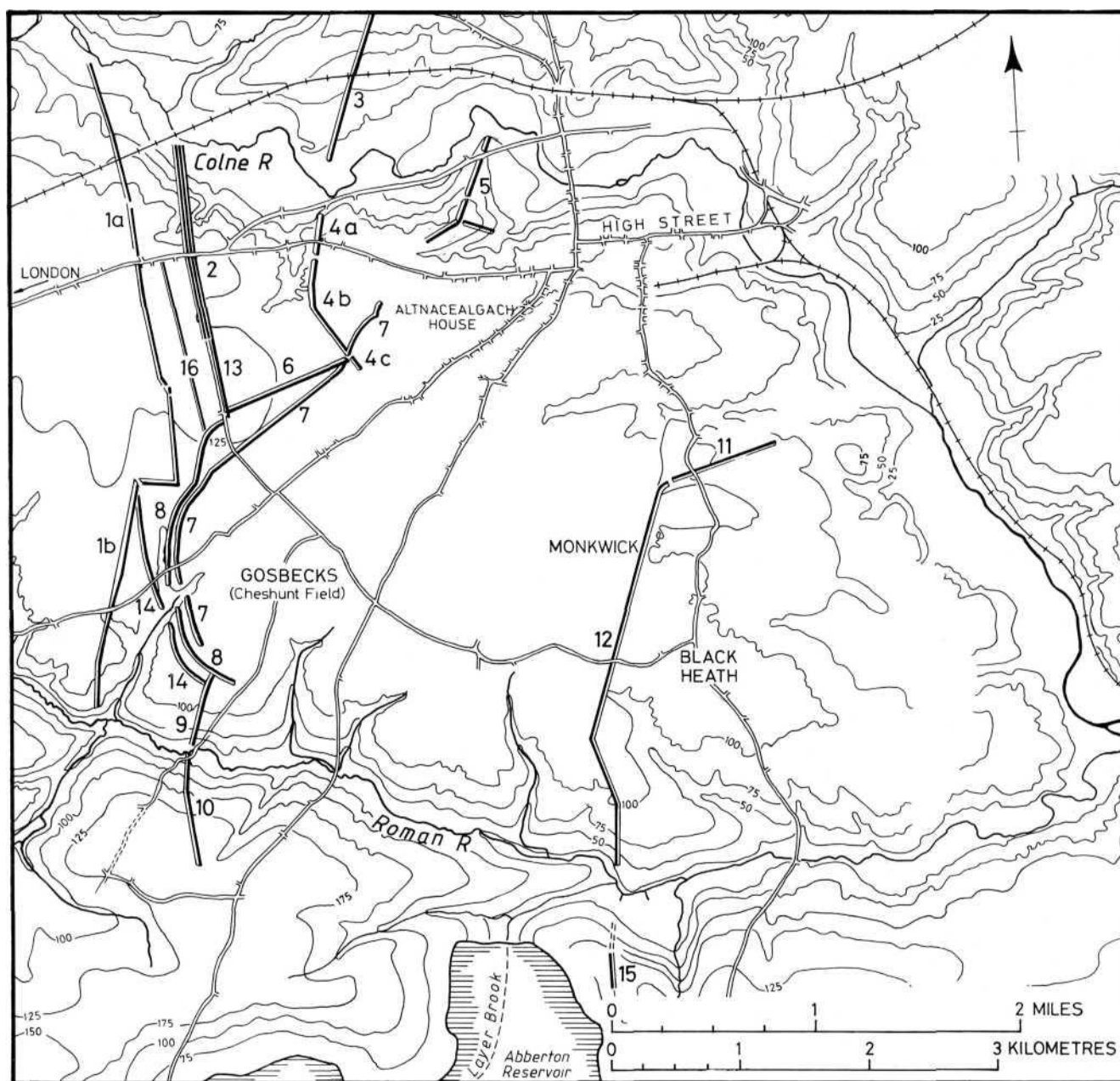


Fig 2.1 The dyke system.

- |                          |                          |                               |                     |
|--------------------------|--------------------------|-------------------------------|---------------------|
| 1a...Gryme's Dyke North; | 4a...Lexden Dyke North;  | 8...Kidman's Dyke;            | 14...Gosbecks Dyke; |
| 1b...Gryme's Dyke South; | 4b...Lexden Dyke Middle; | 9...Oliver's Dyke;            | 15...Abborton Dyke; |
| 2...Triple Dyke          | 4c...Lexden Dyke South;  | 10...Layer(-de-la-Haye) Dyke; | 16...Dugard Dyke.   |
| (Shrub End Dyke North    | 5...Sheepen Dyke;        | 11...Barnhall Dyke;           |                     |
| in early Roman Phase 1); | 6...Prettygate Dyke;     | 12...Berechurch Dyke;         |                     |
| 3...Moat Farm Dyke;      | 7...Heath Farm Dyke;     | 13...Shrub End Dyke;          |                     |

Catuvellauni and so was here an intruder, its contrast with Gosbecks in topography is matchable in history.

### The dykes in relation to movement

Human activity in the dyke-defended areas of course included movement, to and fro across them. To control the crossings, the transverse dykes had entrances —

though the very short Dugard Dyke need not have required one. So the entrances have to be seen as on the lines of trackways, undetectable over the spaces from one to the next, but none the less real because of that. Some would be minor tracks only, but others would be major ones, especially when leading to particular fords across the rivers, and likewise when passing west to east, either to reach such a landing-place on the tidal Colne at Sheepen or at the Hythe, or to reach one right on the sea, as at Fingringhoe Wick. Reaching the sea

at one of these must indeed have been aimed at from far inland, just as merchandise landed from the sea or the tidal Colne can have been carried far inland from east to west. Major trackways can thus have served long-range trade-routes — usable naturally also for movement of migrants or of forces under arms. Our dykes will have therefore served not only for defence of settled areas behind them, but also, by keeping passage through them confined to their entrance-gateways, for control of long-distance and local traffic.

### **The early surveys, 1722-1922**

[Figs 2.2-2.10]

My compiling this section and the next has been aided, often greatly, by use of a typescript study by M R Hull, completed at an unknown time before his death in 1976, and evidently meant for publication. I have checked it and variously shortened or expanded it, but gladly acknowledge my debt to it here.

The brief account in *Camulodunum* of the early surveyors of the dykes gave references for most, and began by noting the 'Gryme' of Gryme's Dyke as an ancient English name for the Devil, thus imagined as its supernatural builder, and also the medieval and later use of it (or of parts under various names) for the marking of legal bounds, notably (as ever since then) the western limit of the Liberty of Colchester (*Camulodunum*, 8-9, n 4; J B P Karslake 1935; Reaney 1935).

Objective field surveys began in 1722 — so just 200 years before the publication in 1922 by the Royal Commission on Historical Monuments England of its volume for NE Essex. The 1722 survey was taken on 12 July that year, by the Reverend Thomas Lufkin and Payler Smith Esq; their description and measurements were transcribed from the original by Philip Morant, and published by him in volume 2 of his *History of Colchester* (1748) on pages 24-5, as 'Note Q'. Ten years later (25 August 1758), Morant made two sketch-plans (Figs 2.2-2.3)<sup>4</sup>, which were subsequently preserved on page 135 of the Album (now in the Colchester branch of the ERO) of the Victorian archaeologist William Wire (on whose activities see further p 22). Finally there is the manuscript map (Fig 2.4), illustrating Lufkin and Smith with their numbering, taken from Morant's own notes in his copy of the *History*, which Dr Henry Laver published with the Lufkin and Smith survey again transcribed (Laver 1908, 19-20).

Between our Shrub End and Gryme's Dykes (Fig 2.4, nos 12-11-10), the line running north from no 11 is our

Dugard Dyke, while the curve, continued as a straight line south from no 10, is clearly our Kidman's Dyke. The line 10-9, however, seems to end in confusion with the triangle on Gryme's Dyke at Stanway Green (cf Fig 2.4 and Morant's similar lines in Fig 2.2). And for those on Figure 2.2, outside Gryme's Dyke and crossing one another, it is hard to suggest any explanation at all.

Then in 1759 came a visit to Colchester by William Stukeley, the antiquary for long by then a figure of repute for the fieldwork and other doings made known through his early publications, but who a dozen years before — already for thirty years past in holy orders after practising medicine — had settled as a clergyman in London. But his extant letters to Morant, though in 1727 and 1762 about coins, show him in 1758 concerned with the dykes; and his letter on them, 15 August, was immediately followed, on 25 August, by Morant's making the sketch-plans here just noticed. Thus Morant will have certainly welcomed, and had probably invited, the visit that Stukeley made in the following August — when Stukeley was a man of 72. Professor Stuart Piggott's comprehensive *William Stukeley* refers to the letters (British Library Add Mss 37222, f 134; Piggott 1950, 175, 198; Piggott 1985, 183 note 401), and (with Piggott 1985, 146-7) to the record in Stukeley's journal (Bodleian, ref Gough Maps 7) of his having 'survey'd the wonderful works of Cunobeline' at Colchester, on 13 August 1759. That these were our Lexden and Gryme's Dykes, with the Triple Dyke between and other features in their area, is declared by the set of five engravings from vanished autographed drawings by Stukeley, all but the last being signed by the engraver, P Benazech. A set of proofs of these drawings (Figs 2.5-2.7) is preserved in Chelmsford District Library (OS1913), where they were found in the 1930s by the E J Rudsdale, in the course of his service as assistant in Colchester Museum. All but the last bear the date of their originals, August 1759; and the '15 Aug.' on the first and fourth shows that Stukeley drew these out on the second day after making his survey; the last, which is a map of the district, would have taken him longer. That order for them, Piggott's in his 1950, pages 198-9, has here been altered, to make the map (his no 5) the first; the second (his no 3), view of the great 'King Coel's Kitchen' hollow outside (and of some date later than) Gryme's Dyke; the third (his no 1) 'perspective view' from the east; the fourth (his no 2) that from the south, and the fifth (his no 4) diagram-section through the Triple Dyke. Found later, and not in Piggott's list, is a perspective sketch of the dykes, from the west, drawing and script alike in ink and in hastiness of style (Fig 2.8): unsigned but undoubtedly by Stukeley himself, made on the same occasion, 13 August 1759, but apparently never sent to be engraved.<sup>5</sup>

That this was a period when engravers might take

4 Unknown to CFCH, Figures 2.2, 2.3 and 2.8 are on the same folio and share it with what appears to be a preliminary version of Figure 2.7. It is hard to reconcile this with the belief that Figure 2.8 was by Stukeley whereas Figures 2.2 and 2.3 were by Morant. The note at the foot of Figure 2.8, "Two sketches of Dr Stukeley's", refers to Figure 2.8 and the preliminary version of Figure 2.7 mentioned above, and would appear to rule out Stukeley as their author. The origin of the four drawings needs further investigation. They may be copies from originals of Morant (hence Figs 2.2 and 2.3) and Stukeley (hence Fig 2.8) by another party (but not Wire). Figure 2.3 may even be a derivative of Figure 2.4 by the unknown party. (I am grateful to Paul Coverley and J Bedford for their advice on the handwriting.)

5 See footnote 4 above.

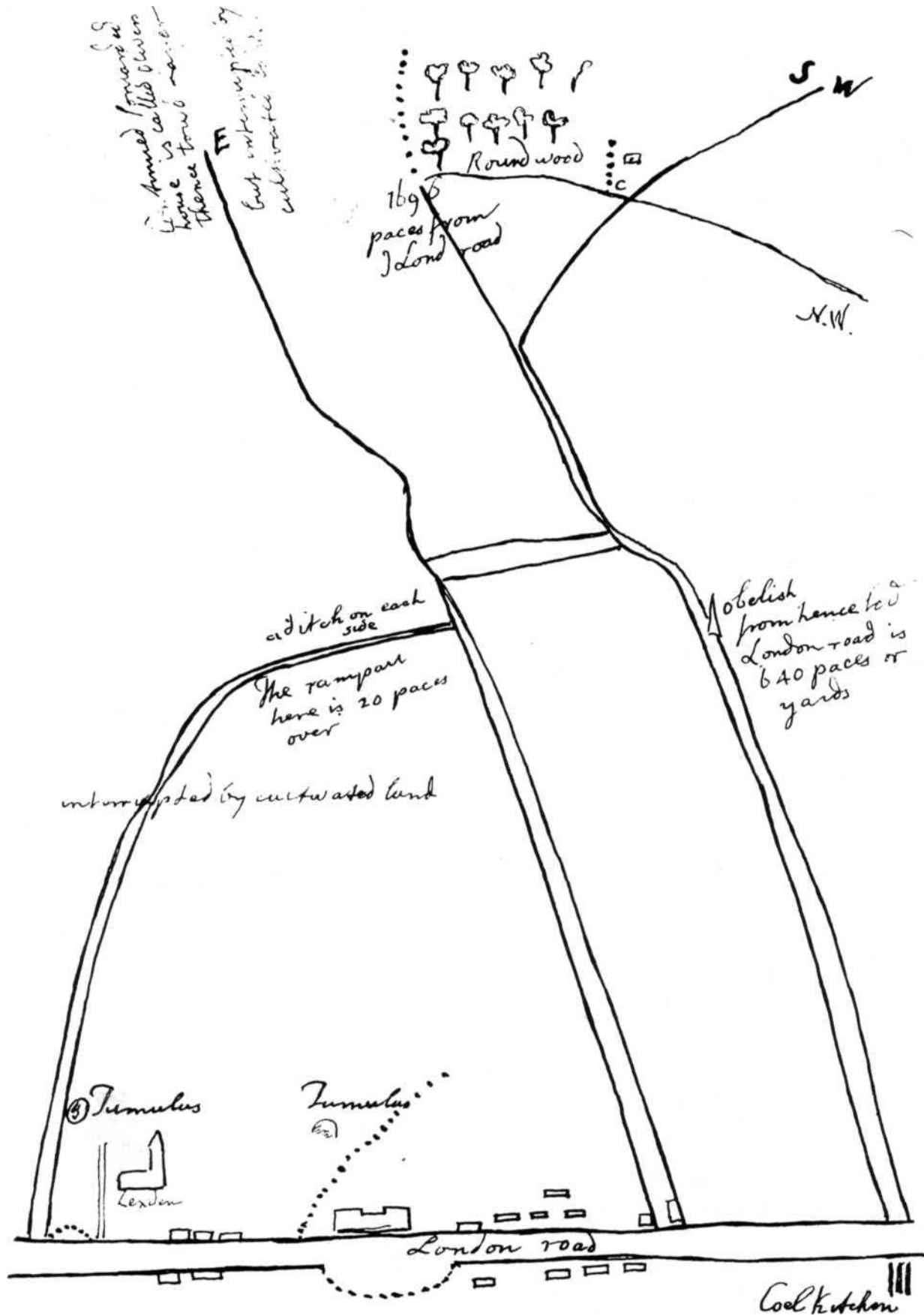


Fig 2.2 A map by Philip Morant of the earthworks on Lexden Heath. See footnote 4 on page 10 about the authorship. Reproduced by courtesy of the Essex Record Office. [Page 10]

M<sup>r</sup> Morant's Draught Aug. 25. 1758. partly from J<sup>r</sup> Mason's paper, partly from his own observations.

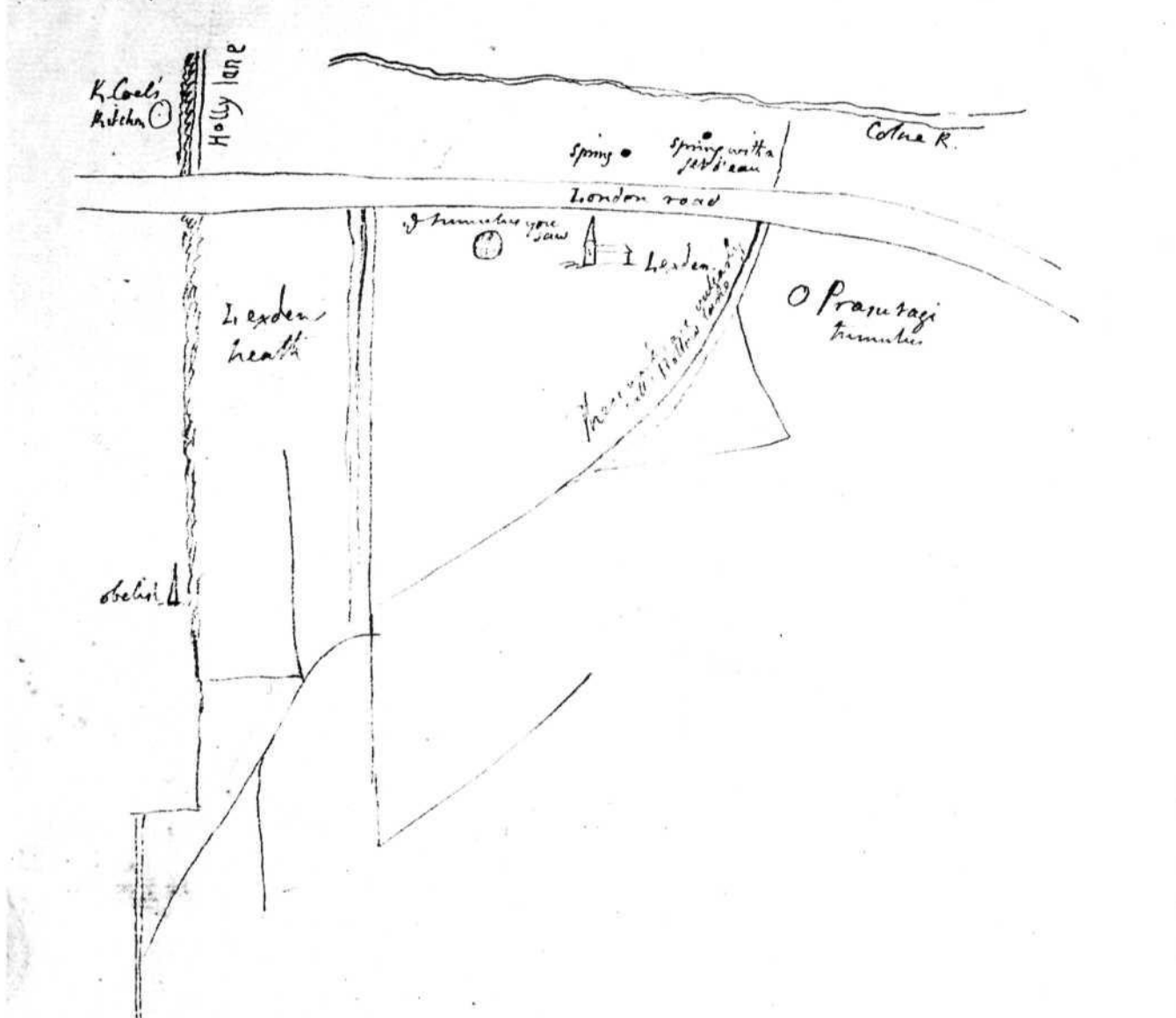


Fig 2.3 A map by Philip Morant of the earthworks on Lexden Heath. See footnote 4 on page 10 about the authorship. Reproduced by courtesy of the Essex Record Office. [Page 10]

liberties with their originals, for beautification or just for convenience, one is anyhow reminded by comparing Figure 2.8 with the rest; also Stukeley himself committed errors or wilful distortions, especially later in life when these could be affected by his stranger religious ideas. But as Piggott points out (1950, 175; 1985, 146-7), there can here have been nothing of the kind, so that we can value the Colchester 'perspectives' as mostly representing the works as he saw them, even allowing for beautification by the engraver, which is very clear in Figure 2.6a, simplification which is clear in Figure 2.5 and the map Figure 2.7; and in their making the Colne artificially straight (as in Fig 2.3 too).

With the error on the engravings Figures 2.5 and 2.7, making Gryme's Dyke continue beyond the Colne — no doubt to match Moat Farm Dyke which is Lexden Dyke's

authentic extension — compare Stukeley's perspective sketch (Fig 2.5 below) with his hand-written note in Figure 2.8 besides Gryme's Dyke: 'This bank crosses the Colne northw'd and goes to a circular work on Horkesley Heath, a mile distant, on the left'. That 'work' is our Pitchbury hillfort, the 'British Oppidum' in Figure 2.7, and 'Oppidum Britannicum' on Figure 2.5. Its distance from the actual end of Gryme's Dyke (which is on the Colne at New Bridge), is a mile and a half (2.3 km), and the map Figure 2.7 shows it stopping nearly half-way to the 'Oppidum' from there. So possibly Stukeley meant goes towards it, and stops when 'a mile distant' from it. Anyhow Morant on Figure 2.2 shows nothing of the dyke beyond the Colne, and neither has its stretching there ever been claimed by others. And what the 'obelisk' was, marked in this and in Figure 2.3, standing beside Gryme's Dyke and on the lip of its ditch, who can say?

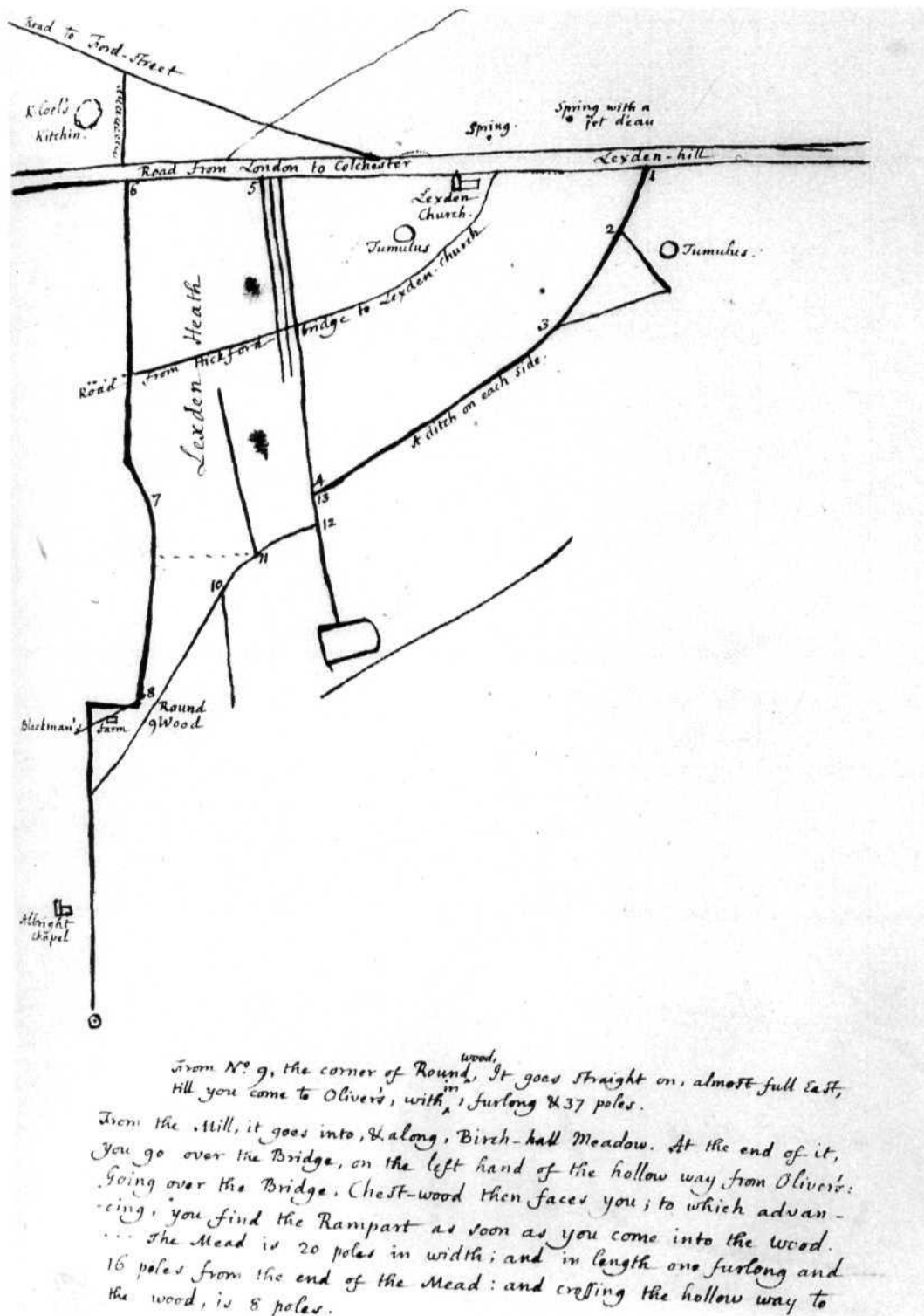


Fig 2.4 A plan by Morant showing his interpretation of Lufkin and Smith's survey of the earthworks on Lexden Heath. In one of Morant's copies of Morant 1745 in the Colchester Branch of the Essex Record Office (D/DQ 73). Reproduced courtesy of the Essex Record Office. [Page 10]

The idea that did infect his understanding of the dykes was that their central work, the Triple Dyke, was Cunobelin's regal racecourse — his 'Agon Regius' in Figure 2.5 — for the chariots that Figure 2.5b shows galloping at large, while the Kitchen hollow was the 'Amphitheater' (Fig 2.8), presumably for gladiator-shows. These notions are neither of them used in the notes in his sketch (Fig 2.8), so he can well have

conceived them after he had made it, when preparing to draw the originals of the engravings. Very notable in all that are the two single-line 'banks' running not north-south, but at a slant (made a curve in Fig 2.5) WSW from 'The Eastern Bank' (Lexden Dyke), and curving away SW from the Triple Dyke's starting-point. This second one is hard to grasp because its curve must be artificial. The first bank is also not easy to



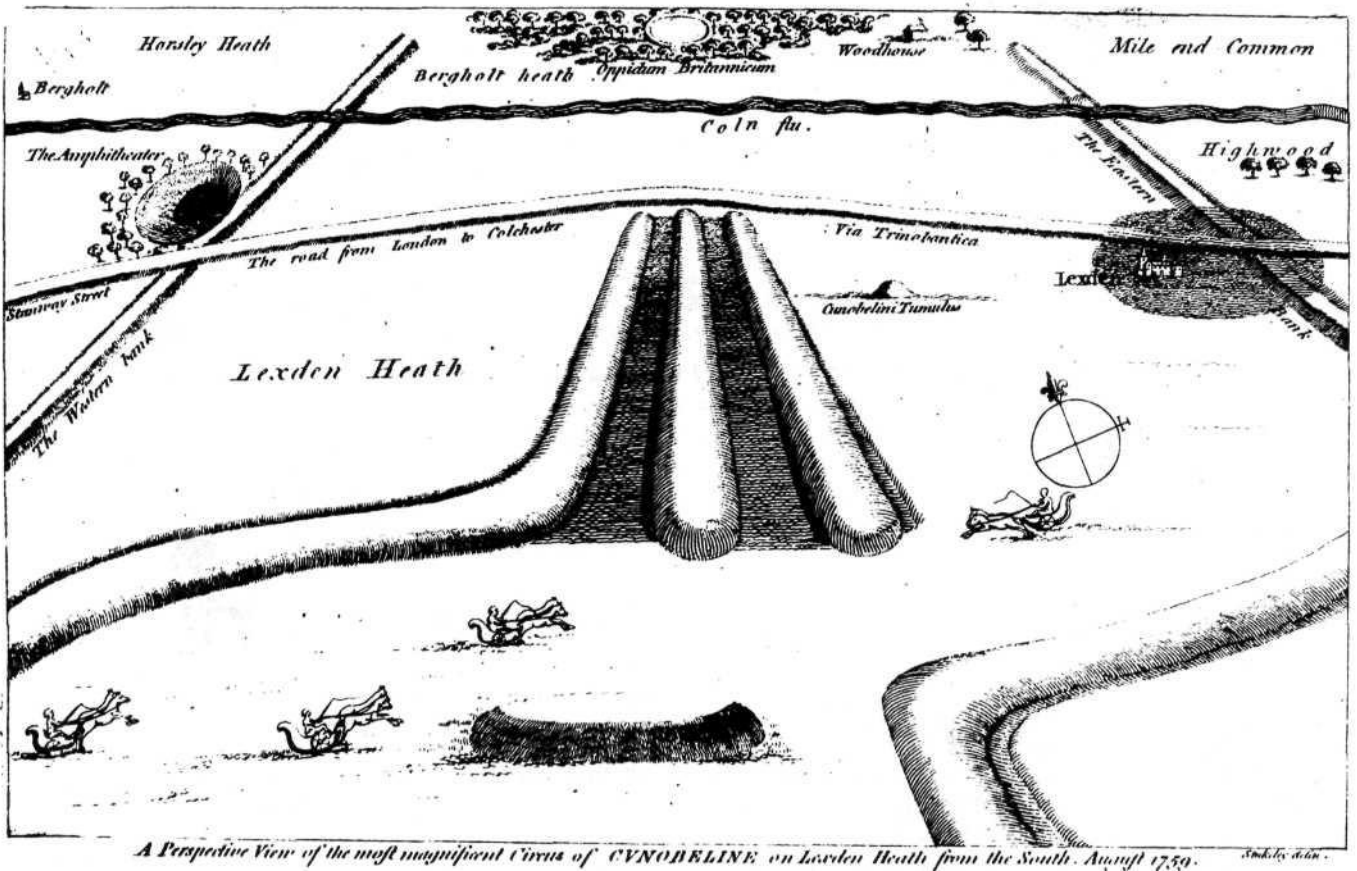
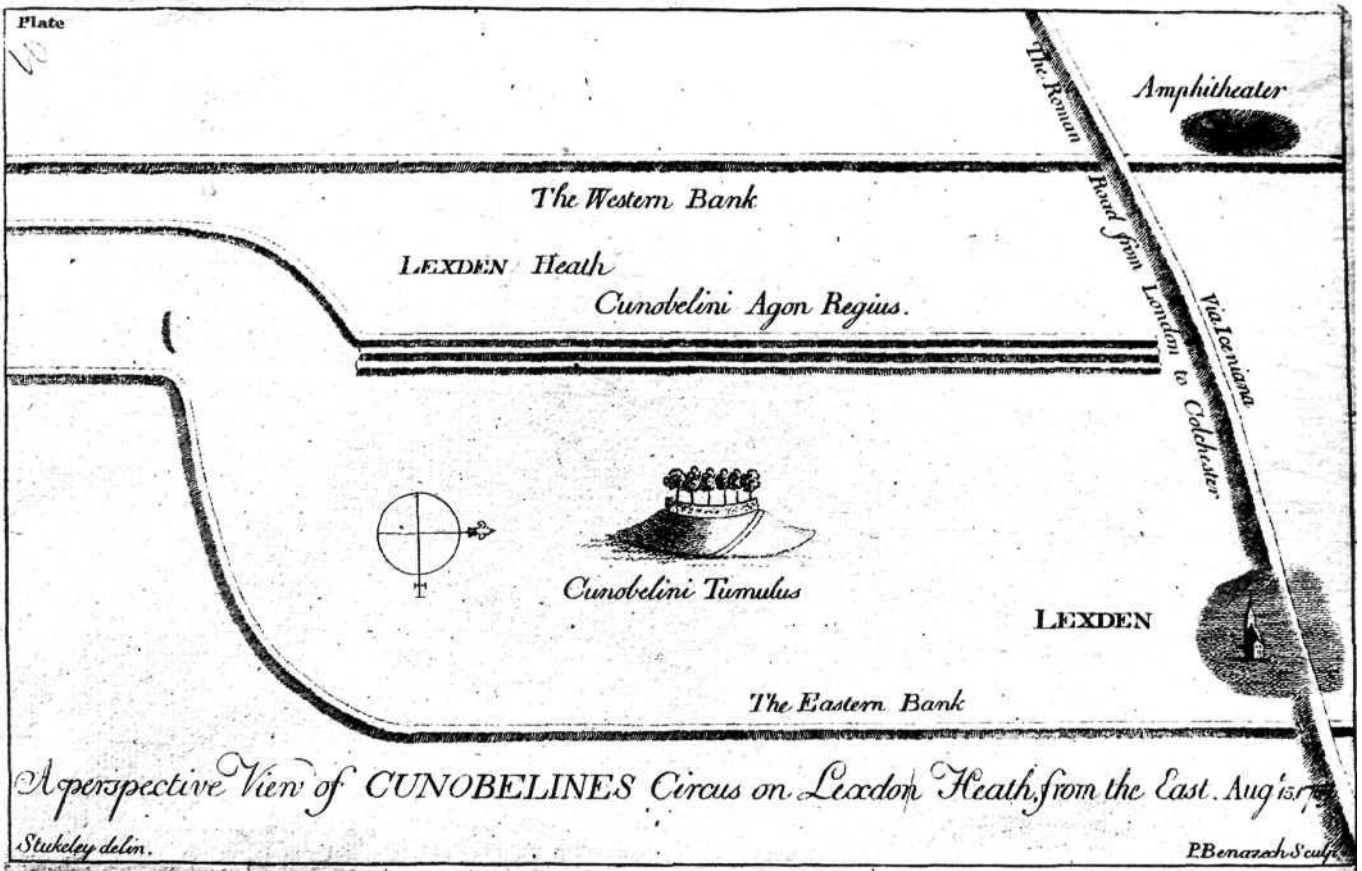
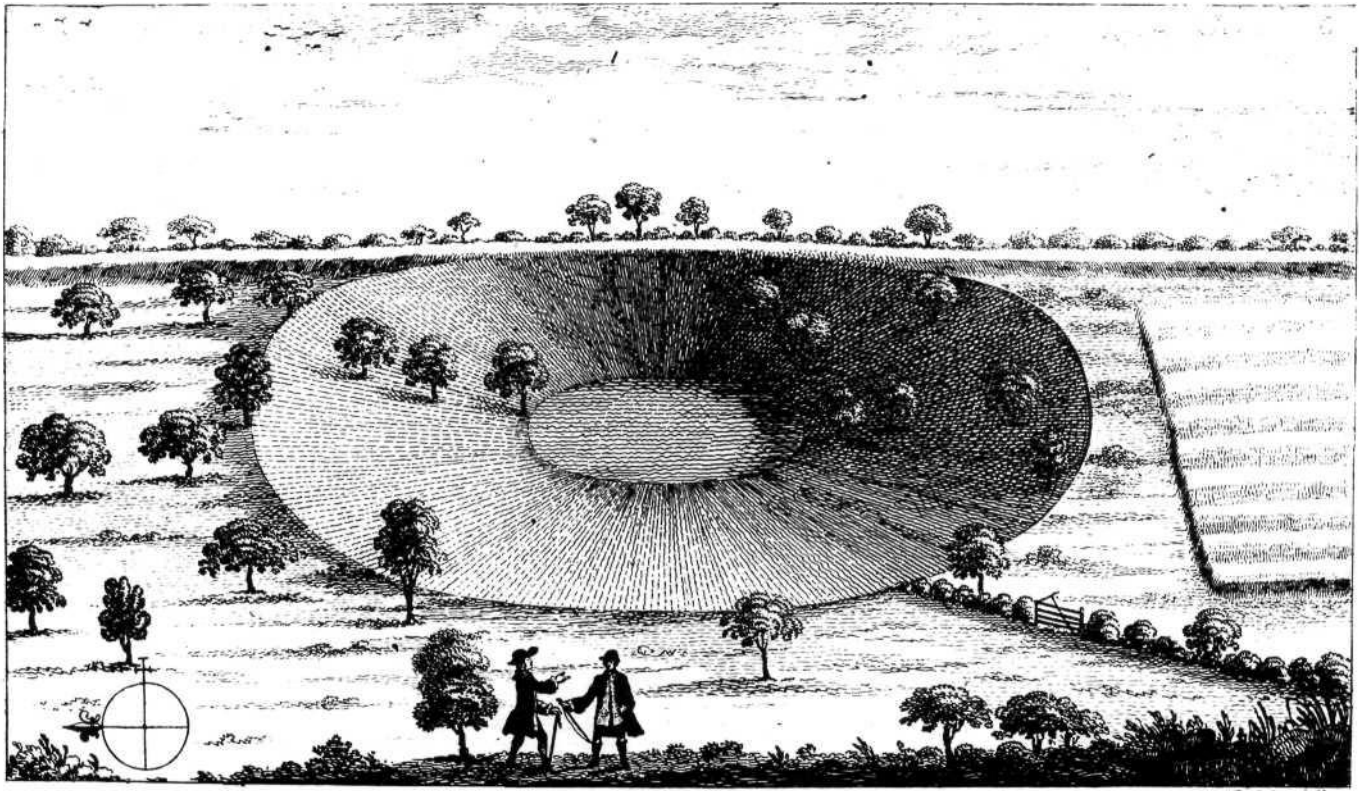


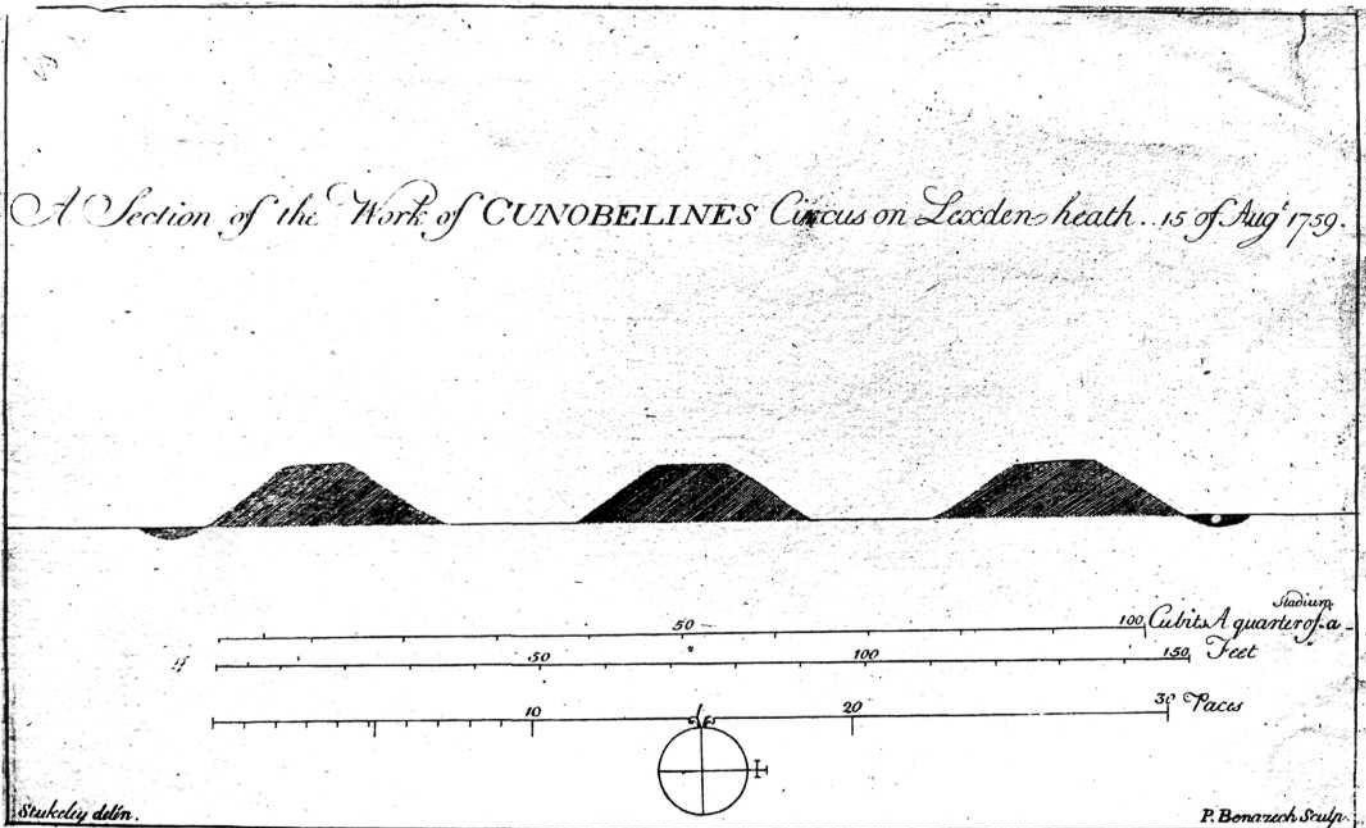
Fig 2.5 Two views by William Stukeley of the earthworks on Lexden Heath in 1759. [Pages 10 & 12-19]





*The British Amphitheater call'd King Coels Kitchen . on Lunden Heath . August 1759 .*

*Stukeley delin.*



*P. Bonazzi sculp.*

Fig 2.6 Above: view by William Stukeley of 'King Coel's Kitchen' in 1759. Below: profile of the Triple Dyke by William Stukeley in 1759. [Pages 10 & 12-19]

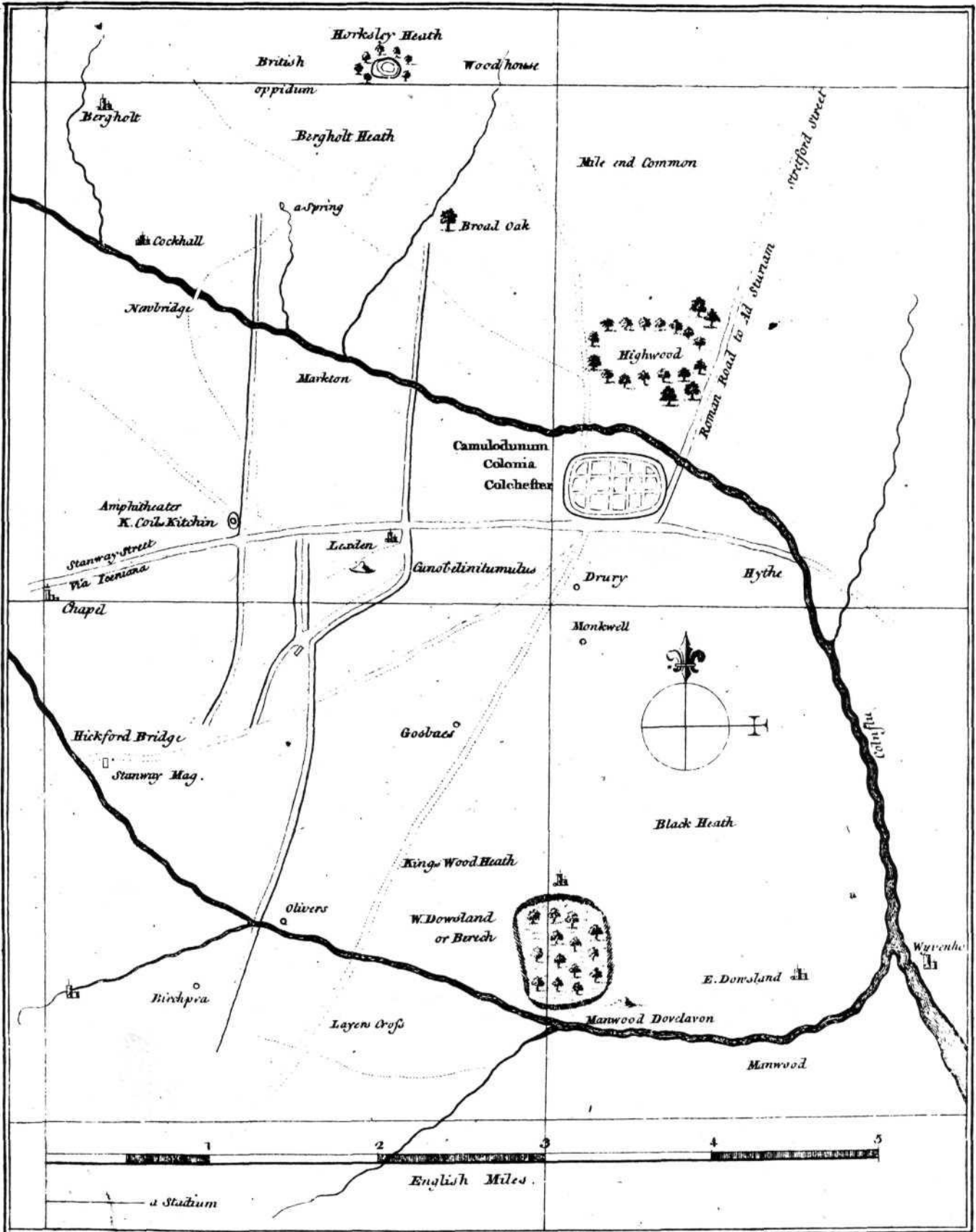


Fig 2.7 Map of 1759 by William Stukeley of the earthworks on Lexden Heath. [Pages 10 & 12-19]

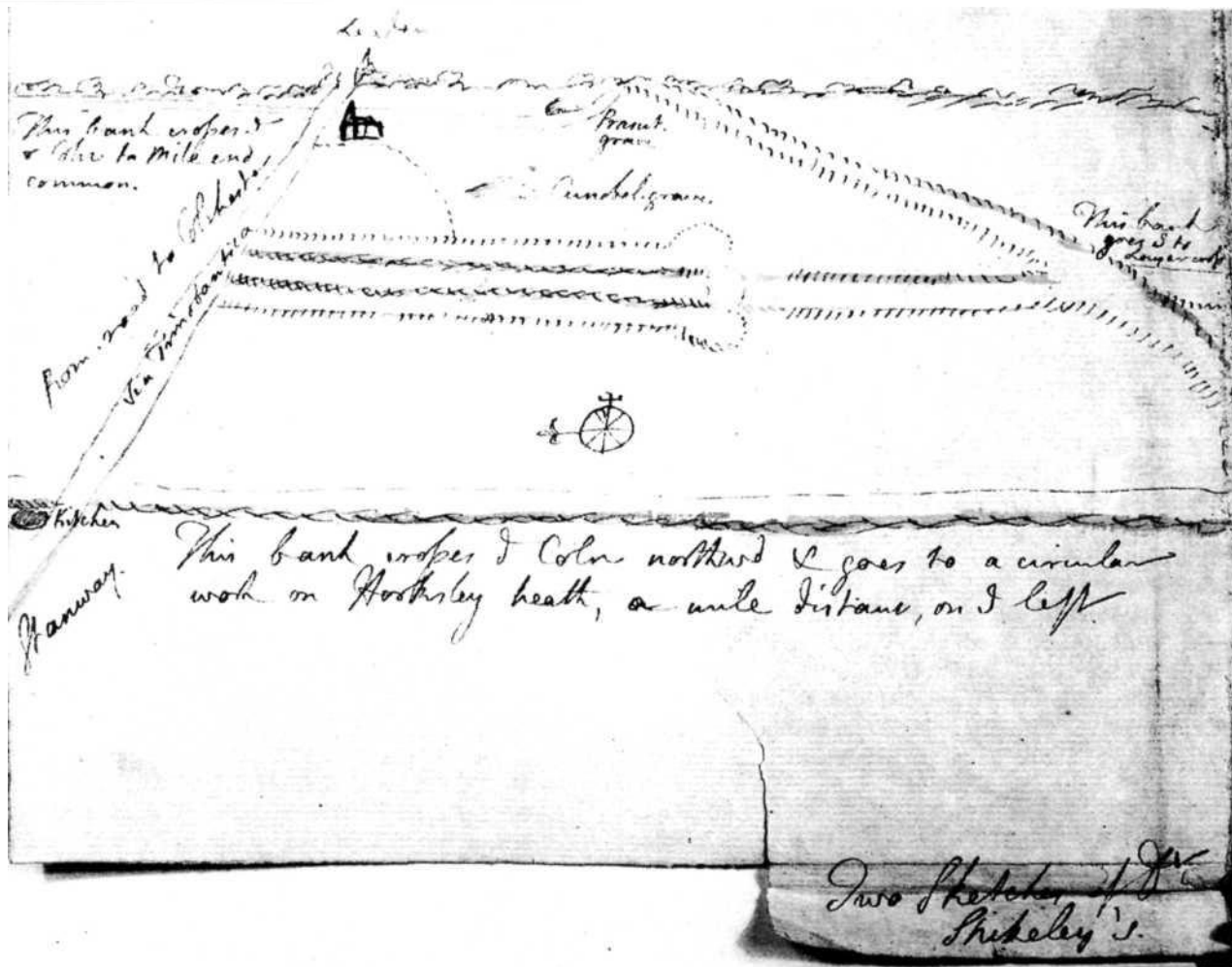


Fig 2.8 A sketch by William Stukeley of the earthworks on Lexden Heath. See footnote 4 on page 10 about its authorship. [Reproduced courtesy of the Essex Record Office. Pages 10 & 12-18]

understand, because of its sharp turn southward leaving an isolated mound just beyond. The slanting line of the first bank, shown in the sketch between the bank running south from the Triple Dyke (in fact Shrub End Dyke) and the farther end of the bent-back portion of the 'Eastern' dyke (Lexden Dyke) — which only the sketch represents — along what now is Bluebottle Grove, merits mention here for its importance.

It is plainly our Prettygate Dyke and its actual straightness is more correctly shown in this sketch by Stukeley, than its earliest mention makes it: in the 1722 survey by Lufkin and Smith, between their 4 and 3, with 'A ditch on each side' as in Figure 2.2 — now confirmed by excavation (pp 46-50) — and continued by a further line to Bluebottle Grove's far end. On the sketch-plan by Morant in Figure 2.3, this line is shown again, and on that in Figure 2.2, still less correct, we have noted the 'ditch on each side'. We shall meet that dyke again, rightly straight and full-length, though with Lexden Dyke's bending very much lessened, on the Chapman and Andre map of 1777 (below, Fig 2.9); but Stukeley's sketch is the earliest to render it correctly as straight throughout. When he came to get his notion of the Triple Dyke as a racecourse, and accordingly drew his originals for the engravings, he made these banks, with

the Lexden Dyke and part of Gryme's, the 'Eastern' and the 'Western' Banks, appear as if boundary-banks for the galloping area.

Before we leave Stukeley, it may be asked how he contrived his 'perspectives'. That in Figure 2.5b is little more than a slanted map (compare the middle part of Fig 2.7); that in Figure 2.5a, however, like the sketch Figure 2.8, seems best understood as a heightening of his view from horseback. The only other point for remarking is his naming of the tumuli — our Lexden Tumulus behind that dyke, and The Mount farther west of it. On his sketch, Figure 2.8, this is called 'Cunobelgrave', and the Lexden one, very surprisingly, 'Prasut grave': meaning the grave of Prasutagus, king not here but of the Iceni, away to the north, who died well after Cunobelin's time, and 16 or 17 years from the conquest under Claudius. Where the engravings show The Mount, they still assign it to Cunobelin, though that of the map, Figure 2.7, has 'Cunobelini tumulus' written behind the Lexden Dyke, where no tumulus is shown, but on a level with The Mount, closer to which this naming would have not had room. Last of all we may note that the London to Colchester road, on the sketch Figure 2.8 and the engraving Figure 2.5b, is named as 'Via Trinobantia', implying it as originally native; and

though that in Figure 2.5a calls it Roman, its calling it also '*Via Iceniana*', though absurdly, may perhaps imply it again as being previously native.

Fifteen years later, John Chapman and Peter Andre finished their survey of the whole of Essex, to be embodied in their map of the county, published in 1777. At its scale, a half-inch to the mile, the area of the dykes that they show is inconveniently small; for our Figure 2.9, this has been photo-enlarged 1.5 times. From the Colchester-Maldon road between Stanway Hall and Well House, but not on the south of it, Gryme's Dyke is shown northward as far as the London road and across it, but not beyond the branching road to Cambridge. It first enters Lexden Heath — the remaining expanse of it still not converted out of heathland — at its angle on what has since become Stanway Green; and most of the rest of the works on the map are within that stippled expanse. On the east, where the future Lexden Park is stippled to match, though with trees in it, Lexden Dyke is shown beside it, but with the bend that leads it farther unaccountably lessened, and with the bank not shown as ditched until beyond (thus along the present Bluebottle Grove). At the end of this stretch, it is met by a bank running in from the WSW, of which the farther portion starts within the heath.

This, perfectly straight, and first so shown, as we saw on p 17, by Stukeley in his sketch (Fig 2.8), is what was recognised there as our Prettygate Dyke. Its marking as obtusely angled, at point no 3 on the Lufkin/ Smith 1722 survey, giving Morant the plan which Figure 2.3, and in Morant's own sketch in Figure 2.2, is explained by the curving doubled line on this sketch, and on the Lufkin/Smith one (single) 3-2-1, the farther part of both being Lexden Dyke beside the subsequent Park. For along the whole of this curve Morant has written, in his sketch, 'These works are vulgarly call'd Hollow Lane'; in other words a sunken lane named Hollow Lane ran right along the curve. The farther part, along Lexden Dyke (Lufkin/Smith nos 2-1), can explain why Chapman and Andre here show the dyke without a ditch: they will somehow have been confusing the lane with this — perhaps because they saw it along the foot of the scarp-like slope, in the northern stretch where the rampart of the dyke had been levelled (pp 40-41).

Returning now to Prettygate in its farther portion, within the heath in Chapman and Andre, one sees that they show it as a bank with a ditch on each side. And this is just what was marked by Lufkin and Smith in their survey, between points nos 4 and 3 in Figure 2.4, and again in the second of the sketches by Morant (Fig 2.3). Though he shows the same curve as in Figure 2.2 — but less correctly where it approaches the London road (on the wrong side of the tumulus) — his note that the farther portion, straight, and part of Prettygate Dyke as we have seen, had 'a ditch on each side', continues, 'The rampart here is 20 paces over'. They were plainly double paces, like the Roman *passus*, which agrees with their showing in the Chapman and Andre map; and how the two ditches, lost to sight since the heath's conversion into

farmland (by the Enclosure Act of 1821: see below), have been re-discovered by two successive modern excavations, can be found on pages 46-50.

It only remains to be noted, in this same sketch by Morant (Fig 2.4), that his writing across the curve (of what in his other one is called Hollow Lane), the words 'interrupted by cultivated land', is explained at once from the Chapman and Andre map. Between the stippled area of what now is Lexden Park, and the edge of the heath where crossed by our Prettygate Dyke, it shows the blank that is its normal rendering of cultivated land; and this is of course that marked by Morant in his sketch. So already by 1758 it had been taken in from the heath.

Of what Chapman and Andre show within the heath itself, we have dealt now with Prettygate Dyke, having recognised it first in the Stukeley engravings, also in his sketch which correctly shows it straight (p 17). This also shows correctly what the 'perspective' engravings omit: the north-south bank of Shrub End Dyke, between the beginning of its triplification and the Prettygate's starting-point. In the map-engraving (Fig 2.7), these points are muddled into one; and its taking the north-south bank right on, to an end well south of the Roman River, should really have been appended to the curving bank outside it, and carried round the west of the Gosbecks site as Kidman's Dyke. Only then should it have turned again south and crossed the river (Oliver's Dyke and Layer Dyke), to end at Layer Cross as Stukeley noted at the margin of his sketch (Fig 2.8) — though Layer Dyke actually ends just north of Layer vicarage.

Anyhow Chapman and Andre have none of this at all. Their map however does show something else, as does Figure 2.4, at a point 540 yds (485 m) north of the Maldon Road which both maps show as the southern end of Shrub End Dyke, but where, in fact, the dyke really bends away to SE. The place is at the spinney stretching east beside Heath Cottage, where actually the line cuts across that of Heath Farm Dyke. What both these maps there show is an earthwork enclosure, on the Chapman and Andre rectangular with corners sharp, but on the Lufkin and Smith with the corners slightly rounded. Nothing of the kind can now be discerned on the ground, but the traces of Heath Farm Dyke in the spinney might have helped to give the idea of an enclosure.

Such enclosures, when their corners are rounded, are of course Roman, either forts or camps. Yet it was not at this place that Lufkin and Smith had a camp, but at their point no 4: the place where Prettygate Dyke starts off, as we have seen, running ENE towards Bluebottle Grove. They state in their survey, 'This place is the Entrance of a Roman Camp'; and before proceeding north along Shrub End Dyke they add that one (other) ridge 'bears S. E. 06'. As this is the bearing that they give to the ridge running off from their no 13, close to no 4 — in fact on Shrub End Dyke just south of no 4 — it appears that the ridge must have run SW; for this is unavoidably their ridge 13-11-10, which has here been

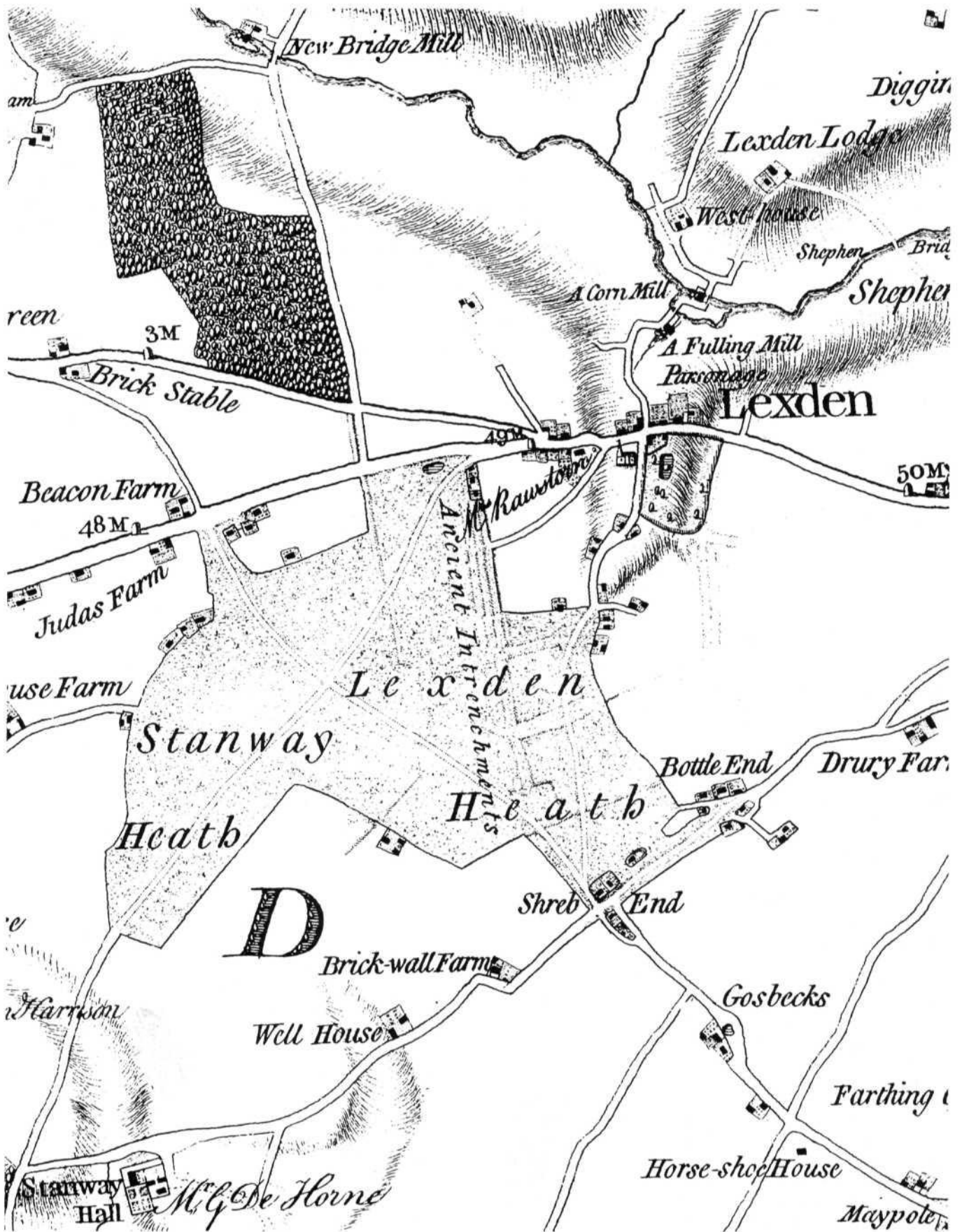


Fig 2.9 Extract of the Chapman and Andre map of Essex, published in 1777. [Page 18]

seen to be Kidman's Dyke (p 116-117), curving SW from just south of the Prettygate's start and from their Roman camp's 'entrance'. Though they do not mark the line that runs in here from the WNW, which is marked in the Chapman and Andre map, and will be seen here (the 'Laver ditch', Fig 2.11, p 22) to be a genuine ditch-line, it seems just possible that its corresponding bank had been destroyed except for an isolated portion, just outside of Shrub End Dyke at the Prettygate's starting-point. For this is where the Stukeley 'perspective' engravings portray the isolated mound mentioned on page 17 — which the map engraving Figure 2.7 also marks, despite confusing all these points on Shrub End Dyke with the beginning of its triplification. Now the Reverend Henry Jenkins, more than eighty years later, knowing the Lufkin/Smith survey from its printing by Morant, with the 'Roman camp' claimed at its point no 4, had a 'Roman camp' again at this point (Jenkins 1842, pi 30); and he declared that at its NW corner was a 'Roman milliary or beacon'. Might this have been the Stukeley engravings' isolated mound, still somehow traceable? Jenkins will be noticed again below; but there is more to come here on the Chapman and Andre map.

Within the heath, north of where it marks our Prettygate Dyke (as identified above) and connected with its northern ditch by a single line, it has a pair of single lines running west to the start of Shrub End Dyke's triplification (shown by three single lines and a ditch-line), making a gap there and running on beyond. Whatever the southern of the pair was intended for, it stops well short of Gryme's Dyke; and the northern barely farther, but bent in the direction of a point on that dyke a little south of its slight shift inward that in fact is for an entrance — now at the NW corner of the Dugard Avenue housing-estate. Though it stops, on the map, without reaching that point, this single line is undoubtedly that of the ditch, on just this alignment, re-discovered in 1949 by Dr St Joseph, from the air in his photograph (Fig 2.11), and on the ground by Mr A F Hall, in his and my own excavations of the 1950s. These showed it to be very early Roman, like the Shrub End Dyke triplification. Here again, then, the value of Chapman and Andre is shown, by what they could see when all the heath had not yet been enclosed and converted into farmland.

This was authorised by the Enclosure Award for the parish of Lexden, under Act of Parliament issued in 1821. The Essex Record Office has both the accompanying large-scale maps (ERO Q/RDc 19, B and C), one at 16.6 inches to the mile, the other at 10. Its eighteen other maps, of various parts of the parish, range in date from 1736 to 1838 (ERO C/T523, DC/T220, D/DE1 P36 and P84, and D/DPa P1-P14). From among them it appears that Lexden Manor (house, gardens, plantations, etc) had already had its own survey in 1819 — with a tree-bearing field in 1817. From its primary owner, the Reverend John Rawstorn Papillon, it had passed to John Fletcher Mills by about 1850, when it was once again surveyed.

As for the Ordnance Survey maps, covering this part of

Essex at their regular scale of 1 inch to the mile, their Old Series sheet 48 was re-engraved in 1838, after previous printings onward from 1805, and re-surveyed starting 1836. About 1850 electrotype printings were begun, and continued over more than the next 20 years; its four quarter-sheets, updated mainly by adding more railways, formed the edition of 1884. But our dykes, and other such features, will mainly still have rested on the survey of 1836-8, prior thus both to the county map by Gilbert (1845, not here of particular importance), and to the map of the dykes by the Reverend Henry Jenkins in his article of 1842 (Fig 2.10). On his unreliability, and not infrequently evident recourse to imagination, we shall be quoting soon Dr Henry Laver's judgement, in the 1880's. But when this region was surveyed, in the 1870's, for the first edition of the Ordnance Survey map at the scale of 6 inches to the mile, this sheet (and the 25-inch edition too), did at least once draw upon Jenkins. From his repeating, at the Lufkin and Smith point 4 (Fig 2.4), their 'Roman camp', and adding at its NW corner 'Roman milliary or beacon' (just possibly explainable as above, but in any case absurd), it named the point 'supposed site of Roman milliary or beacon' — whence these same words on the 6-inch town map of Colchester (1921), still on sale in the 1940s. The idea is now a thing of the past.

Yet it leaves some interest still in the Survey's treatment of the dykes themselves in its map's successive editions at the 1-inch scale, 1838 to 1884. In the 1884 edition's reprint, in 1970 and again in 1980 (in the Royal Institution Library, Albemarle Street, London), their historian Dr J B Harley stresses the basic importance of that of 1838, on which the 1884 one will have mainly relied for such features as these. It marks Gryme's Dyke from the London road south to Stanway Green (where named in Gothic type 'The Ramparts'), the Triple Dyke only in the short stretch still preserved beside Lexden Straight Road (no name), 'Pitchbury Ramparts' (Gothic), and Berechurch Dyke (named THE RAMPARTS from Berechurch Hall Road south to the Roman River, where named ENTRENCHMENT — but north of that road, on a line due north but not through the modern Borough cemetery, as ROMAN WAY. That was the most that the Ordnance surveyors recorded, 1838-84. And in 1885 began the long series of papers on the dykes, or various parts of them, by Dr Henry Laver. His last came in 1905, and the results were all used in the Royal Commission's volume, published 1922 from its own re-survey, begun in the years just before 1914. The extent of open farmland which the Royal Commission's map of the dykes showed within their westernmost lines and stretching away towards suburbs of Colchester (RCHM 1922, 72) contrasts quite sharply with its reduction through the modern growth of these — out through the westernmost line (Gryme's Dyke) and into Stanway, widely SE from Lexden nearly to Berechurch, and south from the town to Blackheath. Even the 1980 OS map, 'Colchester and the Blackwater' (1:500,000 sheet 168), shows less building-up than today's; thus the work on the dykes and within them, since the Commission's map was published, has throughout been timely.



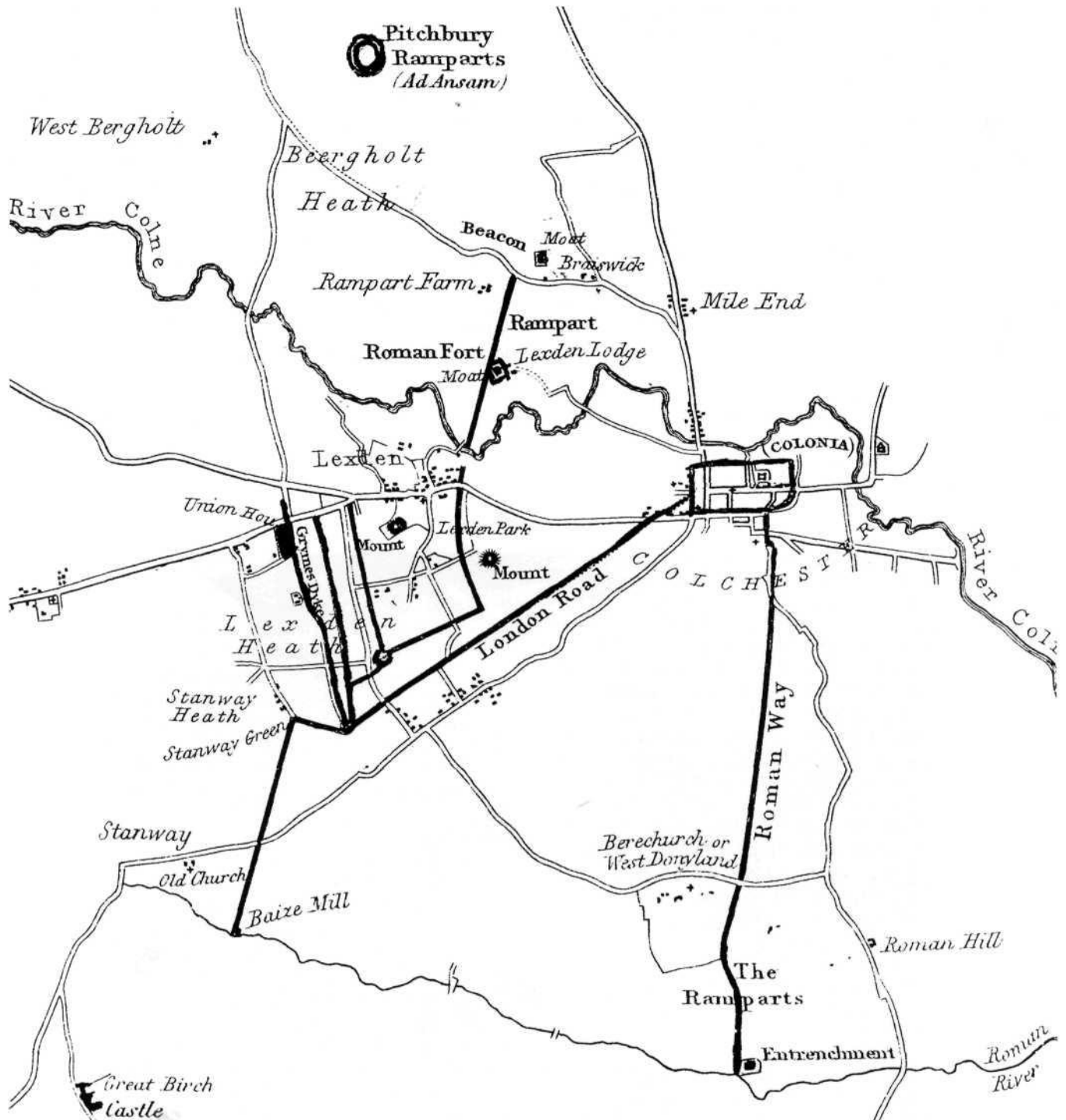


Fig 2.10 Central part of Jenkins' plan of Camulodunum. Reproduced from Roman Colchester (fig 117) by permission of the Society of Antiquaries of London. [Page 20]

**An introduction to work since 1922, on the dykes, and within them**

[Fig 2.11]

The first important operation in these years, outside of Roman Colchester, was the excavation of the Lexden Tumulus, by Dr Henry Laver's son H E (Ted) Laver in 1924. His untimely death soon followed, and his younger brother Philip, who had taken some part in the

work, prepared an account of it, and of various principal finds, which he read (23 January 1926) at a meeting of the Society of Antiquaries. This was published, with a report of the discussion there (from notes by Mr Reginald Smith, then a vice-president), in *Archaeologia* 76 (Laver 1927). All the finds are in the Colchester Museum, and in 1941 were joined there, in Philip Laver's bequest of all his papers, by the excavator's original drawings and notes. These were used in the

re-assessment of the work, with exhaustive publication of the finds, by Dr Jennifer Foster (1986). (See pages 85-94 for this and for the companion historical essays by the present writer.)

Mr M R Hull, the museum's curator from 1926 until 1963, was in his first years occupied with discovery and excavation inside the Roman town, but throughout was increasingly aware of the importance of the area of Sheepen Farm. This had been indicated already in the 1840s by William Wire, then was so again in 1905 and 1926 from finds along the foot of the hill there, and from those being gathered from 1923 onwards, by E J Rudsdale (museum assistant) and his friend George Farmer, in their watch on the two sand-and-gravel pits on the hill-slope there (*Camulodunum*, 22). Thus it was Hull, in his report for 1928, who first put the Sheepen site forward as potentially that of pre-Roman Camulodunum (*CMR* 1928, 22). The result in 1929, when the plans for the by-pass road along the foot of the hill were made known, was the committee that promoted the excavations of 1930-39, discovering the Sheepen Dyke and extending, by the time they ended,

over almost all that site (*Camulodunum*, 21-3). Of the great array of the dykes outside of Sheepen, viewed as pre-Roman but never yet dated from actual finds, it was opportune to choose one for comparing with Sheepen's own, by excavation; and this was done in 1932 on Lexden Dyke, in Lexden Park, under the then Miss Thalassa Cruso (of the London Museum), after first joining H W Poulter (of Colchester Museum) in a further survey of all of the dykes then visible. Her finds from Lexden Dyke confirmed its date as contemporary with Sheepen (p 44). Then in June 1933 Mr O G S Crawford, the Ordnance Survey's first Archaeological Officer, procured the taking by the Royal Air Force of a set of aerial photographs, vertical and covering the whole of the Colchester region at a uniform scale. These immediately and long after proved invaluable for the study of the topography of the dykes.

Mr A F Hall, of the Colchester Royal Grammar School, who in the early part of the Sheepen excavations had briefly assisted Mr Hull, became thereafter active in intermittent sole exploring of some of the dykes, making many maps and plans, until his death in the spring of



Fig 2.11 Aerial photograph by J K St Joseph of the palisaded earthwork and the 'Laver ditch' converging on Gryme's Dyke. © Crown copyright/MOD.



1961. But this involved him also with Roman roads, and especially in the grounds of the Grammar School (the main building, Gurney Benham House, and Gilbert House: hence his paper on the three-tracked road there (Hall 1942) and on the Roman walled cemetery adjacent (Hall 1945)). His further endeavours, to explain the large ditch, plainly unfinished, which he found along the road's south side but on an alignment slightly different, are discussed on pages 48-50. He had started on those operations not long before the war of 1939-45, during which he was able to complete them; but in 1940, when on Home Guard service, he chanced to make a vital contribution to our knowledge of the dykes. When engaged in digging the tank-trap ditch along the Bluebottle Grove sector of Lexden Dyke, through its ditch-silt and paring the sand below its rampart's foot, he found a filled-up ditch running across from it to pass beneath the rampart; and this is now proved to have been the ditch of our Heath Farm Dyke.

In the years that followed the war, among his other explorations, he examined much of the entrance and gateway through Gryme's Dyke (now the NW corner of the housing-estate stretching north from Dugard Avenue), thus bringing on, in 1947-8 and 1956-7, my own excavations reported below (pp 27-9). He had also recognised the early Roman palisaded ditch — noted on page 20 as a line on the Chapman and Andre map — slanting from the end of Shrub End Dyke's triplification, across into Gryme's Dyke south of the excavated entrance. And with this we revert to the vital modern means of tracing buried earthwork-ditches from their filling's darker colour: aerial photography. That palisaded ditch was clearly revealed by Dr J K St Joseph, in his oblique aerial photograph (Fig 2.11). Moreover his successor at Cambridge, Dr D R Wilson, and independently Mr J N Hampton of the Royal Commission on the Historical Monuments of England, at the same time in 1976, each took aerial photographs giving the plan, with many internal details, of the Roman fort close beside Gosbecks. And of the Gosbecks site itself, the account (pp 95-105) has for the most part rested on aerial photography alone.

Excavations at separate points on the dykes may be noted briefly next; the two first were Mr Hull's. In 1936 he dug at the 'Peartree Junction', that of Prettygate Dyke with Shrub End Dyke (pp 46-8; *Rom Col*, 4-5). In 1939, when he proved the Roman fort near Altnacealgach (pp 62-3; *Rom Col*, 271-3), he also sectioned the ditches pointing nearly NE towards it (his A and B: *Rom Col*, 272, fig 117). That A was that of Heath Farm Dyke in its northernmost stretch (where the parallel B ran close beside it) was confirmed by the cuttings in it made by Mr Hall, farther back south-westward (p 32). My own in the Gryme's Dyke entrance first exposed by Hall, and behind the dyke, southward of that, not only proved the Roman date of the palisaded ditch, and found it in 1956 cutting obliquely through the Gryme's Dyke rampart, but also discovered that a larger Roman ditch, directly subsequent, had passed through the Gryme's Dyke entrance and at once turned south, running on till it had cut across the palisaded ditch,

and then (while a branch ran farther south still) curving away into a straight course, ESE. This is the course that Dr Henry Laver knew, and claimed to be that of a major Roman road, bending, where crossed now by Lexden Straight Road, to head for Colchester. As our ditch had come through the Gryme's Dyke entrance and had no apparent rampart, its explaining as the ditch of that road can scarcely be doubted; yet the 'Laver line' (Hull's term for it) still has its puzzles. In the section on early Roman roads below (pp 68-9), where Road 1 is the main road from London, this is Road 2. Hull, when he discussed it, dismissed it altogether (*Rom Col*, 4-6), except in the final stretch, numbered here as Road 4, which runs ENE from the Grammar School up to Roman Colchester's Balkerne Gate, after turning sharply off from Road 1's main line. Road 2's outstanding puzzles involve particularly Prettygate Dyke, first at its start where Hull dug in 1936, and again where its course had been halted, on Lexden Dyke at the end of the Bluebottle sector, in the area of the 1957 excavations. Both sites, our 'Peartree Junction' and 'Prettygate Junction', are described on pages 46-50. Thus the 'Laver line' question, at all the three places here noted, is bound up with evidence from excavations on dykes.

In 1956-7 were also cut four more sections: one each at the ends of Dugard Dyke (p 26), through the upstanding portion of Kidman's Dyke (p 33), and one through the Gryme's Dyke ditch south of Dugard Avenue (pp 28-9). Mr Hall's operations on Berechurch Dyke (following a section cut by Mr Hull on the ditch at Barnhall), are described on pages 24-6, and a more recent section cut farther south is dealt with on pages 159. Mr Brian Blake, while serving on the staff of the museum, explored in late July 1961 the course of Layer Dyke (p 34), south through and past the fringes of Chest Wood, and cut a section there across its ditch. In August 1963, when a water-main was laid to run east from Lexden Straight Road where it bends to the SSE, towards Shrub End on the Maldon Road, he watched its trench in the western end of the spinney which there stretches east, and planned and measured the section cut by the trench through Shrub End Dyke's ditch (pp51-2).

In August 1961, closely south of Dugard Avenue, the buried ditch of Kidman's Dyke was located and a section cut (p 33); directly afterwards, north of the London road, in the expanse called Hunter's Rough, an excavation was made across the Triple Dyke, sectioning all its three ditches and clearing the rampart-spaces between them (pp 55-61). I had charge of both sites, assisted at the latter by members of the Colchester Archaeological Group, led by Mrs K de Brisay. But it was only Philip Crummy's interpretation of my sections that explained the Triple Dyke: as an early Roman tripling of the native Shrub End Dyke North. His excavation and observations in 1971-3 in the Kiln Road area, south of the top of Sheepen Hill, are reported by him on pages 131-7; and those of features northward from the Gosbecks site on pages 116-20. See also pages 109-115 for the 1977 section across Gryme's Dyke Middle.

The combined results of these intermittent activities, through barely less than 60 years, have now superseded the frontispiece-map in the 1947 *Camulodunum*, and replaced it by the general map here published as Figure 2.1.

## The Iron Age dykes

(in alphabetical order)

[Fig 2.1]

### Abberton Dyke

This dyke starts (no doubt again at the edge of wet ground) directly south from the Roman River's tributary Layer Brook, where crossed by a footbridge almost exactly opposite the end of Berechurch Dyke directly north of it. Along the path leading south from that bridge, a discernible fold in the ground should mark its line. On the path's turning SSE through the grounds of Abberton Manor, it still will have followed the line, however invisibly. For south of where the path turns off and joins the road heading east up Oxley Hill, the profile appears where the modern 6-inch Ordnance Survey map shows it, running south to end just outside the Abberton churchyard, after passing, very plain, by the edge of the grounds of Abberton Hall. From its start to its end is a distance of only some 830 yds (760 m) but its continuing from Berechurch Dyke, matching that of Layer Dyke from Oliver's Dyke, shows a likeness of plan, facing here east instead of west like all the others that are later than Heath Farm Dyke.

### Barnhall Dyke

See Berechurch Dyke following.

### Berechurch Dyke, with the Barnhall sector

[Fig 2.12]

Berechurch Dyke, aligned at S15°W, is crossed by Berechurch Hall Road between Berechurch Hall and Blackheath, 300 yds (290 m) west of Blackheath Farm. For the next three-eighths of a mile (600 m) southwards, the steep inner slope of its east-facing ditch defines its course on this alignment, while its rampart, considerably lowered and set with trees, carries a lane. To its course so aligned before reaching that road we shall soon be returning, but it has crossed the Birch Brook (where it was dammed into ponds at the now long-demolished Monkwick Farm), rather more than 500 yds upstream from the Mersea Road bridge. Hull in his *Roman Colchester* (p 10) put an end to the theory (Henry Laver's, followed by others) that it was really a Roman road, from a south *colonia* gate beside

St Botolph's down to Monkwick in a fancied straight line, and thence along what is really our Berechurch Dyke.

The dyke's own course down to Monkwick, however, admits of connexion with a first piece approaching it westward — actually at W15°S— from the east. From Barn Hall, which is near it, east of the Mersea Road and to the south of Bourne Mill, this has been named the Barnhall sector. East of the Hall, along the old Ordnance Depot area's northern edge, where ground was being trenched for sewers, a section cut in April 1945 found a ditch of size suited to a dyke's (Hall 1960, 10; Fig 6.1, no 76). This sector must seem to have started from the flood-plain of the Colne, beside New Quay where it would have been wide through the confluence of the Bourne Mill stream (now dammed for the pond east of the Old Heath Road). The section found the ditch a good deal filled; pottery was absent from the primary silt, but was present higher up. This was thought at the time to be Anglo-Saxon, but must in fact have been, as Paul Sealey has pointed out to me, of the Little Waltham type of the Middle Iron Age but lasting at its type-site till 50-25 BC (Drury 1978a).

That the dyke implied by the ditch should belong with the others of the Iron Age system, though not here attested outright, could at any rate be guessed; and Hall determined to test a connexion for its western end with Berechurch Dyke. He reported all this work in full in the *Colchester Archaeological Group Quarterly Bulletin* (Hall 1960).

The alignments of the two, at a wide obtuse angle to each other as of course they had to be, allowed of a connexion only within the Borough cemetery. Consequently Hall, with great patience over several years (1945-7), prevailed upon the grave-digger there to let him observe, when graves were dug in places that could serve his purpose, any meeting of loose material instead of natural gravel, and anywhere the natural gravel had been undisturbed. At last, he had me to visit the cemetery with him, be introduced to the friendly grave-digger, and have the final results of these probings explained to me on the spot. They were these. The Barnhall ditch, still at W15°S, had crossed the cemetery as far as a point somewhat short of its western boundary, where the looser material — its filling — gave place to natural gravel at the normal depth. So too with the ditch of Berechurch Dyke, on a line about S24°W, from the same place where the depth of the gravel was normal. Hall inferred that at this place, where the two lines would otherwise have connected each with the other, at an angle close to 140°, there must have been an entrance. This itself will be confirmation of their having been designed together — there being nothing to show for a prolonging of either beyond it. However, this entrance-gap had been closed, at some time, by a cut across it — similar to that across the Dugard estate's Gryme's Dyke entrance (pp 27-9), so perhaps of AD 60 or 61. Hall's plan of his cemetery observations is reproduced as Figure 2.12.

On beyond the cemetery's SW corner, Berechurch Dyke will have made the bend of 9° needed to bring it to its farther course at S15°W. It was found on this

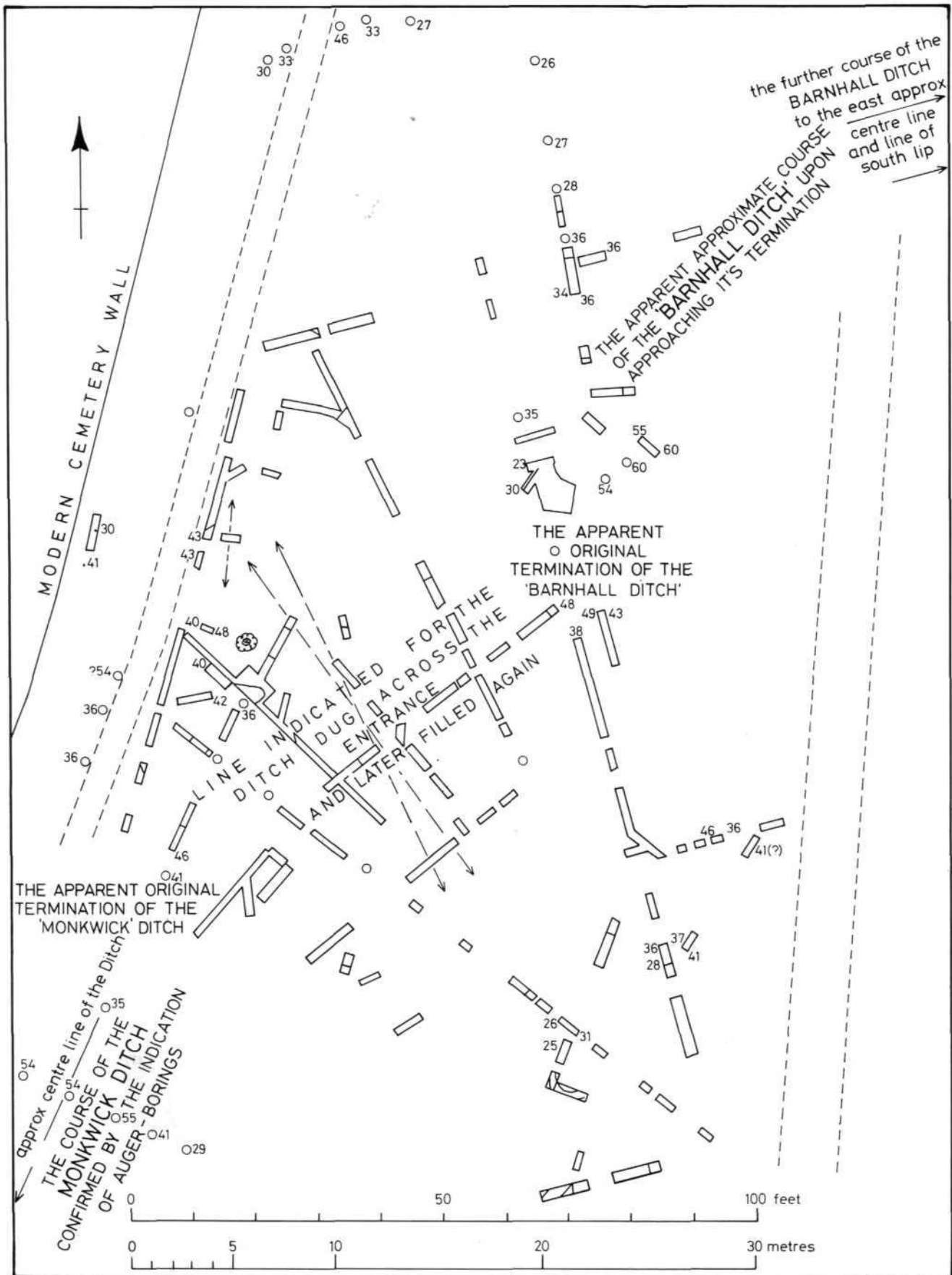


Fig 2.12 Plan of the trenches recorded by A F Hall at the Borough cemetery on Mersea Road where the Berechurch Dyke ('Monkwick' Ditch on the plan) meets the Barnhall Dyke. (No 71 on Fig 6.1.) [Pages 24-6]

course already some 170 yds (158 m) short of Monkwick, beside the crossing of two field-hedges, by Hall in a section dug on 4th January 1944 in which he located the ditch and the (lowered) rampart (Fig 6.1, no 77).

South of Berechurch Hall Road, where we saw at the start what profile the dyke now has, Mr Crummy took a measurement across it and a cut to reach the bottom of its ditch (12 November 1984), 350 yds (320 m) from the road (Fig 6.1, no 78). On beyond, where the alignment changes to S20°E, the profile is almost immediately levelled off, leaving the lane that the rampart has carried thus far to proceed along its line, but on the flat. And where the line bends again, to S5°E, on approaching Park Farm to which the lane turns off behind it, Mr Hall was able to explore a quite extensive occupation-site, in 1945-6 (Fig 6.1, no 72). He dated it by obtaining an amount of Late Iron Age pottery (now in the museum) and he cut a section through the profile where it reappears, farther south and almost half-way down the length of the wood called Charlotte's Grove, which extends outside it to end on the path running west from Rock Farm and has a footbridge across the Roman River (Fig 6.1, no 79). Though the bridge is exactly on the line of the rampart, the profile disappears just short of it, no doubt at the edge of the ground through which the river then would have flowed — thus terminating Berechurch Dyke.

### **Dugard Dyke**

Behind the northern half of Gryme's Dyke Middle, not quite parallel with it but at an average distance from it of 185 yds (170 m), this is named from the Dugard housing estate, which it crosses from north to south. On beyond Dugard Avenue and its southern row of houses, where close to their back-gardens' hedge was cut a section through its ditch in 1961 (Fig 6.1, no 16), it will have ended just short of the curving line of Kidman's Dyke. North of the avenue, it must have been crossed (so already filled in) by Roman Road 2 (pp 23 & 68), the ditch-line of which slants obliquely across the estate towards Peartree Junction. Mr Hall's intention of a dig to test the crossing was frustrated by the speed of the estate's developers. However, the dyke's north end was proved in 1957 (Fig 6.1, no 15) by the section (ditch-dimensions as above) to have been close to the estate's north hedge, about half-way along, making the dyke's total length about 484 yds (443 m).<sup>6</sup>

Further to be noted briefly here is the fact that the early Roman ditch, flanked on its north side (the inner) by the slot for a palisade (as was proved in the excavations farther west), ran slanting immediately past the dyke's north end. Its purpose, in the Roman Phase 1 to which it certainly belongs, will be found on pages 59-61 below to fit the morrow of the conquest, in line with our findings

farther along it (in the excavations just mentioned above).

As for dating this dyke itself, in relation to Gryme's Dyke, what invites attention most is its position. Its north end is nearly 40 yds (37 m) farther north than a line due east from the middle of the Gryme's Dyke entrance in the Dugard estate's NW corner. Pre-Roman traffic entering that, and prevented from passing to the south of it — if turning to the right — by finding that end of it closed by the curve of Kidman's Dyke, had therefore to turn half-left and pass by its north end. If the traffic then could somehow cross Shrub End Dyke, a straight course onward would bring it to Lexden Dyke; and there, whichever entrance it made for, it could safely be controlled, if not kept out. Thus Dugard Dyke would seem either designed along with Gryme's Dyke as an obstacle behind and supporting it, or at least so designed as an afterthought while Gryme's Dyke was being built. This way of explaining it does seem likelier than supposing it built already a little before Gryme's Dyke.

### **Gosbecks Dyke**

[Figs 5.1 & 6.1]

#### *South*

This is the outermost earthwork rounding the SW quadrant of the Gosbecks site, above the 100 ft contour and south of the stream flowing out of the site; length about 510 m (some 550 yds). Ditch clear on aerial photographs (hence on plan in Fig 6.1). Surface profile degraded (more so towards SE end), but appearing some 10 ft (3.0 m) across, as in Henry Laver's section, taken 1902. He observed from the roots of an oak there how greatly the bank had been purposely lowered. They were upstanding from the bank for at least 4.5 ft (1.38 m) before reaching the trunk, thus showing not only the previous height of the bank, but its having been lowered within the lifetime of the tree, which he put at 100-150 years (Laver 1902, 371). So the lowering will have been a sequel to the field's enclosure, at whatever time after the Lexden Enclosure Act of 1821 (p 20).

#### *North*

Its ditch resumes the outermost line from a point 24 m (some 26 yds) beyond the stream, crossing a narrow scarp before reaching the 100 ft contour, then bending N10°W, a course interrupted by the Colchester-Maldon road. Of its total length of about 800 m (860 yds), 470 m are north of the interruption, and its course for nearly half this distance is almost true north. This takes it well away NW from the Gosbecks site, and might suggest allowance for the site's expanding farther than it did (see again Fig 6.1). The destruction of the bank has left the aerial photographs showing only the ditch.

<sup>6</sup> Gilbert's plan of Colchester (dated 1854) shows what appears to be Dugard Dyke extending northwards as far as the London road. See further p 172.

*Henry Laver's putting our Gosbecks South into a longer line of 'Rampart'*

Henry Laver's article of 1902 took the name of Oliver's Thicks, the wood that then stood in front of our Gosbecks South, to entitle not only this but a line much longer, which he reckoned it belonged to. Ending southwards on the vicarage garden at Layer-de-la-Haye, it was there our Layer Dyke (p 34); between the Roman River and Gosbecks Dyke South it was our Oliver's Dyke (p 46). His start for it, however, was over 2.6 km (1.6 miles) distant from Gosbecks Dyke South and away to the NE, at the south end of Bluebottle Grove (for which he used the Victorians' politer name 'Bluebell Grove'), from which its line, running WSW, is in fact our Heath Farm Dyke. He followed this correctly — mapping also our Kidman's Dyke — into the curve of Heath Farm Dyke Middle which protected the west of the Gosbecks site. However, from its end (nearing the stream here mentioned above), he drew a dotted line veering SW, quite wrongly, to connect it with Gosbecks Dyke South — leading thus to the completion, over at Layer vicarage garden, of a 'Rampart' almost 5.4 km long (3 miles and nearly 600 yds in all). 'Oliver's Thicks Rampart' was his name for the whole of it, though the wood so named touched only a little of our Gosbecks Dyke South; it does so now no longer through its cutting back westward to the edge of Butcher's Wood.

Laver was uncertain whether any of his 'Rampart', or even all of it, might have been or might have served as a Roman road; that it could have been at all pre-Roman, he refrained from guessing, and that what he fancied was a unity was really a conflation of his own, out of a number of dykes, set mostly end to end, and all beyond doubt pre-Roman, he never declared. Yet we shall find that something like that, though with differences certainly, was seemingly effected in fact at a certain stage within the dyke-building period (pp 46-8). Of those differences, the chief concerns Heath Farm Dyke (see pages 29-33).

**Gryme's Dyke**

This outermost, longest, and latest of all the westward-facing dykes was designed in two parts. The northern of them, southward from the Colne, is our Gryme's Dyke North, with its south portion, here called Gryme's Dyke Middle. The other part, Gryme's Dyke South, seeming laid out northwards, runs up from the Roman River to Stanway Green, where it turns sharply in to cover the ending of Gryme's Dyke Middle. The topographical account given here is set out in that order.

*Gryme's Dyke North from the Colne very nearly as far as the London road*

Gryme's Dyke North begins by the Colne's south bank at New Bridge, beside the road running SSE, up to Chitts Hill and across the railway. The road must

run along the line of its levelled rampart, for beside the length of this, on its outer side, crossing the river's broad flood-plain, a dark line on the 1933 aerial photographs marks its ditch. This disappears when it begins to climb the hill, but the long-existing Colchester Borough boundary, running straight from its crossing by the railway to the London road, at SSE by south, must here have followed the rampart's line, as the old perambulations have recorded right along it to the southern end of Gryme's Dyke Middle. On approaching the London road, its ditch is swallowed by the great old pit named traditionally 'King Coel's Kitchen'; and just past that, under the modern King Coel Road and the properties that flank it, will have been a principal entrance. Roman Road 1's use of this must place it where the Roman branch road ran out from it nearly NW, as was proved by a cutting of Hull's just south of the railway: Road 8 (p 69 below). The entrance was therefore slightly to north of the modern London road.

*Gryme's Dyke North from the London road to the NW corner of the Dugard housing estate*

Covered by the London road and southward from the east side of Council Road, its rampart begins to be visible a quarter of a mile from the London road (400 m), where the biggest remaining portion of the former Lexden heath (field 1248) has its northern field-hedge. All down the western side of that field, and of the next (the subdivided 1252), the rampart — somewhat lowered and with a belt of trees along it — is continuous but for a short gap opposite the northern of the New Farm buildings, on the lane that runs all along its ditch. On approaching the east-west footpath beside the northern hedge of the Dugard housing estate, the dyke and the lane bend inwards; and in the short open space before they resume their southward course, the gap so formed in the rampart is explained, as Mr Hall was the first to infer, by the presence of an entrance. And this, some 1100 yds (1 km) from the entrance farther north, marks the end of this latter part of Gryme's Dyke North.

*Gryme's Dyke Middle, and the entrance*  
[Fig 2.13]

In this entrance, at the NW corner of the housing estate, Mr Hall made the excavation, in 1946, that confirmed it. I was able to complete Hall's work on the entrance in the spring of the next two years, with the help of Oxford University Archaeological Society volunteers and, in the second of them, Colonel R J Appleby, who finished his work alone later on that year. At the same time I carried out most of the excavations on the dyke to the south which, with the then Miss Margaret Smith, I returned to finish in 1956-7, apart from final work undertaken in 1958 by Colonel Appleby.

Everything found east of the dyke was early Roman, and will be dealt with below under the 'palisaded

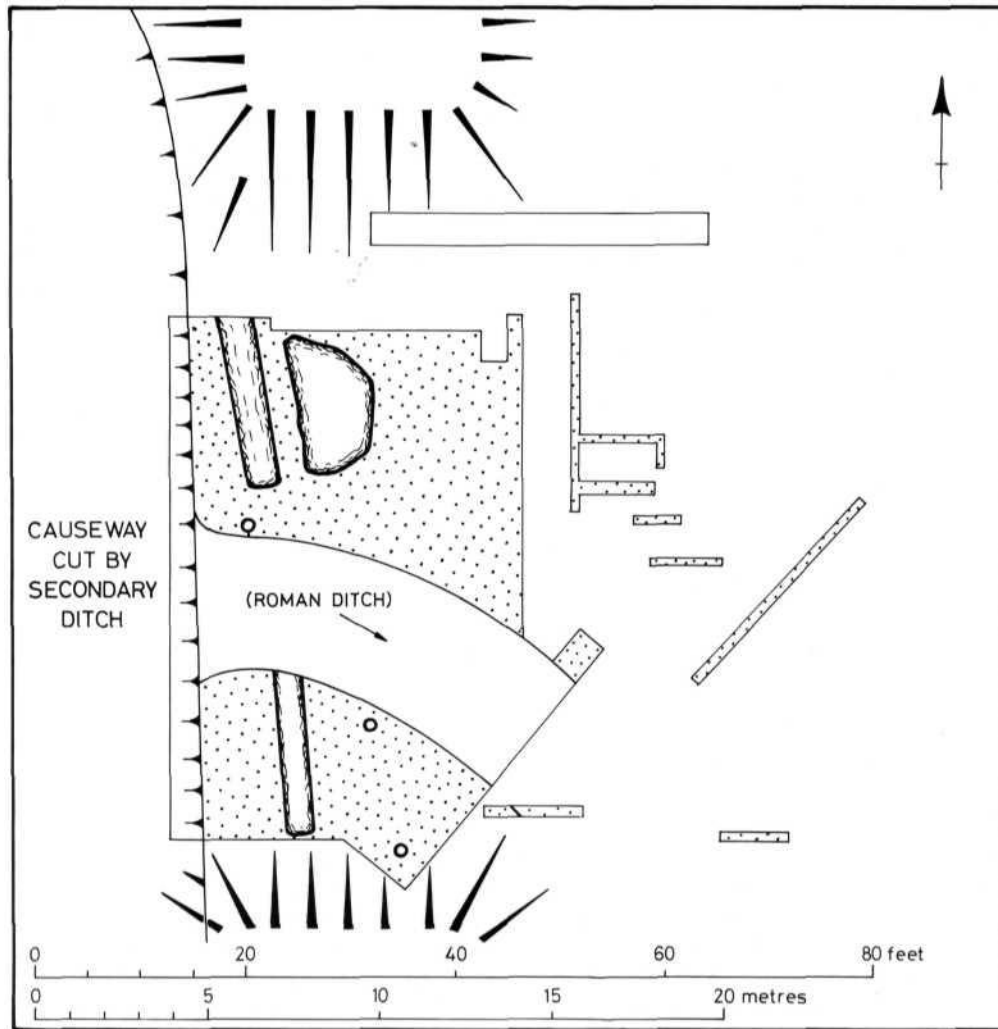


Fig 2.13 Plan of the 1946-58 excavations of the entrance of Gryme's Dyke. (No 19 on Fig 6.1.) [Pages 27-9]

earthwork'. Our present concern with the dyke itself, beyond a brief description of its present-day profile, need only be directed to the entrance — though Roman Road 2 is already involved, being found to have passed in through it. The rampart of the dyke, just as in most of Gryme's Dyke North, has in all this middle stretch been somewhat lowered, and the explanation is, as Miss Smith and I were able to confirm, that its upper part had been thrown down into the ditch to make a bed for the lane which now runs along it. As for the entrance, it was a gateway built with posts, set in normal post holes (Fig 2.13). The gateway was destroyed to let Roman Road 2, gravel-metalled, use the entrance — with a south-side ditch which turned with it, directly within, to run for some way along the Gryme's Dyke rampart's tail. The reason will be given below on pages 59-60, the latter concerning the road and its turn, still with the ditch, to run SE.

But notice is appropriate here, to revert to the entrance, of the fact that the ditch of the dyke — which in front of

it must from the start have left a gap of solid ground, for in-and-out passage, and so for the Roman road too — had been joined up later by a cutting that abolished the gap. The cutting was found re-filled, and though the lane confined excavation of the filling to its inner side only, this side of the cutting had a downward slope implying sizeable dimensions. What seems the likeliest explanation, since it cut through the Roman road, must be reserved for later.<sup>7</sup>

The dyke's further course, unchanged, south of Dugard Avenue, for about 475 yds (440 m) has the same lane still along its ditch, and the same lowered profile of rampart, though with more of its inner slope visible against the field-hedge flanking it on the east. On from where breached by the footpath giving access to the lane's south-westward branch towards Stanway Green, this stretch of it ends where the rampart disappears beneath the lane, which bends a little inwards to replace it on its southward line. This allows the margin of its ditch to be seen, along the edge of the patch of

7 This was probably intended for a discussion of Gryme's Dyke Middle. See page 69.

rough ground outside the lane; and here, just at the point from which a downward slope begins, we were able in 1956 to section the ditch. Its dimensions, distinctly less than had appeared beyond Dugard Avenue to the north, were nearly the same as in the section cut in 1977 (pp 109-115), some 240 m (263 yds) south of Dugard Avenue, where the date was proved to be Roman by finds below the rampart. The 1977 section will therefore have been taken through a stretch where the dyke had been left unfinished by its builders, and the intermission then filled up by a replacement at a post-conquest date.<sup>8</sup> And the comparable dimensions found in our section of 1956 may perhaps — although not certainly — imply this too. A reason for the unfinished stretch, as well as the date of this later replacement, is considered below.

### *Gryme's Dyke South*

This, described here from south to north (as it seems to have been laid out), begins at the Roman River at Baymill Cottages, a little upstream from Oliver's Dyke and Layer Dyke. Set a few degrees east of north, and bending slightly more east along the west side of Butcher's Wood, it is crossed by the Maldon Road and thereafter, at 14° east of north, heads straight for the western edge of Stanway Green. What is visible of it at first, south of the wood and from the Maldon Road onwards, is its ditch's inner slope, falling steeply from the forward edge of the nearly-flattened rampart to the level of its partial filling and the path that runs along it. On reaching the SE corner of the largest field outside it (field 281), both slopes of the ditch become visible, and what runs between them becomes a narrow lane, while the path itself takes up a line along the ditch's outer edge. Where its inner side meets the belt of trees in the last of the fields towards Wiseman's Farm, the rampart's whole profile appears, though at less than full height and with the trees standing upon it. Finally the field (283) flattens it out by encroaching to the side of the lane, which now has the ditch underneath it completely filled up. But the rampart, with profile as before, reappears along the west of Stanway Green, with the ditch still filled completely beneath the lane, for about the last 95 yds (87 m) to the dyke's sharp inward turn to run due east. Through this corner passes the lane from across the green, which then, together with the lane along the ditch and with another from the east, issues in Stanway Green Road, heading off north-westwards.

Neither here nor anywhere else has Gryme's Dyke South had an excavation; but its course's continuity cannot be doubted, for the whole length of 1.25 miles (2 km) from the river to the Stanway Green corner. The green's slight depression along the rampart's back, up to the corner and east from it too, need not be ancient, but rather the result of gravel-quarrying to produce material to even up the rampart for tree planting. But

that along the rampart's outer side from the corner eastwards should represent the ditch; and this length of the profile (123 yds or 113 m) ends only at the wider end of the trough in the ground, nearly 90 yds (82 m) long, which fringes the SE side of the green's expanse. Whether that, as on the Royal Commission's 1922 plan (RCHM, 72) and the modern 1:1250 OS map, somehow itself belonged to the dyke, making thus the long side of an earthwork triangle here, may be doubted but has not been tested. Beyond it is a possible fragment of the eastward rampart's back, between the paths that join and continue due east as the single path that enters the lane described above which runs along the ditch-line of Gryme's Dyke Middle. That path approaches the lane through quite rough ground, yet shows no sign of overlying any earthwork, so that the junction between Gryme's Dyke South with Gryme's Dyke Middle is unclear. If the two had never been joined up, so that the final part of Gryme's Dyke South was left unfinished, the result would only match the unfinished stretch of Gryme's Dyke Middle, close by. But while the latter has been proved to have been finished in Roman times (by the section of 1977), there still might not have been a similar completion of an unfinished part of Gryme's Dyke South, on the way from the green.<sup>9</sup>

### **Heath Farm Dyke**

[Figs 2.1, 2.14-2.15, & 5.1]

Our Figure 2.1 is an overall plan of this dyke, combining all three of the parts into which, for convenience of description, we have divided it: South first, then Middle, then North. It is unique among the dykes in several ways, and its place in their relative dating is most important.

### *Heath Farm Dyke South*

Aerial photographs show the ditch of this very short stretch to be 340 m (370 yds) long; it is the innermost, protecting the SW quadrant of the Gosbecks site, running from just above the 100 ft contour beside the stream flowing out of the site, at about S15°W Its southerly part (about 60 per cent of it) appears extremely narrow, as if unfinished, but cuts across the ends of half-a-dozen still narrower lines, passing out from the site's interior, that are boundaries of fields or of longer enclosures. Some of these, being themselves cut across, within the site, by ones which are presumably later, can point to a relatively early date for this part of its system of enclosures — consistently then with the ditch's cutting across their outer ends (Fig 5.1 on p 96). If this stretch of the ditch is as relatively early as the rest of Heath Farm Dyke will be found to be, then the Gosbecks enclosures here behind it should be earlier still.

<sup>8</sup> See p 174 for the view that the whole dyke was post-conquest.

<sup>9</sup> See previous footnote.

Heath Farm Dyke Middle

This stretch of the dyke is very long. The aerial photographs show it protecting all of the northern expanses of Gosbecks, from the start of its ditch about the 100 ft contour on the northern side of the stream; gently curving, from a line a little west of north, and beyond its interruption by the Colchester-Maldon road, till its course becomes N55°E. Directly beyond that road is the Roman auxiliary fort, identified in 1976 from the air (pp 23 & 99-101). Lying in Well House Field, except where the same road crosses part of it, it is placed, and without doubt purposely, still to be close to the

Gosbecks site, but had its west front formed by the rampart of Heath Farm Dyke. Directly in front of the ditch of this is Kidman's Dyke Middle, which from a start just over 800 m farther south (some 870 yds) has followed along its curve, just as already in their south parts. Here it diverges northward, now as Kidman's Dyke North; and this was the point (no 30 on Fig 6.1) where Henry Laver (1902, 370-71) drew his section AB across both the earthworks. The drop from the toe-line of Heath Farm Dyke's bank, to the level of its ditch-fill and the flattened Kidman's bank, was about 1 ft (0.3 m); thence to the bottom of the hollow over the Kidman's ditch, about 2 ft 8 in (0.8 m). Their divergence was

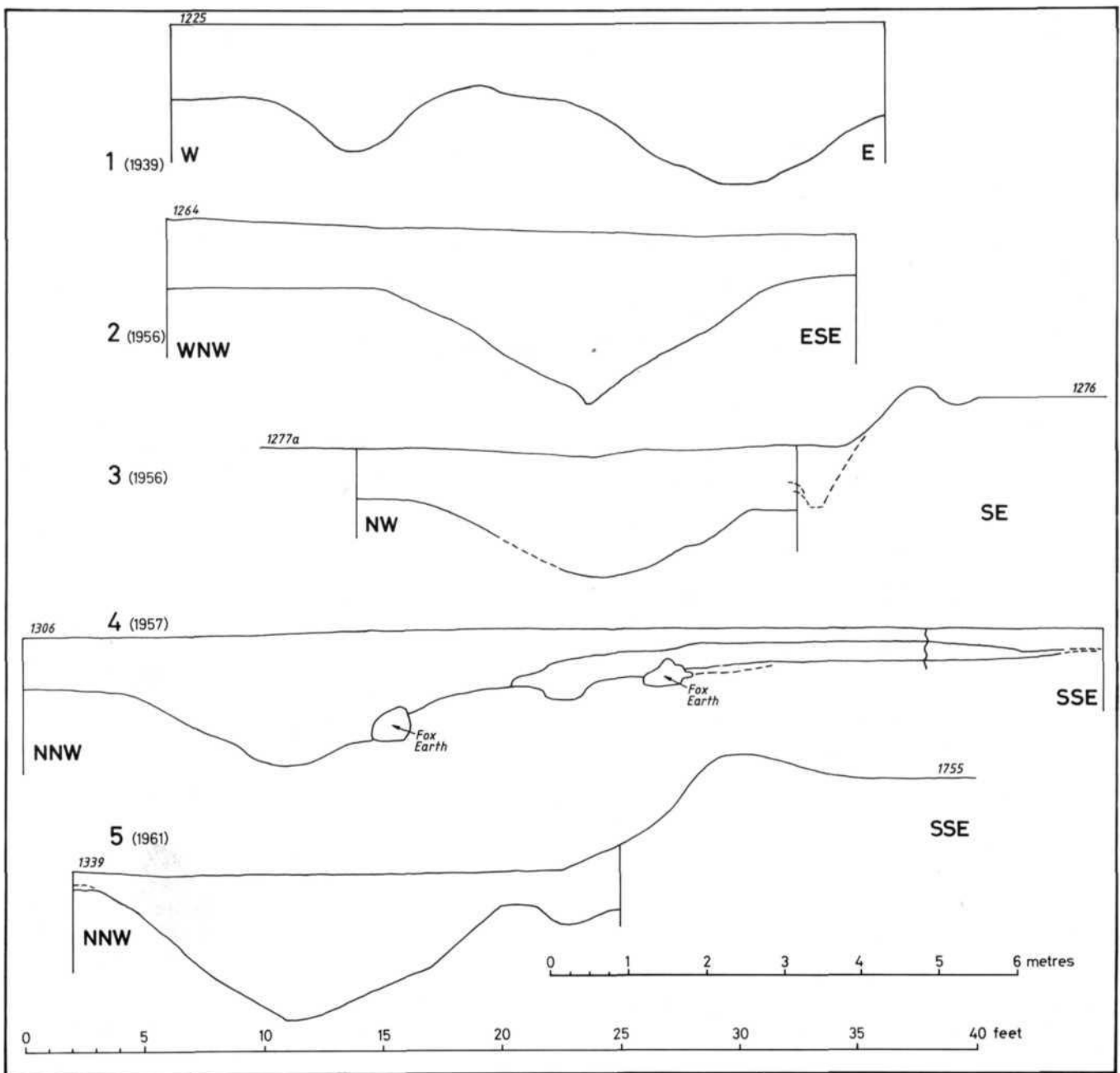


Fig 2.14 Various profiles across Heath Farm Dyke. 1... near Altnacealgach (by M R Hull) in 1939 (no 58a on Fig 6.1); 2... Prettygate Junction in 1956 in field 1264 (no 54 on Fig 6.1); 3... Prettygate Junction in 1956 in field 1277a (no 54 on Fig 6.1); 4... Co-operative Society sports field in 1957 (no 36 on Fig 6.1); 5... 380 ft (116 m) west of Straight Road in 1961 (no 34 on Fig 6.1). [Pages 29-32]



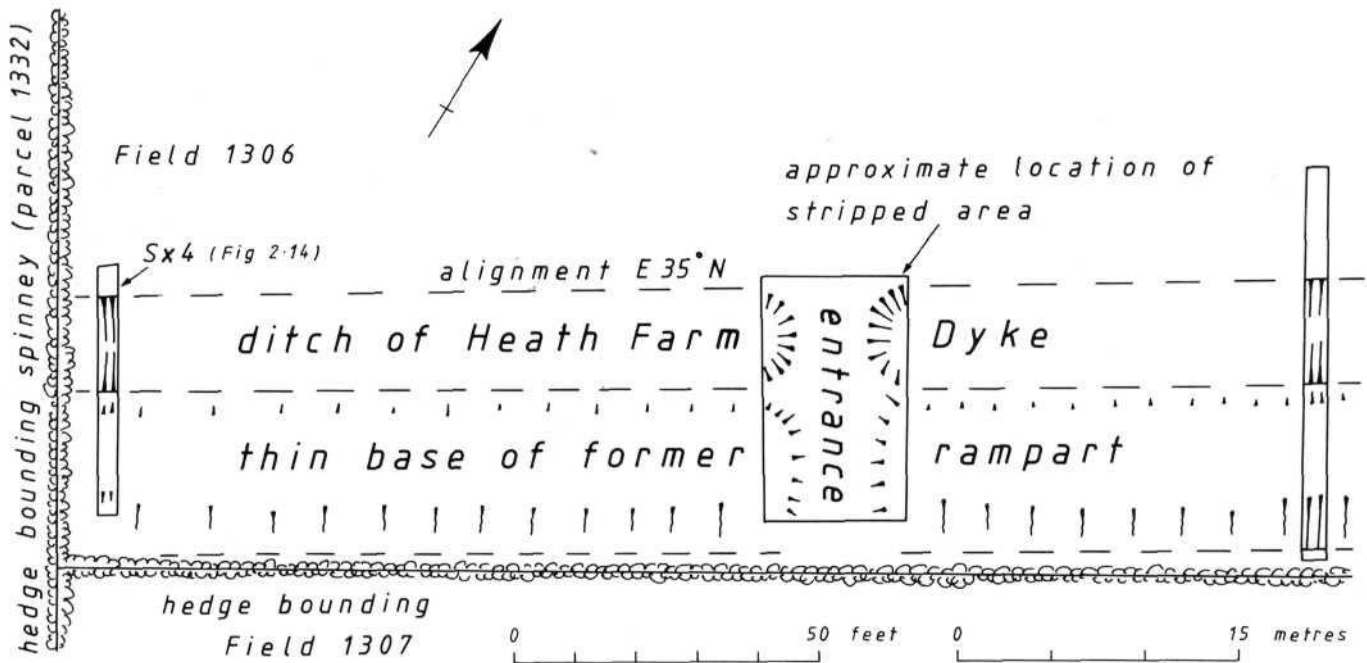


Fig 2.15 Plan of the 1957 excavations of the Heath Farm Dyke on the Co-operative Society sports field (no 36 on Fig 6.1).

gauged, in the 1950s, by Mr A F Hall's cross-cutting at no 25 in our Figure 6.1. For the rest of Kidman's North, see pages 33-4.

Heath Farm Dyke Middle, with bank now a hedge-bank but the ditch on its north still traceable (as by Laver), runs on from there on a course at first E40°N and then E30°N, along the edge of the great sand-and-gravel pit (Shrub End pit) reaching nearly to Lexden Straight Road. In the enclosure of the former Heath Farm, named later New House Farm, it was levelled, and so passed on, at nearly 305 m (333 yds) from Laver's section, into what was the deserted inner farmyard (parcel 1339) still in 1961, when I there laid the section (Fig 6.1, no 34; Fig 2.14, Section 5) across its ditch that I supervised and measured. The line of that was some 380 ft (116 m) short of Lexden Straight Road. Of its depth of just over 6 ft (1.8 m) from the farmyard surface, the lowest 7 inches (180 mm) were very sandy gravel silt, beneath 3 inches (80 mm) of darker sandy old turf. On this lay the modern filling, which had been thrown down from the original rampart and consisted of 16 inches (400 mm) of stony red gravel, covered right up to the surface by loamy material.

Its same straight course, past Lexden Straight Road, and beneath the houses and gardens just beyond, must there have been crossed by Shrub End Dyke, on the southward line confirmed by Brian Blake, from his section cut in 1963 (p 51), a little to the north and in the western end of the spinney stretching eastward from the road (parcel 1332), which slants across the continuing course of Heath Farm Dyke. And towards its eastern end, for about 235 ft (72 m), the dyke's degraded profile appears on the surface — as in Henry Laver's plan of 1902. What bounds this end of the

spinney is the western hedge of fields 1306 and 1307, themselves divided by a hedge-line at 90° to it. As 1306 was the Co-operative Society's sports field, its permission was obtained for an excavation, in summer 1957, to test the line of the dyke here, out beyond the spinney. It was conducted by me with Miss Margaret Smith (my assistant in Oxford 1955-8, thenceforward Mrs Margaret Brown).

Trench 1 (Fig 2.15), across its line along the hedge bounding the spinney, found its ditch where expected, and beside it the remnant of its rampart's loamy base, in thickness never more than about 6 inches (150 mm). The trench could not be long enough to reach the back of this, but its full width of approximately 24 ft (7.3 m) was exposed in our companion Trench 2, and is added from that to complete it as Section 4 on Figure 2.14. The profile of the ditch was identically similar in both the trenches, and the silt and modern filling in both were just as in the Section 5, above described (in the yard of the old Heath Farm). Trench 2 was the excavation's limit, at some 200 ft (60 m) from the hedge; and the ditch's two exposures fixed the alignment of the dyke, at E35°N. The rampart-base's remnant showed as a very shallow rise in the field's level surface, but appeared to be interrupted by an equally shallow dip, around some 80 ft (24 m) back from the line of Trench 2. Suspecting a possible entrance through it here, and thus a corresponding causeway across the ditch, we had a rectangular area stripped. And so it proved, as shown in the plan Figure 2.15. The causeway's minimum breadth was about 10 ft (3 m); that of the passage through the rampart seemed to agree, though poorly defined. As the middle stretch of the dyke was so long, and protected such a big expanse of southward land behind it, this entrance need not have been its only

one, and another is surmised farther on, in the 'Prettygate Junction' area, soon to be reached.

On the rest of Heath Farm Dyke Middle, between this field and its approach to that area, we owe confirmation of its course to Mr A F Hall. In 1950, by the SW corner of the former field 1277, he had seen that a dip in the surface marked the presence of its ditch; and in 1959 began the building-estate development, abolishing that field along with 1277-9. This exposed the ditch, on its same line still, to view by Mr Hall in four places, each recorded by him and measured, its breadth being similar to that in the sports field (no 4 on Fig 2.14). He thus upheld the continuity of the earthwork's middle portion with what next becomes its north one, on approaching the area now named 'Prettygate Junction', but where, long before Hall had been the first to discover what was only explained correctly through our excavations of 1956-7 in this area. These, treated fully on pages 48-50, may here be described more briefly for what they revealed of Heath Farm Dyke North.

#### *Heath Farm Dyke North*

The Prettygate Junction excavations first showed that in the northern part of field 1276, the dyke must have bent from its previous line, at or close to E35°N, through about 7°. For beyond the boundary hedge of the adjoining field 1277a, its ditch was found to be aligned at E42°N (Section 3 in Fig 2.14) where its depth is a bare 5 ft (1.5 m) and its profile shallow. This showed it heading for a point on Lexden Dyke close to the end of Bluebottle Grove, but its line, before approaching that, is crossed by the public footpath, running WSW on what remains of the rampart of Prettygate Dyke. This prevented us from testing our surmise of an entrance through the dyke here, with a causeway interrupting its ditch (which in the nearby section had been found already shallowing). And that point on Lexden Dyke at which it aims is just where Hall, in 1943, confirmed what he had noticed first in 1940, when engaged in the Home Guard on digging a 'tank-trap' trench along Bluebottle Grove through the contents of the Lexden Dyke ditch, namely that the latter cut across the line of a filled-in ditch which passes underneath its rampart. He then believed, as did Hull and I in 1943 when he showed us his exposure of its profile, that this ditch belonged to Prettygate Dyke; but all were convinced by the work of 1956 that in fact it was unquestionably Heath Farm Dyke's. So this was the critical proof that Heath Farm Dyke had been prior to the defences that here included Lexden Dyke. The work of 1956 was then extended into field 1264 behind the back of the Lexden Dyke. The section that was cut there found that the ditch, in passing under the rampart had bent, again through 7°, to a course at N41°E, and was filled in as before. This section (no 2 in Fig 2.14) shows its profile to be broadly V-shaped, and at its middle more than 7 ft (2.1 m) deep from the surface, with silt and filling much as in Sections 4 and 5.

From the rest of the 1956 work, and that of 1957, south of the ending of Bluebottle Grove (showing Heath Farm

Dyke prior also to Prettygate Dyke), we turn to the course of Heath Farm Dyke North farther on. In September 1957 it was pursued by Mr Hall, in search-trenches in the same field 1264. These showed its course there bending slightly, and finally through 8°, had settled at N33°E; and its keeping to this in the next field, 1265, he observed to be shown by a cropmark crossing this, not quite straight but resuming, at its farther end, that same alignment. This led him to the next field northward again (field 1225), lying west of the property of Altnacealgach House. And that was where Hull, in 1939, had found a ditch on just that course, there with a smaller ditch beside it, giving Section 1 in Figure 2.14, which is a reverse-drawn copy of his own, figure 117 upper, shown on plan in figure 116, in *Roman Colchester* (pp 271-2, site W 17B). At W 17A, he had the typically Roman-style ditch of the early fort discussed below (p 62), but the W 17B profiles are not at all Roman, and the larger one is quite in place among the Heath Farm Dyke profiles, as our Figure 2.14 shows. Hull acknowledged Hall's cropmark in field 1265 (*Rom Col*, 273, n 2), and had apprised him of both these findings in 1225. So Hall, in 1957 while *Roman Colchester* was still in press, cut search-trenches across the line of the W 17B profiles, in 1265 but as near them as possible, along the hedge dividing the fields.

Hull had thought that the smaller (western) ditch could be ending against the hedge: rightly, for Hall found nothing beyond on the line of its cropmark. The larger ditch's continuity with Heath Farm Dyke, however, was obviously proved. The smaller ditch in 1225 might have been added, as a strengthening element, along the last stretch of the dyke, if approaching an end for it. It seems in fact that both will have ended where (or nearly where) their course would be crossed at 17A by the ditch of the Roman fort, for Hull had cleared enough of this to prove that they were absent inside it. No farther from the boundary hedge than about 125 yds, or 115 m at the most, we may be sure that Heath Farm Dyke had been brought to an end. Its stopping unfinished can seem less likely than its ending where intended, whether or not for an interruption with a resumption somewhere beyond. Since its course was approaching the flank of the assumed main trackway, east-west between the waters of the coast and the country inland, the expected width of this could in either case explain the ending here.

#### *Heath Farm Dyke: discussion*

From its start to this ending, the length of Heath Farm Dyke is almost exactly 2 km (1.25 miles). This is not to gainsay what is obvious from Figure 2.14: the variations in the profile of its ditch in the different places. While the bottom in Sections 1, 3 and 4 is rounded, in Section 5 it is nearly V-shaped, and in 2 quite sharply so. The variations — and there may have been many more where we cut no sections — imply that the working-parties employed on each stretch of the ditch could have different notions of how much work was required for a tolerable finish, or how fast they needed to labour before being told that enough was enough.

Thus the whole dyke must have been left with its finish uneven; this distinguishes it further from all that we know of the dykes that are seen to be later. (Gosbecks Dyke, noted above as being probably or seemingly earlier, is at present still known from aerial photographs only.) Its unevenness is no sufficient reason for believing it was left unfinished at its northern end; but whatever the truth about that may be, the ditch's varying forms do give the impression of a work done hastily, or at least with resources less than should have been demanded by its length. For while Gosbecks Dyke served only the Gosbecks site, Heath Farm Dyke protected an expanse 2 km long, and no such earthwork in Essex can be claimed to be older. Its priority to Lexden and to Prettygate Dyke, and its difference from every one both in length and in direction, must point to its being the first such earthwork at Colchester. Laid out over country free of anything else of the kind, hardly military but plainly a protective barrier, its uneven execution would be consistent with its novelty, at any rate to most of the men who carried it out. Any older tradition of linear earthwork belongs, in southern Britain, to regions elsewhere; and its closest match is in Hertfordshire, one of the works of pre-Roman Verulamium.

This, more regular in workmanship, though somewhat smaller in scale, is the linear earthwork laid along the brow of the rising ground, above the river and the Roman town-site west of St Albans. Discovered in Mortimer Wheeler's excavations of the early 1930s, it was no more than 13 ft (4.0 m) wide and 5 ft (1.5 m) deep, but its total length was some 1700 yds (60 short of 1 mile), just over 1.5 km (1555 m). Curved back to a hook at its NW end, in Prae Wood, and doubled along the curve to protect a contemporary settlement-enclosure there, its whole protected terrain which had been cleared from woodland, divided behind by an intermediate and a terminal palisade. Through it, some 90 yds short of the terminal, was an entrance, the only one found. Wheeler's description (Wheeler & Wheeler 1936, 40-46) used the settlement-enclosure's pottery and other finds (*ibid*, 151-81) to link these works with the Verulamium ruler Tasciovanus (*ibid*, 6-9, & 151-81; pp 6-7 above), still quite early in his reign, so hardly as late as 10 BC. Though Frere's excavations (Frere 1972, 1983 and 1984) have altered and enlarged much of the rest of what Wheeler propounded, they have not changed this, nor the parallel therefore with our Heath Farm Dyke. In position, the Gosbecks and the Prae Wood sites are clearly similar, behind one end of the earthwork guarding extensive lands for each. In date there may also be a fairly close accord.

One further point should be noted. Heath Farm Dyke, in the whole of its course from Gosbecks onward, disregards the Lexden Tumulus and Lexden cemetery. Whether they existed already or not, their area was ignored by its designer.

### Kidman's Dyke

This owes its name to Mr Hall, from the name of the friendly Mr Kidman who allowed him to explore it across his fields. But its start is far southward of those.

#### *Kidman's Dyke South*

From its start at the south of the Gosbecks site<sup>10</sup>, on the brow of the valley-slope there, its course is north-westward but bends very nearly north, having the whole curve of Gosbecks Dyke South in front of it. After the bend and for as far as the dip to the stream flowing out from the site south-westward, it has Heath Farm Dyke South close behind it (already existent: see above on this page). It thus reinforces the protection against the west of the southerly part of the site. Of the narrow boundary-lines internal to the latter (pp 98-99), those that cross Heath Farm Dyke South stop short where they must have met the bank of Kidman's South —thus agreeing with the facts farther on which show that Kidman's is the later dyke of the two.

#### *Kidman's Dyke Middle*

This, with Heath Farm Dyke Middle closely behind it, so determining its course, begins directly north of the dip to the stream, and gently curves, along the line of the modern hedge-bank, past the early Roman fort (pp 23 & 99-101) until, at the NW corner of the Gosbecks site, it leaves Heath Farm Dyke (which then runs ENE) and turns nearly north.

#### *Kidman's Dyke North*

Its initial nearly-northward course —visible as a fold in the ground and confirmed by three transverse cuts of Mr Hall's in field 1338 (Fig 6.1, no 25)<sup>11</sup> — curves away across the next field in an arc which brings it up, at nearly 90°, against Lexden Straight Road, but not visibly indeed, but under one of the modern houses, each in its garden, in a row along the road's west side. As the similar row along the east covers Shrub End Dyke, Kidman's has evidently ended abutting against it — though whether or not with rampart built across its ditch, that row of modern property prevents our knowing. Farther back along its curve, we cut in 1957 a section through it (p 120, no 28).<sup>12</sup>

The placing of Kidman's in the sequence of all the dykes (pp 52-5) follows jointly from its ending on Shrub End Dyke, thus shown as the older, and its start protecting Gosbecks as older still. Shrub End Dyke's congruence of layout with Lexden and Sheepen assigns it to a Colne-based system, excluding Gosbecks — which is now, however, brought back into

10 The OS marks a short stretch of earthwork in Walk Wood (Fig 6.1) which looks as if it was part of Kidman's Dyke.

11 Also the work in 1973-6 in fields 1333 and 1303 (pp 116-19, Fig 6.7).

12 CFCH mentions on page 23 a section cut in 1961.

connexion with that, by means of Kidman's. And north along Shrub End Dyke some 94 yds (85 m) from where Kidman's has reached it, the connexion was next carried further.

### **Layer(-de-la-Haye) Dyke**

Our name for it is that of the village beside its end. Just opposite the terminal tump of the Oliver's rampart, and starting from the Roman River's ford there, Layer Dyke prolongs the southward line. Crossed by the lane running on from the bridge just mentioned, it enters Chest Wood. Up the gently-rising slope that this covers, and flanked at first by one of its internal boundaries, it runs through the more than 400-yard breadth of the wood (about 370 m altogether) to where the southernmost patch projects, then along the west edge of this, turned slightly to east of south, to the hedge between fields of Wiseman's Farm. Thus it reaches the northern tip of the enclosure round the vicarage of Layer-de-la-Haye.

It can easily be recognised through the wood, and as the slightly-raised band of ground along the length of the field-hedge (discernible especially, as a remainder of its rampart, when a field is under plough). But Brian Blake, in 1961, surveyed the whole of its course, from the river to the vicarage enclosure and sectioned it in three places.

We have separated Layer Dyke from Oliver's not only because the Roman River divides them. Since Gryme's Dyke, the outermost of the westward-facing works, has no resumption beyond the river, Oliver's too might have ended there for a time, before the addition of Layer Dyke. So too, farther east and facing eastward, might Berechurch Dyke before what would then be its own addition, Abberton Dyke; and this is why we have also separated those. Even Moat Farm Dyke might perhaps have been a later addition, north of the Colne, to Lexden Dyke. It is at least equally possible, indeed, that all three trans-river works were coeval with those whose lines they prolong, but as there seems no means of deciding with positive certainty, the separations that we have made should be seen to be prudent.

### **Lexden Dyke**

#### *Lexden Dyke North*

The dyke, between the Lexden Road and the northern by-pass, a distance of only some 230 yards (210 m), could in its northern part be discerned on one of the 1930's aerial photographs. The line of it simply prolongs the line on the south of the Lexden Road, so that the intermediate stretch may be safely assumed. The course of the dyke runs, very nearly due south, to where the modern road from London, on the line of our Roman Road 1, makes its slight change of direction up

Lexden Hill—which being originally Roman must show that the dyke had an entrance here, though the modern road's running in a cutting has engulfed it altogether. We have used it to divide the North sector of the dyke from the much longer Middle one, which begins, though defaced by gravel pits, in Lexden Park directly opposite. The existing North sector starts from the wet flood-plain of the Colne, on its edge below Lexden Bridge; but on the opposite edge, picking up the alignment, is the point at which there begins a prolongation, Moat Farm Dyke, which we shall follow from that point outwards.

#### *Moat Farm Dyke*

This earthwork is so named from its passing very near to Moat Farm, the square and water-filled moat of which, implying an older farm or grange, is certainly not, as the Ordnance Survey stated till at least 1921, 'on the site of a Roman fort'. The dyke, aligned approximately NNE, has not had any excavation, but its purpose is unmistakable: the prolonging of Lexden Dyke North beyond the Colne.

In its southernmost stretch of nearly 440 yds (400 m), it still has a standing rampart, of height somewhat lowered, as far as Moat Farm. From thence, it extends northwards, up to its crossing by the railway and beyond, for a total of some 530 yds (480 m), with the rampart levelled. However, in the Royal Commission's map from survey shortly prior to the 1914 war but published 1922 (RCHM, 72), it was marked as existing as far as the sharp right-angled turn in Baker's Lane, and as ditch along the western edge of the lane's short length beyond, thus reaching the road to West Bergholt at just one mile from Colchester Station. Untraceable any farther, an end for it there could be explained if on the southern edge of woodland, natural growth on the loams as marked on the frontispiece map in *Camulodunum*. This will make its length about a mile and a third or some 2,346 yds (2.11 km). The profile of the rampart and ditch of the Moat Farm Dyke can best be seen at its cutting through by the railway, where seems to have been in general similar to Lexden Dyke's.

#### *Lexden Dyke Middle*

[Figs 2.16-20]

Much of the middle sector of Lexden Dyke is in Lexden Park, nearly all of it aligned approximately N58°E. North of that, and north of its interruption by Lexden Road, a short sector prolonging its line may be inferred from air-photographic indications. Within this sector there will no doubt have been an entrance. The middle sector of the dyke ended where crossed by the Colchester northern by-pass road, as was observed in 1930 when the road was under construction. Whatever then the course, the stream running east to the Colne from Lexden Springs is directly beyond, being here at the edge of the narrow band of flood-plain opposite a ford, which no doubt represents an ancient one. North of this

there is alluvial marsh, and northerly flood-plain. Exactly where the flood-plain begins, the line of dyke is visibly resumed at N58°E, by Moat Farm Dyke.

Lexden Dyke Middle runs nearly north-south through Lexden Park, where there are certain interruptions in its otherwise fine preservation. Here, in Lexden Park, took place the excavations of 1932 (Fig 2.16). The park's north fence bounds a stretch where it cannot be seen: between the Lexden Road cutting (no doubt destroying a major entrance), for the 50 ft (15 m) distance to the fence, it has been levelled. At 360 ft (110 m) south of the fence, a large gravel-pit has cut into it, for nearly 63 ft (19.2 m). But from the fence to the northern edge of the pit, and beyond its southern edge for a measured length of 250 ft (76 m), no rampart can be seen: the dyke appears simply as a steep escarpment, along the 100 ft contour flanking the valley that runs through the park, northward to Lexden Springs and so to the Colne. The 250 ft (75 m) length is ended next by a narrow gap, some 12 ft (3.7 m) wide at bottom and suggesting an entrance, approached up the valley-side and seeming quite probably original. From the middle of the gap's bottom south for the next 500 ft (150 m), the rampart, rising steeply, is well preserved; so too is the ditch although towards both ends its outer lip has an inward curve, due possibly to modern encroachment. The ground in front rises, forming a bluff which projects into the valley. The southern side of this recedes into a transverse natural gully, about 32 yards (29 m) wide, where the dyke sinks down in what appears on the surface as another interruption. Finally, onward to its levelling, right in the SE corner of the park, where it has bent to an alignment heading SSE, is the further 150-yard (46 m) stretch in which its profile is better preserved than anywhere else, with ditch deep and wide and rampart standing nearly to what will have been the original height. The full long section through it here, cut in 1932 just past the stretch's middle point, disclosed that the gravel of the rampart had been thickly faced with turves, and further reinforced along its foot with what must have been timbering, retained by posts set in holes in the margin of its berm, of which the section came on one, full of the carbonised wood of its post (pp 37-40, Figs 2.17-2.19).

The remainder of Lexden Dyke Middle is the stretch running approximately S35°E, picking up from the above-mentioned bend within the park, which is visible, on from its crossing by the lane that leads west to King Harold Road, and all along the quarter-mile length (400 m) of the tree-belt known as Bluebottle Grove. Its rampart has been lowered, and carries a footpath. Into its partly-silted ditch was cut, in the war-time of 1940, one of the steep-sided trenches for hindering possible enemy forces and thence called 'tank-traps'. How in this, near to the end of the grove, Mr A F Hall found it crossed by the filled-up ditch, running on beneath the rampart, identified later as that of Heath Farm Dyke, has been recorded above (p 32). And at the end of the grove itself, after a gap where must have been an entrance, Lexden Dyke Middle was found to be supplemented by Lexden Dyke South.

## EXCAVATIONS AT LEXDEN DYKE MIDDLE, 1932

The excavations were done in August, after the survey of the whole dyke-system had been made by Mr Poulter and Miss Cruso (p 22). She thus was free to become the site-supervisor that I needed to have for this work, being myself in charge at Sheepen. My intermittent visits let us decide on the placing of trenches, discuss the sections that they showed, and agree on their significance. Their purposes were twofold: firstly, to examine the rampart's construction, and explain the part of it that seemed no more than an escarpment: secondly, to prove the existence or non-existence of entrances through it. Thus a complete section was cut across the rampart and ditch where fully preserved (Section 6 at Site 4), and a long section was dug across the escarpment (Section 3 at Site 1); the gap on the south of the latter was examined in a series of trenches, giving Sections 5 and 7-11 (Site 2); and those at Site 3, the seeming interruption farther south, gave Sections 2 and 4. Figure 2.16 is a plan of the whole.

The proprietors concerned gave ready consent, and I gratefully here record them: Mr D Locke for Sites 1 and 2, Mr H Clarke for Site 3, and Mr S Blomfield (Councillor Blomfield) for Site 4. Miss Cruso herself did the measuring and drawing of the sections, of which one is here redrawn as Figure 2.17. Outstanding among her workmen was Mr Thomas; formerly a miner at a South Wales colliery, he cut the complete Section 6 across the dyke at Site 4, with only a single assistant. Figure 2.18 shows him standing in Section 6 which is still the most complete section through any of the dykes.

The sections and sites are numbered in their order of starting, but to keep the work's two purposes distinct, Site 4 and then Site 1 will be treated here first, since they both bear on the rampart's mode of construction. Then follows Sites 3 and 2, each being at an apparent interruption for an entrance.

### *i) Site 4: Section 6 through Lexden Dyke where best preserved*

[Figs 2.16-2.19]

This, which was the only section on this site, was cut across the full breadth of the dyke in the stretch in the south of the park where it stands at its best and where it was sufficiently free of trees. The trench was cut 3 ft (0.9 m) wide, and measured on its south face; thus Figure 2.17 shows the section reversed, rampart to east and ditch to west. The natural ground was sand, compacted firmly; its surface under the rampart was bare of turf, having doubtless been stripped. The bottom of the ditch was found to be a little more than 27 ft (8.3 m) below the present crest of the rampart; the maximum silt-depth was 7 ft (2.1 m), the lowest 15 inches (380 mm) at most sandy-gravel rapid silting, and the rest brown mould. There was no sign of any outer rampart. The ditch's profile is a widely-splayed V, with a sharper plunge for the bottom 30 inches (760 mm); the

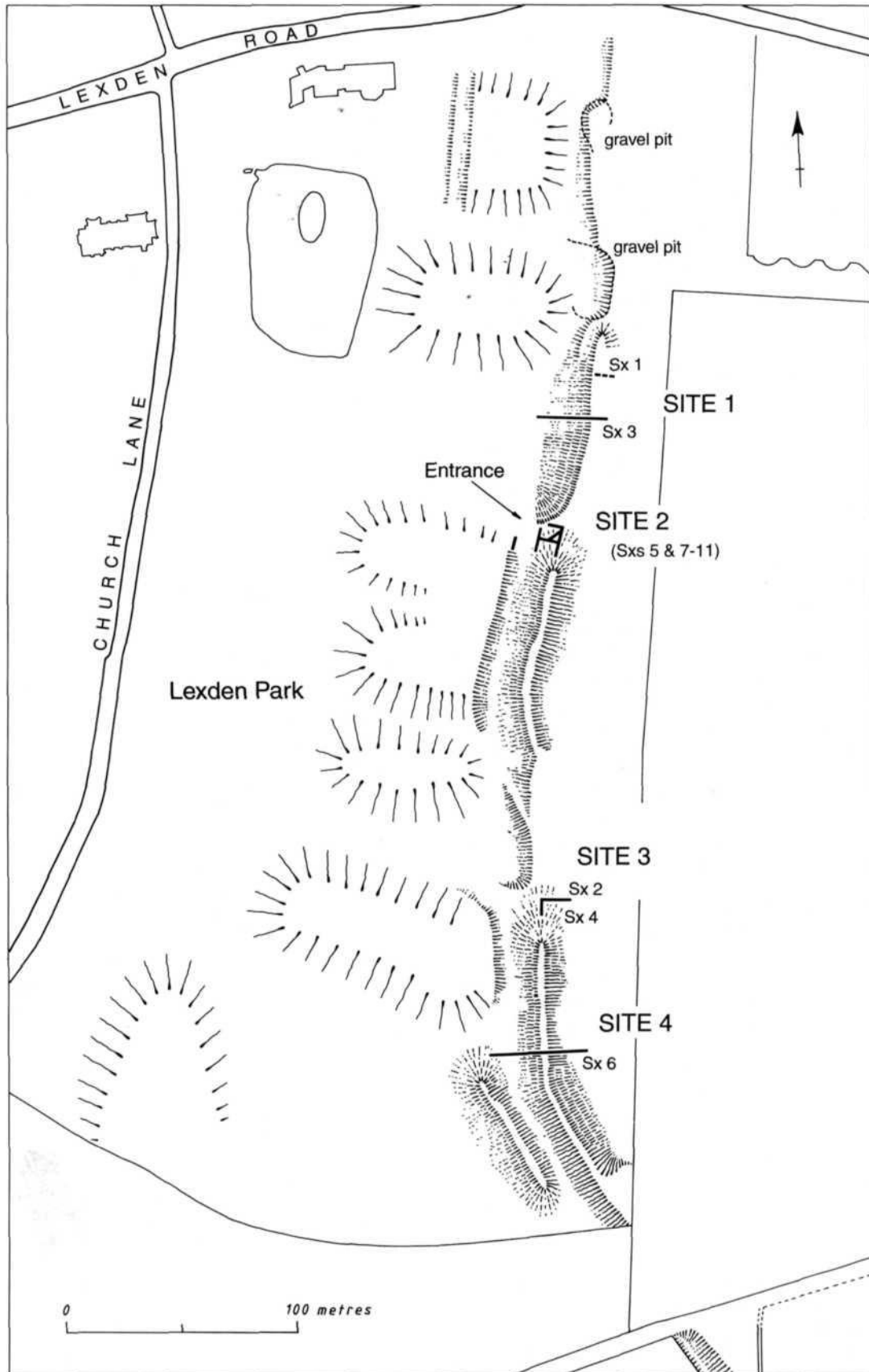


Fig 2.16 General plan of the 1932 excavations of the Lexden Dyke in Lexden Park. The plan is based on CFCH's descriptions of the locations of the trenches, supplemented by Fig 2.21 and a sketch plan in his archive on which he roughly indicated the large mounds to the west of the dyke. The locations of the trenches are not necessarily indicated accurately. (No 53 on Fig 6.1.) [Pages 35-45]

present slightly-concave form of the sides must be due to erosion: that this has also affected the lips is shown by the presence of a big cylindrical post hole, 2 ft (0.6 m) in diameter (Fig 2.19), some 4 ft (1.2 m) over the edge of the inner one. This had clearly held a post for a revetment of the rampart's front, standing close to the lip of the ditch; if a level surface here were preserved, the hole would be 4 ft (1.2 m) deep, but the lip's erosion was evident and had allowed rampart-material to slip down over the remains of the post. The original breadth of the ditch from lip to lip must thus have been some 44 ft (13.4 m), and its depth from the old surface line 15 ft (4.6 m). No finds were made in the silt. The rampart was in the main composed of gravel. Its present-day crest stood 10 ft (3.0 m) above the sand surface-line, throughout clearly traceable beneath; to the effect of erosion some flattening must be added from the modern footpath along it, and 12 ft (3.7 m) may pass a minimum guess for the original height, and it could well have been 13 ft (4.0 m). Its tail was found stretching over a fold in the material ground, to taper away at a measured 70 ft (21.3 m) from the frontal revetting-post. A sharp slot, cut in the natural sand at just this point, 15 inches (380 mm) across and 21 inches (530 mm) deep, marked the beginning of a further downward slope of the firm sand, underlying the soil that from the crest of the rampart ran down to cover its tail and the slot and on beyond. At 6 ft (1.8 m) past the slot began an intervening layer of a sandier brownish soil, running farther and thickening; the soil still above it, after a further 7 ft (2.1 m), began to be covered by a spread of modern gravel, but the trench was cut farther, for 74 ft (23 m) beyond the tail of the rampart, to trace any possible quarrying of material for the rampart. The firm sand surface indeed sloped on, to a flat bottom 3.5 ft (1.1 m) deep from the top of the modern gravel, and in the sandier brownish soil appeared a trough, scarcely less deep and 25 ft (7.6 m) broad. Beyond it, the firm sand sloped up again, and the brownish silt only ended 4 ft (1.2 m) from the end of the trench, but whether there had been quarrying for rampart material in either could not be ascertained. Finds were absent. Of far greater interest is the structure of the rampart itself.

Directly behind the post hole (still with remains of the post) at its front, the rampart's remaining forward site was found to have consisted of turf. This was clean and pale grey, often flecked horizontally with thin darker lines, representing the humified surfaces of sods, cut and laid in level courses. Doubtless they had been cut from the whole expanse that the dyke was to occupy, for the total amount of this turfy material was great. It covered the whole of the forward slope to which erosion had reduced the rampart's profile, and had split, as noticed already, over the ditch's eroded lip, the edge of which was found 3.5 ft (1.1 m) behind the post hole. Here the turf, under shallow humus, was 3 ft (0.9 m) thick; and beneath it, lying directly on the level of natural sand, began a sloping bank of gravelly-sandy material. This was too loose to have stood without protection, so that the purpose of the turf had been to retain it. Behind and beneath, however, from 12.5 ft (3.8 m) farther in for

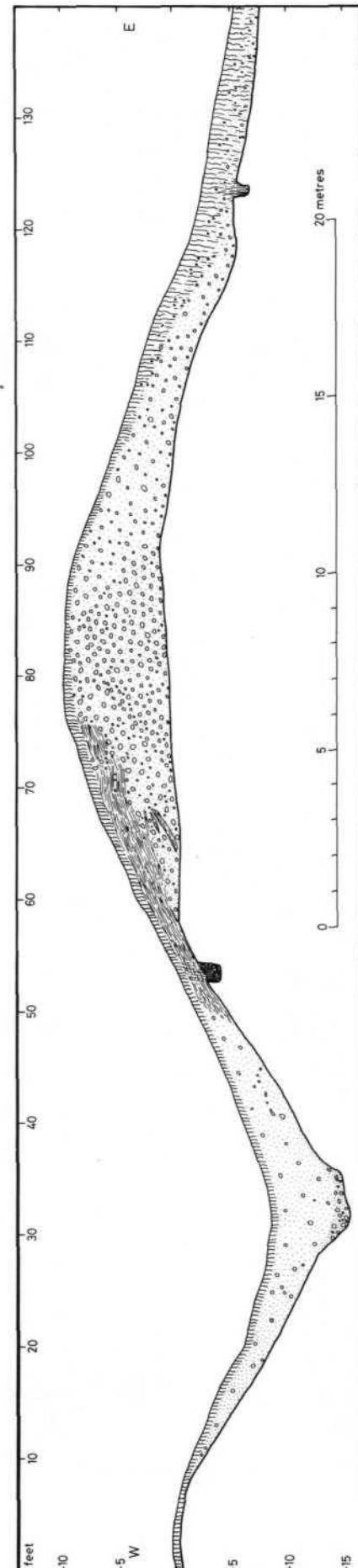


Fig 2.17 Section across Lexden Dyke Middle. Dug in 1932 (Section 6, Site 4). (No 53 on Fig 6.1.) [Pages 35-40]



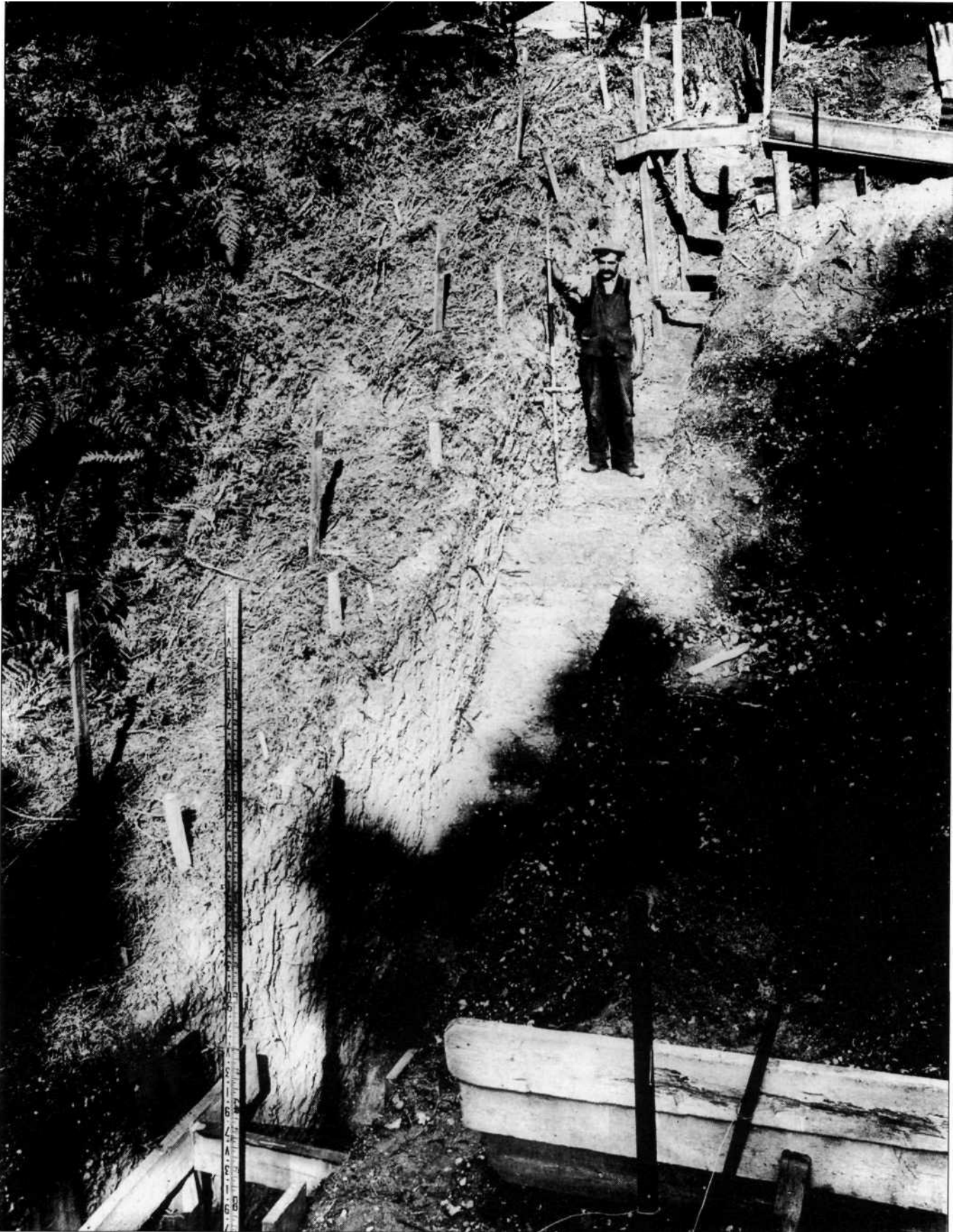


Fig 2.18 Section across Lexden Dyke Middle. Dug in 1932 (Section 6, Site 4). The ranging rod, marked in feet, is held by Mr Thomas. (No 53 on Fig 6.1.) [Pages 35-40]





Fig 2.19 Section across Lexden Dyke Middle (Section 6, Site 4), showing on the right the charred post which formed part of the revetment of the rampart. (No 53 on Fig 6.1.) [Page 37]

a further 21 ft (6.4 m), ran the base of a rampart-core of pebbles without much sand, built up as a solid bank standing 8 ft (2.4 m) high, with a forward slope of some 40° (lessening to 20° on approaching the top) and a nearward slope steeper (some 70° near the base, 35° towards the top). The gravelly-sandy material covered all of this core: still 1.5 ft (0.5 m) thick at the top, under 6 inches (150 mm) of humus, and nearwards sloping gently (only once with a short streak of turf to the tail of the rampart, with the slot along it, crossing our trench, as already described). If a fence had bounded it, set in the slot, this could arrest erosion but would scarcely aid the solidity of the rampart: very different was the construction that retained it in front.

The gravelly-sandy material there, tipped evidently up from the direction of the ditch, had had turf spread on its lowermost slope already during construction, and a smaller spread very soon after. When its base was finished but its height was still a little less than 2.5 ft (0.8 m), a level spread was laid on it, running back from the turf mass laid over and in front of it, and when its height had reached 4.5 ft (1.4 m) its surface was laid back level, and turf was piled on it to an additional height of some 3 ft (0.9 m). Three of the layers of sods

were particularly visible, and inwards there were slants of turf again on the rising slope; but the turf above these had its inner edge vertical and sharp, with some gravel behind it very loose for a foot farther inwards. There the thickness of turf was resumed at 3 ft (0.9 m), laid on a steepening gravel slope to taper out just under the rampart's forward crest. As for the vertical edge, which was 5 ft (1.5 m) forward of that, it had possibly been a construction-feature only; but perhaps it had stood against a vertical wooden fence, later removed and letting its emplacement fill with the gravel that we remarked as being very loose. If so, and if continuous along at least this stretch of the rampart, one could fancy it carried up to form a breastwork; but the certainty is of course ruled out not only by our trench's having been so narrow, but by a surface-erosion so great as is shown by the volume of the silting in the ditch. This consisted very largely of fine dark mould which was nearly free of gravel pebbles and thus must have been mainly of disintegrated turf. The turf structure, anyhow, served to solidify the rampart's front, and also, being not so heavy in mass as the sandy gravel, to lighten its weight against the outer timber revetment, implied as we believe by the post hole discovered, and

thus as near to the lip of the ditch as was compatible with safety. This might have been low, revetting just the rampart's foot; but the turf behind it, much steeper in slope than its eroded surface is now, might have risen to a platform top.

In this the imaginable breastwork might have been set; and if that appears too much to believe throughout a dyke's full length, it may be credible at least for particular sectors. Between the park's south fence and this dyke's next stretch, along Bluebottle Grove, the wide modern gap may conceal an original entrance similar to that presumed on the line of Lexden Road. On either side of this, say for a length of 300 ft (90 m), a breastwork would aid its defence against possible assault. But such a conjecture can only stem from what was observed in our single section: the turf's vertical edge and the very loose gravel directly behind interrupting it nearly at the top of the rampart's present day forward slope. For such a feature to be merely an internal one does appear strange. But an original platform top for the turf, whether or not with a breastwork set in it, seems credible in itself. It would simply be a practical improvement on the profile of an ordinary 'dump-construction' rampart, suiting especially one where the core was too loose to be left without a solid front of turf.

*ii) Site 1: in the sector where the dyke has been reduced to an escarpment*

Two sections were cut, both at right-angles to the north-south line of the escarpment, and at distances measured northward from the middle of the interruption at Site 2 (ie Section 1 at 200 ft (60 m) and Section 3 at 140 ft (43 m)). Section 1 was a short preliminary trench to test the ground where the rampart ought to have stood. On discovering a thickness of gravel remaining from its base, we closed this trench and started on Section 3.

Section 3 was cut through a 30 ft (9 m) length of the rampart's base, and found it laid on almost level natural sand. This had been cleared of its turf except in one place, between 7.25 ft (2.2 m) and 10.5 ft (3.2 m) from the eastern end of the section. The section's total length was 98 ft (some 30 m) from this end to the foot of the visible escarpment, which was 18.25 ft (5.6 m) lower than the modern surface above it. The humus beneath that surface was in average thickness 1 ft (0.3 m) and the sandy gravel of the rampart base below it, undisturbed, varied from 1.75 ft (0.5 m) at the trench's end to a mean 2 ft (0.6 m) farther along, being left with a slightly uneven top by the evident removal of that above it. Between about 5 and 17 ft (1.5 and 5.2 m) from the end, it lay on a shallow layer of sand that could hardly be distinguished from the natural, but seemingly had been dug from this but never cleared away, since it overran the length of natural turf above remarked on. Thus the laying of the rampart-base here had been a little less careful than in Section 6, and nothing could be seen, as there, of any core of harder gravel.

Where the slope of the present-day escarpment began, at 20 ft (6 m) from the trench's eastern end, the thickness of the base was nearly 3 ft (0.9 m) But its surface had been cut down steeply for the next 5 ft (1.5 m) along, reducing the thickness to not much more than a foot, though the natural sand beneath it was itself sloping down. This flattened out again for a further 3 ft (0.9 m) to where the steeper slope of the original escarpment started; but where it still lay flat, so too did the surface of the rampart-gravel over it, forming a shelf. Though some of this might have been scraped down a little by the work's eventual destroyers, it had doubtless been originally laid as a seating for turf-work. While the build will somewhat have differed from that in Section 6, and our trench disclosed no trace of any timber revetment, the gravel was certainly sandy enough to have needed retaining by turf. And over the shelf, the soil was 2.5 ft (0.8 m) thick, enough to suggest that not all of it was cumulative humus, but some had grown over a residue of rampart-turf. This indeed would have been too slight to have resisted humification once the rampart was destroyed. But an original turf structure was still to be supposed.

When the trench was carried down the slope for a further 70 ft (21 m) right to the natural, that supposition became a certainty. The natural sand had been scarped for a distance of 37 ft (11.3 m), to a bottom 18 ft (5.5 m) from its level beneath the remains of the rampart and 11.5 ft (3.5 m) from the present-day slope directly over it. The bottom was 5 ft (1.5 m) broad, and for 16 ft (4.9 m) beyond it the sand had been scarped more gently up again, till its natural level was resumed at 3.5 ft (1.1 m) below the surface becoming 2.5 ft (0.8 m) at the foot of the present-day slope 12 ft (3.7 m) farther on. Thus the ground's original fall to the valley, about 1 in 4 (13°), had been steepened by scarping to 2 in 5 (25°), offset beyond the bottom by counter-scarping at 1 in 5 (9°) to make a shallow lop-sided version of a normal ditch. Filling its bottom, for a length of 19 ft (5.8 m) and to a maximum depth of 3 ft (0.9 m), was rapid silt, consisting of a reddish gravel. Over that, from the top of the scarp to the middle of the counterscarp, was a layer of dark soil, running to a maximum thickness of 5 ft (1.5 m). And while this must at first have been a growth of natural humus, its thickness should mean that, though it showed no stratification, there had been thrown on it soil from the rampart consisting of humus and humified structural turf. Of the overlying tips, the lowest was similar but dirtier. Next came gravel, then sand and further gravel, and then alternations of sand, earthy material and yet more gravel. Further earthy material covered the whole, passing up into the humus of the present-day slope. The lowest of these tips were as plainly derived from the rampart as the layers of sand and the gravel with which they were interleaved. As comparison with Section 6 makes clear, they had come from the structural turf-work of the rampart. Evidently all this tipping was the work of its destroyers.

When and who were they? Only a single datable object was found. It was embedded in the top of the last tip of gravel, at 1.5 ft (0.5 m) from the surface of the present-

day slope, 22 ft (6.7 m) down it. It is a Roman bronze plate-brooch (Fig 2.21), dated by Hull to the time of Trajan, or to c AD 100.<sup>13</sup> The brooch cannot have reached its position until the last of the gravel had been tipped there, yet the object was in the gravel and not over it, where the earthy material seemed all naturally found. But the gravel might still have been loose when deposited, and the brooch could have sunk into it later. If this were the case, then the destruction of the rampart could be earlier than 100. There are various historical possibilities for such an event prior this date, the earliest being the morrow of the conquest. The latter, at the time of the excavation, was our belief. But the question cannot be adequately answered on the strength of this brooch and its position. When we pass below to Site 2, we shall find that pottery found there, in our Section 5, can bear upon it also. Discussion of it is therefore held over, and will be found on pages 44-5. We have to turn meanwhile to the excavations' second purpose: to explain the apparent breaks, inside the park, in the dyke's continuity.

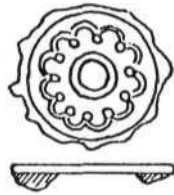


Fig 2.20 Roman copper-alloy plate brooch found in 1932 on Site 1 of the 1932 Lexden Dyke excavations. Drawn by M R Hull. 1:1.

Hi) Site 3: at the seeming interruption having its middle 210 ft (64 m) north of Site 4

This apparent break in the dyke was reckoned as 100 ft (30 m) in width. The rampart on the south stands high, so falls to it steeply; that on the north more gently, and with the ditch in front of it narrowing. At the other end of this stretch it narrows again, towards Site 2, and throughout that length has its outer edge bordered by a shelf, set slightly down from the bluff of ground beyond, which the landscaping of the park had made an artificial plantation of trees, having the step down to the shelf as its eastern boundary. Thus the narrowing of the ditch towards its ends seemed also artificial and thus recent. Since the apparent break begins at the southern one of these two ends, our exploration of the break was by means of trenches located towards its south side, where the next plantation to the south had not narrowed the ditch and thus where we expected the ground to be free from the effects of the landscaping plan (Fig 2.16).

They were two trenches, planned as a right-angled L, with a meeting-point 75 ft (23 m) south of the break's apparent beginning on the north. The first, at

right-angles to the line of the dyke, was dug to this point from a start made 42 ft (12.8 m) behind it, giving Section 2. The second, giving Section 4, was dug from it southward for 22 ft (6.7 m) to end within the rampart's steep fall to the seeming interruption. Taken together, they showed that the interruption was not real, but merely apparent; the rampart was crossing the head of a natural gully and dipped down to do so without any break, as shown in Section 4. As the line of this was well behind the rampart's original front, its material here was a mass of red sand, overlain by red gravel to a consistent thickness of a foot, and sitting directly on a surface of a natural gravel. This had been cleared of any turf, but was flecked with much charcoal, showing that growth on it had had to be cleared by burning. Over the 1 ft (0.3 m) thickness of structural gravel lay again red sand, which on the rampart's steep slope had been encroached upon by roots and humification, but which passed beneath these into a thickness increasing downwards, from 1 ft (0.3 m) to 2 and nearly to 3 ft (0.9 m). Thus at half-way down the trench's exposure of the side of the natural gully, the rampart was standing 5 ft (1.5 m) high, with under just 4 inches (100 mm) of sandy surface-soil and roots. The gully's westward fall, continuing under it, will of course have let it stand higher where nearer its front.

At a point 6 ft (1.8 m) along our trench from its northern end, where the rampart stands 5 ft (1.5 m) high, Section 4 shows its red sand and gravel, slanting from the south, intermingled and interleaved with rampart-material which was on the floor of the gully and came from the rampart to the north, on the opposite side. And this material was heavy grey clay, laid in two masses with a tapering red sand layer in between them, and with the upper red sand that extended from the south overlying the clay, and at one point running into it. We inferred that the gully had borne no turf fit for the structural use revealed in Section 6, so that the rampart builders had dug, somewhere nearby, into the London clay which lies beneath the Colchester gravels and sands, and had used the clay to make their construction firm across the gully.

The extent of the clay towards the rampart's tail had already been disclosed in Section 2. With the tapering red sand layer 3 ft (0.9 m) long and still between its two masses, the clay stretched back for 13.5 ft (4.1 m) from the line of Section 6, its thickness there (with the sand layer enclosed) being 3.5 ft (1.1 m). It sloped away to nil, lying throughout on the charcoal-rich surface which here again had been left by clearance of the natural gravel. And over it the upper red sand, rising first to a thickness of 2.75 ft (0.8 m), and 3 ft (0.9 m) over the vanishing point of the clay, became gradually thinner until it tailed away at 26.5 ft (8.1 m). Here will have been the rampart's tail, unless the overlying sandy soil had taken up more of it. Although naturally thin elsewhere, the overlying soil was nearly 4 ft (1.2 m) thick above the tail of the red sand under it, and seems here to be

13 In his unpublished corpus of Romano-British brooches, M R Hull described the brooch (catalogue no 558) as follows: central circle empty, inner band may have been red, outer is bright green. Pin sprung on single lug; edges much damaged.

mostly an accumulated filling of the hollow behind the rampart's long nearward slope. The underlying natural gravel's surface had in one place some turf on it, although it was little more than 3 ft (0.9 m) long. Two feet (0.6 m) west of that, in an 18-inch (450 mm) deep pit covered entirely by the gravel, was a hollowed deposit of charcoal and scraps of incinerated bone, which Miss M L Tildesley (Museum of the Royal College of Surgeons) found to be human long-bone and cranium fragments, perhaps of a child. The date of this cremation, prior to the rampart, cannot be known; its having been a 'foundation deposit', sacrificial, by the rampart's builders, though conjectural, may be allowed to be quite conceivable.<sup>14</sup>

At Site 3 then altogether, these two sections allow of no break, appearances notwithstanding, in the dyke's continuity here. For the mass of the structural clay, though not pursued right across the natural gully, was at the meeting-point of two sections of dyke, although the gully beneath it was still falling away, and the modern ground level over it rises a little, then is level, till it reaches the rise of the rampart on the gully's north side. If the clay stopped short, leaving room for an entrance in between, it is there that a hollow in the surface ought to appear. In fact, the surface is at its lowest (slightly) where the clay meets the gravel (at 5 ft (1.5 m) along the length of our Section 4). How the surface over a genuine entrance behaves, we shall see in Site 2. And as for the use of any entrance through the dyke for a Roman road, neither of the two roads that have traces pointing east at this sector of the dyke is aimed at either of its seeming interruptions (see Fig 2.32, p 57).

*iv) Site 2 (Sections 5 and 7-11)*  
[Fig 2.21]

Site 2 comprises Section 5 across the gap in rampart (visible bottom 250-262 ft (76-80 m) from the south edge of the gravel pit) and the ensuing Sections 7-11 showing an original entrance through the dyke (Fig 2.21).

We suspected an original entrance here from the start, and made all our trenches narrow and affording minimum interference with any future complete excavation to disclose its features, such as structural timberwork. This would plainly be of great interest, and still remains to be undertaken. Meanwhile we hoped for some evidence of date, and in this we were not disappointed.

The visible gap, on its northern side, is merely a shallow depression, as what it terminated here was the stretch where the rampart was destroyed, by throwing most of it down the scarp in front, leaving only its base, as shown on Site 1 by Section 3. It is the south side that marks the gap so strongly, by the steep rise of the rampart to a well-preserved crest, and in front of it by

the resumption of the ditch: evidently much as we were to find it in Section 6 (Site 4) since the ground in front of it rises, as already explained. We determined to test the gap right down to the natural, and to do so first in line with the crest on the south.

Section 5, to accomplish this, was begun at 4 ft (1.2 m) short of what superficially appeared to be the floor of the gap, but was very soon lengthened for a further 8 ft (2.4 m) northwards. Southward, it was taken to the start of the steepest rise of the rampart; the total length was 46 ft (14 m), with an extra foot to test the upward slope of its solid material. The solid material was a hard and stony gravel, laid direct on the cleared surface of the natural, firmed sand. This surface sloped down, at 1 in 4 roughly, for 22 ft (6.7 m), from the section's southern end, to reach the quite flat bottom of the beginning of lower ground, here only 12 ft (3 m) wide but slanting forward to west and north-west, to pass into the floor of the valley, lower again. This same surface, towards the section's northern end, was rising again but very little; its reaching the level found in Section 3, 140 ft (43 m) farther, must therefore be gradual. Thus the place for this entrance was chosen not in the hollow of a narrow gully, but simply where the approach from the valley would be easiest. And over the gentle natural rise in the northern 12 ft (3.7 m) of this section, was the hard stony gravel of the rampart again. The natural sand's surface, flat between this and the steep rise southward under its own rampart-gravel, was overlain by 6 ft (1.8 m) of silt. So the entrance was proved.

The two slopes of rampart-gravel, the southern hard and stony, the northern sandier, were each too slanting to be thought very near to their rampart's front. By aligning our trench on the crest of the southern rampart, we had evidently cut them well clear of its front. But as explained above, since a total clearance was never our purpose, we wanted the minimum possible interference with any future use. So our next trench, crossing the entrance, giving Section 7, was cut well forward, 24 ft (7.3 m), in front of Section 5. It was 36 ft (11 m) long, but touched nothing of the rampart on the north; at its other end, the southern rampart's gravel was tailing off, just as in the last 3 ft (0.9 m), of it in Section 5. And again as there, over it and from end to end of the trench, was a deep accumulation of silted soil.

However, this had been covered by a layer of gravel which was 1.5 ft (0.3 m) thick in the middle of the trench, but tailed off to the present ground surface 7 ft (2.1 m) to the south in the trench's west face and 9 ft (2.7 m) to the south in its east face, suggesting that the gravel's extent lessened as the entrance narrowed. That it did so, appeared in Sections 8 and 9. In Section 8, along the middle of the entrance passage, from Section 5 to meet Section 7, it was found running out to the surface 9 ft (2.7 m) along from Section 5, and deepened to continue throughout the 16 ft (4.9 m) to Section 7. That it reached no nearer to Section 5 than this, but had ended here transversely, was shown in the adjoining diagonal

<sup>14</sup> Alternatively it may be part of the Lexden cemetery since it lies not far from known cremations (pp 164-9). Its location in relation to the Lexden cremations is shown by the letter A on Figure 7.1 (p 164).

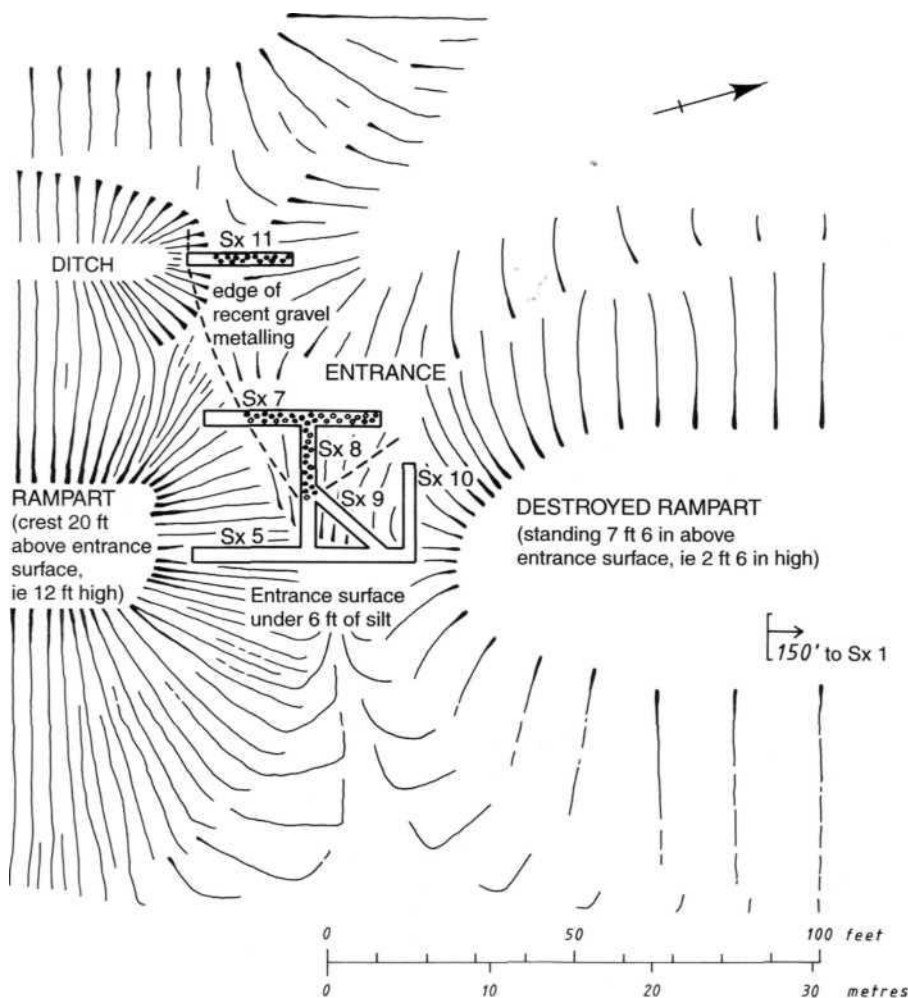


Fig 2.21 Plan of Site 2 of the 1932 excavations of the Lexden Dyke in Lexden PanX. The figure is based on a plan on a glass slide in the CFCH archive. (No 53 on Fig 6.1.) [Pages 42-4]

Section 9. The transverse ending, directly continued from Section 5 into this, is in its opposite face 5 ft (1.5 m) from the SW end. Moreover, beneath it here and in Section 5 overlying 4.5 to 5.5 ft (1.4-1.7 m) of silt, was a growth of natural turf 4 inches (100 mm) thick, evidently formed in the hollow here appearing in the silt. The same superficial gravel reappeared when Section 11 was cut, 50 ft (15 m) farther out, to confirm that the ditch in front of the southern rampart really ended where expected (which the section proved, well clear of the approach to the entrance). Where the section ended above the full depth of the ditch, the overlying silted soil was 3 ft (0.9 m) deep and more; it continued to the trench's north end, growing thicker as the surface of the natural gravel under it sloped downhill. The superficial gravel began 1 ft (0.3 m) from the trench's south end, continuing north of the ditch at a thickness that varied from 6 to 9 inches (150-225 mm). It was continuing still where we ended the trench, which altogether was 22 ft (6.7 m) long. A gravelly humus had formed on it, seldom more than 6 inches (150 mm) thick.

Thus the gravel had been laid to overspread the approach to the entrance, but at a time considerably later, as was shown by the depth of the underlying silted soil. We thought the gravel was at least as recent as the park. But the chronology we most required was in the entrance itself, as a guide to the

dating of the Lexden Dyke altogether. Returning to Section 9, we had only to note that the northern rampart-end, as shown by its structural gravel exposed at the section's NE end, had a steeper termination, 2 to 3 ft (0.6-0.9 m) from that, than in the directly adjacent part of Section 5. This suggests that were the entrance completely cleared, as in the future it might be, its flanks would be found to have originally had a structure that held them both to a face still steeper of timber, but using also either turf, as in Section 6 (Site 4), or clay, as in the Site 3 sections and as here suggested in Section 11 by the silted clay in the ditch. For the rampart on the south is preserved with a crest some 20 ft (6.1 m) high above the level of the excavated passage-way in Section 5, and the rampart on the north is now only so low because mostly destroyed, while between them the 6 ft (1.8 m) depth of silt implies that a great amount of material drifted down from them. The weight of the ramparts could hardly have been sustained on either flank without a structure stronger than a single pair of gate-posts. The most that we felt we could do towards getting a further indication of the kind of structure involved was to cut a section (Section 10) at right-angles to the northern end of Section 5, to ascertain whether enough gravel remained from the destroyed north rampart to show that it had really flanked the

whole of the passage-way. The gravel of its base was sectioned, down to the natural for the trench's full length of 18 ft (5.5 m); it was quite undisturbed.

### Dating

The main question remains: what had we for dating? On Site 1, it has been seen how little can be made for precision, of the Roman brooch in Section 3 (p40), and Sites 3 and 4 produced no finds at all. But here on Site 2 there were nearly 30 fragments, all of pottery, save one piece of Roman tile, and a Roman bronze coin. In Section 11, above the approach to the entrance and nearly at the trench's north end, the gravelly surface soil formed on the superficial gravel contained Roman potsherds, worn and indeterminate. Though they might suggest that the superficial gravel had been laid in Roman times, this need not follow from their lying merely over it, close to the surface. All the other finds occurred in the entrance excavations; and here there were various scraps of pottery loose in the deep overlying silty soil, in Sections 7-10, and in Section 5 rather more. Some were Roman, some of pre-Roman native fabrics, and any of these could have come with the silt from the wastage of the rampart-slopes above. But this was not all.

Twice, in and under the undisturbed gravel of the northern rampart's base, were found sealed pre-Roman pottery pieces. They must have been dropped while the rampart was in course of construction. The piece lying under the gravel, on the natural sand's cleared surface, was in Section 10. It was a sherd from the shoulder of a native coarse ware jar of Cam Form 263, showing part of its typical band of finger-tip impressions; it lacks its rim, but is finger-tipped as *Camulodunum*, pl 133, no 263A (which is datable as perhaps only Period 1 of the Sheepen occupation). The piece found sealed in the gravel itself was at the farthest north end of Section 5. This too was of a native coarse ware jar, but with rim and part of neck. It is within the range of simple profiles assigned to the black cooking-pots of Form 264 (*Camulodunum*, pi 133), but this jar had been not so small, and the piece could be placed between profiles 12 and 13 in *Camulodunum* figure 56, (p 268). At the bottom of Section 8's trench, on the surface of the entrance passage, and thus covered only by the 6 ft (1.8 m) thickness of silt, was a further native coarse ware sherd, though unfortunately featureless. In the lowest of the silt above the entrance-passage surface, at 8 inches (200 mm) up from it in Section 5, was another such sherd together with two of cordoned finer ware (one along with a scrap of iron), each of which if larger would have had a profile among the many in *Camulodunum* figure 54 (pp 256-7 & 260). Both of these sherds can have been pre-Roman. And up within the silt as far as 3.6 ft (1.1 m) from the bottom, there were scattered four further pre-Roman sherds, three coarse and one again of the cordoned finer ware. The silt yielded also some Roman sherds: small scraps higher up, and below, within 2.5 ft (0.8 m) from the

bottom, six miscellaneous pieces more, with a fragment of Roman tile, part of a Roman-looking iron file, and the single Roman coin. This, with a radiate obverse head was submitted, with the Sheepen Roman coins, to Dr Humphrey Sutherland (Ashmolean Museum, Oxford); he identified it as an antoninianus of the Gallic emperor Postumus (AD 259-68), fair to worn (in condition 5), comparing *RIC* 377. The distribution of all these finds, in the silt above the passage-way, is consistent with an initial silting during the Roman period, during and after which it included pre-Roman pieces, derived from the rampart-slopes as already here presumed.

In the northern part of Section 5, in the first few inches of silt, directly over the surface of the destroyed rampart's base, lay five pieces of coarse ware appearing pre-Roman, one Roman sherd appearing early, and a rim fragment of Gallo-Roman terra-nigra platter of Cam Form 4A, which is normally in Britain pre-conquest (*Camulodunum*, pi 49 & 216, fig 46,1). These should bear on the question of a date for the destruction of the rampart, leaving a mean 2 ft (0.6 m) thickness only of its base). Here, where they were lying, just above the base's surface, this was sloping down from the north, being 2.75 ft (0.8 m) below the level modern surface at the section's north end, and 3.75 ft (1.1 m) below it at 4 ft (1.2 m) along. Down this slope at least some of the pieces may have drifted. But all the little group was in the lowest of the silt, which was a brown soil throughout representing a gradual accretion of humus; although it is hard to guess how long the latter will have taken to grow to a thickness of 2 to 3 inches (50 to 75 mm), it was anyhow post-destruction. The pottery being therefore in a derived position here, the early Roman sherd in it is no surprise, along with the pre-Roman pieces of earlier date somewhere near. Thus the group does not suffice to give the destruction a very close date. One in the 1st century can suit the position of the Roman brooch datable to c 100 or a little after (Section 3), whether dropped when the destruction was recent or a longer time after it.

The date at which the spread of superficial gravel was laid is a different matter. Its purpose was to give a better surface to the approach, sloping up from the valley, to the entrance. Though at the time of excavation we regarded it as modern, because of the depth of the underlying silted soil, and though the three Roman sherds found in the gravelly soil above it might have washed downhill from the surface of the rampart at any time, there remains still the question who would want this better surface? For whoever it was would be using the entrance, which it led to. Would the laying out of the park in modern times have really required this? An alternative answer might be attributing the laying of the gravel to the Romans; yet the silted soil beneath was at its thickest in Section 11, and overlay, at the section's south end, all the silt in the ditch of the dyke, resuming here to continue southwards. We were therefore right in regarding the gravel as modern. Yet it is clear that the Roman road from the west, inevitably early, forked into two to pass the dyke through each of the pair of appropriate entrances: that on the line of the modern Lexden Road, and this one. Its surface here would thus

have been formed by clearing the natural: gravel where revealed in our Section 11 (p 43), but changing in the course of the next 50 ft (15 m) to the hard red sand, which formed the surface in the entrance itself (Section 5, pp 42-3), and which ever since this was constructed had served as its roadway. However, the Roman road after it had passed through the entrance, and passed at some point across the line of St Clare Road to reach Fitzwalter Road at the point numbered 162 in Hull's *Roman Colchester* (p 7 & map pl 43) had at that point a laid metalling of gravel.

*Lexden Dyke South*  
[Fig 2.22]

This was located first by Mr Hall. He suspected it would start beside the footpath, continuing that along the rampart in the grove to flank the field-hedge running thence south-westwards; and of his short search-trenches cut at 90° to the hedge, three cut the outer lip of the ditch of the dyke. In 1959 a single trench was cut beyond the gap at the end of the grove to confirm this, and also to determine where the dyke would begin. Directed towards the gap and at a right-angle to Hall's trench, it found the ditch beginning about 130 ft (40 m) out from the end of Lexden Dyke Middle. The rampart of Lexden Dyke South has been levelled beneath the footpath and the hedge, but the section dug in 1959 across its ditch (Fig 2.22) showed dimensions as ample as expected —though with the outer lip cut obliquely by a later smaller ditch. That section, in the NE angle of the field (no 1276) was about 30 ft (9 m) from its eastern hedge (see the bottom right inset in the Prettygate Junction pre-Roman works plan, Fig 2.25).

While the ditch's inner lip, back at its start, could be estimated rather over 9 ft (2.7 m) out from the hedge, it could here be some 2 ft (0.6 m) less. As the hedge is straight, the dyke seems thus to have started on a slight bend inwards. Intervening modern enclosures make its further course untraceable, but if brought by the bend to approximately E20°S, it would reach the point where Capel Road ends on the Maldon Road. Just about there, as will appear below on page 68, our Roman Road 5, running past on its way south-westward, seems to have made a slight change of direction, anyhow required by its recognised alignment farther on. If that were the point where the building of our dyke had been halted, the change of line would come best where the road passed its end. And the dyke has never shown a trace of continuance farther. For this and its placing in the dyke-system's sequence, along with Sheepen Dyke, see pages 52-5.

**Moat Farm Dyke**

See Lexden Dyke.

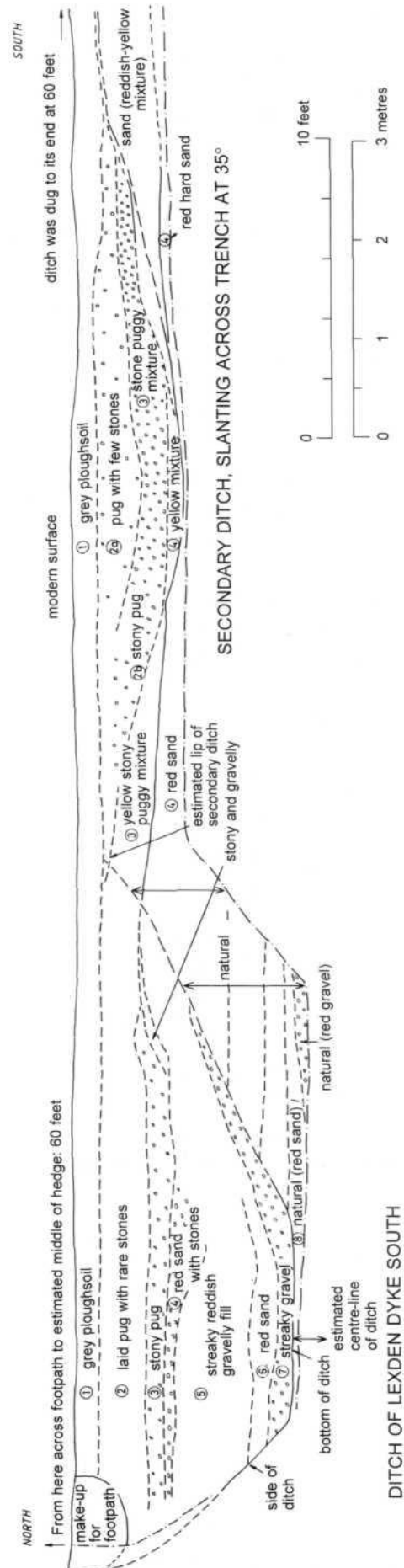


Fig 2.22 Section across Lexden Dyke South. Dug in 1959. (No 54 on Fig 6.1.)



**Oliver's Dyke**

This, taking its name from Oliver's Farm, one field to east of it, moves us back to the south of the Gosbecks site, where protected by Gosbecks Dyke South and Kidman's Dyke South. Its course, a little west of south, starts from the latter, Kidman's Dyke South, at nearly a right-angle, aligned on the eastern end of the other one, Gosbecks Dyke South. It is clearly thus later than both, though by how much one can only guess. Running straight down the slope to the Roman River, it is visible at first, but is soon levelled to leave its line marked only by the hedge-bank that ends just west of the footbridge over the river, on the mill-leet which rejoins it under the bridge. The finish of the Oliver's line, past its cutting by the leet, is marked by that of its rampart, emerging as a tump.

**Prettygate Dyke**

*Excavations at Peartree Junction*  
[Figs 2.23-2.24]

Peartree Junction, with our reason for its name, has been already introduced (p 8), noting Hull's excavation here in 1936. He sectioned Prettygate Dyke at the beginning of its ENE course, disclosing the structure of the rampart, and a filled-in ditch on either side of it. The rampart had been lowered, but enough remained to show that its gravelly mass was faced with turves on the north; the ditch on this side, which was greater in dimensions than the southern one, had therefore been primary. The southern ditch, about 12 ft (3.7 m) wide and 4 ft (1.2 m) deep,

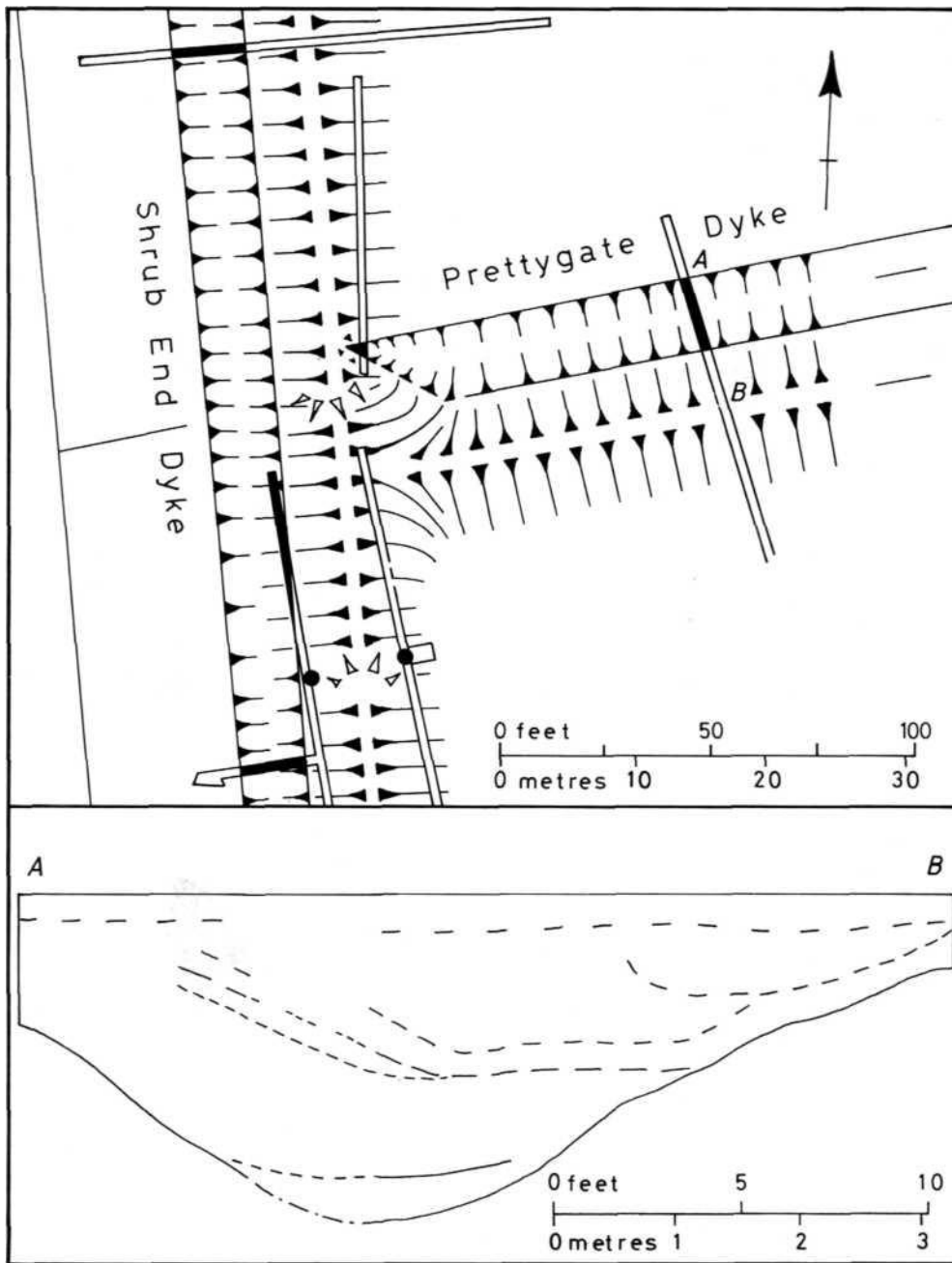


Fig 2.23 Plan of the pre-Roman works at Peartree Junction, and a section through the north ditch of Prettygate Dyke. Dug in 1936 by MR Hull. (No 37 on Fig 6.1.) [Pages 46-8]



must therefore have been subsequent. That it in fact replaced the northern one, thus reversing the purpose of the dyke, will be explained on page 48. It has already been seen to be early Roman and is discussed for its relation to the much-debated Roman Road 2 (p 23). The other end of Prettygate Dyke (at Prettygate Junction), where the 1956 excavations confirmed both ditches (pp 48-50), was regarded by Hull as its start, so that at Peartree Junction — our start for it here — he had it ending. His own account of what he did in 1936 at Peartree Junction is in his *Roman Colchester* (p 4).

The extension of Hull's excavations into the ditch of Shrub End Dyke, to seek whatever might have been built across it, in line with the Prettygate rampart

(having first proved its filling to be undisturbed on either side of that line), was done by a diagonal trench through it. But since his concern with this rampart had mainly to do with the debated Roman road (Henry Laver had believed the rampart was the agger for its course between here and the end of Bluebottle Grove), the primary aim of his trench was to see if any causeway had been built to allow the road to cross the Shrub End ditch and bring it on to the Prettygate rampart line from its slant through the fields outside that ditch, where Laver had confidently claimed it. For a causeway crossing the ditch is what the trench did seem to have disclosed. Hull indeed saw a guessable alternative: that the Prettygate rampart itself had crossed the ditch, and had ended

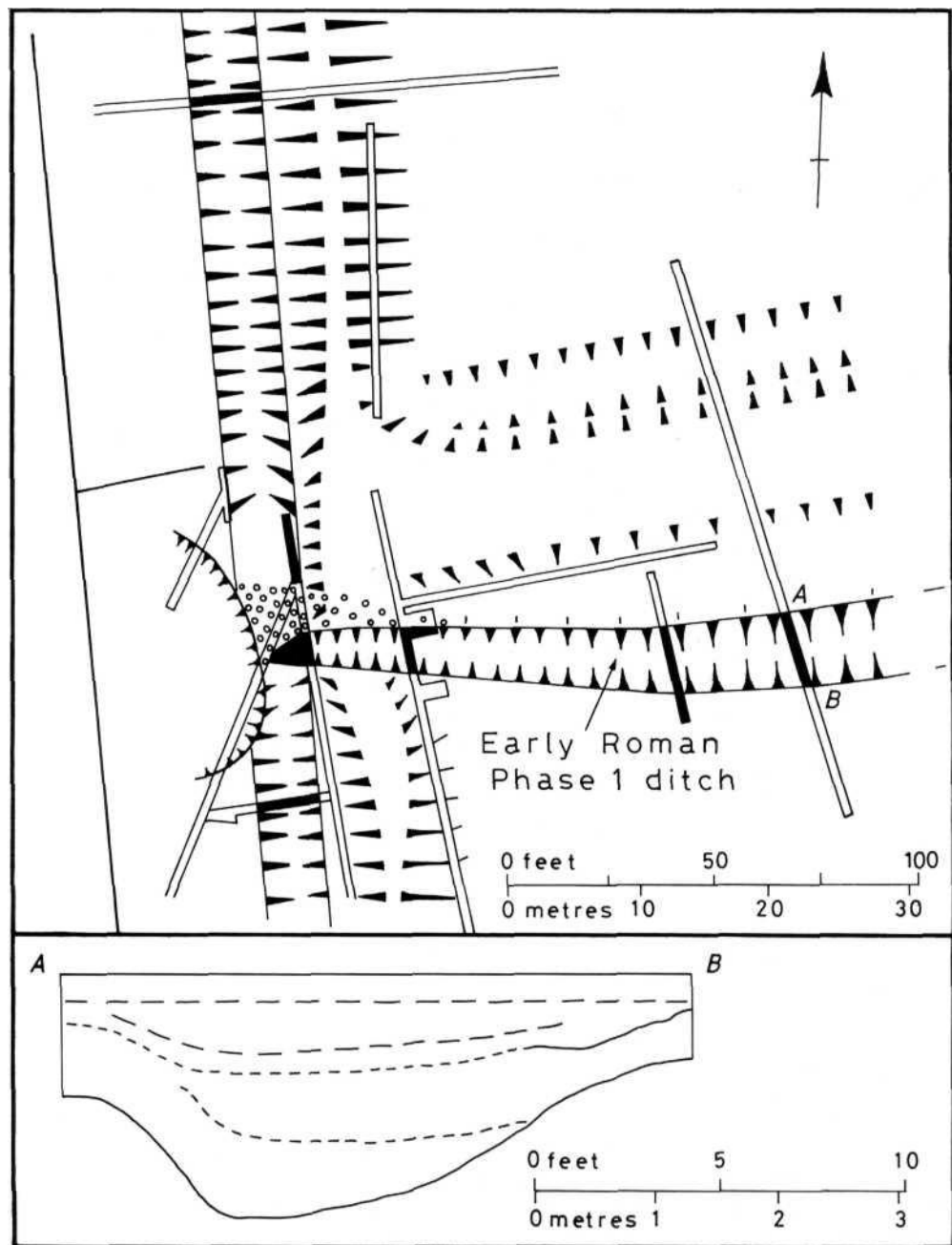


Fig 2.24 Plan of the Roman works at Peartree Junction, and a section across the ditch of Roman Road 2. Dug in 1936 by M R Hull. (No 37 on Fig 6.1.) [Pages 46-8]

its course there — or as we would say, begun it. Yet the Shrub End Dyke had its rampart on the ditch's inner side, and the Prettygate's crossing them both would disagree with what is clear from Hull's account, that neither was crossed by its primary great north ditch. Why the subsequent south ditch along it, early Roman as was mentioned above, was in his same excavation found to turn and run into the ditch of Shrub End Dyke, thus cutting across the Prettygate rampart, will be considered below. But Hull could in any case stoutly affirm that directly east from that, where he sectioned the Prettygate rampart, he found no Roman road-metal laid upon the rampart's surface — lowered as this had been to give the northern ditch's filling.

#### *Excavations at Prettygate Junction*

[Fig 2.25]

Prettygate Dyke's full length, from the Peartree to the Prettygate Junctions, is about two-thirds of a mile (or roughly 1 km). It was traceable along the row of field-hedges (just to the south of them) as far as King Harold Road, with a broad low ridge in the ground to mark its rampart; this crossed the first field after, and in the next had along it the footpath leading to the south end of Bluebottle Grove. In 1958, there was there enough room for us, on the ridge's northern side, west of the hedge here bounding the grove, to cut a section across the great north ditch. Two shorter trenches, in between it and the hedge, dug down into the filling of the ditch, at 90° to one another, both proved that it had ended joining the ditch along the grove (that of Lexden Dyke Middle). In one trench it was passing on beneath the hedge to do so, while in the other, which was parallel with the full section, the position of its outer lip showed it bent a little inwards to do so — just as the rampart would have been, if it was to continue past the butt end of the Lexden Dyke Middle. We have seen above (p 32) that the latter had cut the pre-existing ditch of Heath Farm Dyke; but the footpath, which forbade our testing the expected entrance through that, kept us away from where the Prettygate rampart must have met, at the end of the grove, the Lexden Middle's — though there must be interference there by the 1940 'tank-trap'. Nonetheless, it is not to be doubted that Prettygate Dyke was the later of the two.

The lowering of the Prettygate rampart here, leaving only its base — which carries the footpath that leads to the end of the grove — is exactly as found by Hull at 'Peartree Junction' and all along from there. Roman Phase 1, which followed directly on the conquest, required immediate measures for quartering the army (p 61). And a principal one would be converting Shrub End Dyke North, facing west, into the Triple Dyke; the expanse protected by this, stretching north to the Colne, and back into Lexden Park where were Lexden Springs, was essential for the troops' supply of water. As it had Prettygate Dyke along its south

side, one might perhaps expect a reversal of this, with ditch to south instead of north, for protecting it on that side too — if the work could be carried out fast enough to meet the immediate need, in addition to that for the Triple Dyke, which was more essential and greater in length. Possibly it could have been; yet any southern ditch's upcast would be needed for a temporary bank, on top of the rampart as lowered for a filling of the northern ditch. And not only is such a bank hypothetical in any case, as it would have to be removed to let the rampart's lowered top be prepared for the laying of Roman Road 2, but sectioning the northern ditch found it filled no more than once. So the fill had come from only one lowering of the rampart: for the intended Roman road and not before; thus in Phase 1, whether or not with a southern ditch added, the rampart had its full height still, and had its northern ditch still open. It might indeed have been judged, even so, to be an obstacle sufficient for protecting this side of the northward expanse; obstacle it certainly was, and the matter may now be left at that.

For the intention to lay the Roman road on it, though metalling this with gravel was never carried out, must bring us into Roman Phase 2. Proof of that is given best farther west along the line of the road: first in the Gryme's Dyke entrance described above (pp 27-9), where it passed into the dyke-defended area complete with metalling, and with a side-ditch on its right which marks the whole of its further course, and completes the proof. As has already been shown above, the road, after turning south immediately behind Gryme's Dyke, cuts through the certainly Phase 1 ditch and palisade-trench (pp 59-61), and slants from there SE, across what now is the Dugard Estate, straight onward to Peartree Junction. From there, this ditch for the road is resumed along the south side of the rampart of the lowered Prettygate Dyke, and is the ditch that has here been discussed — carrying, anyhow now, its proven Roman Phase 2 date. Next, then, we pass to its behaviour right through Prettygate Junction, still using our early Roman plan (Fig 2.25).

It was located here by numerous trenches, including full sections across its filling. It cut through that of the ditch of Heath Farm Dyke, and approached the southerly portion of the entrance separating Lexden Dyke Middle and South. All this is shown on the Prettygate Junction early Roman works plan (Fig 2.25). At this entrance, where it bends to pass the beginning of Lexden Dyke South, something has caused it a reduction in size, which suggests that, onwards from there, it was not on the course it actually took, as far as its disclosure by Mr Hall in the Grammar School playing-field (see below). But if originally meant, as is conceivable, to turn from that entrance just a few degrees more north of east, and to go straight on, its aim could then have been intended for the only suggestible major feature of the station for an army unit where now is the Grammar School itself: that believed by Mr Hall to explain his own discoveries there, described and discussed on pages 64-6, and given a historical context on pages 66-7.

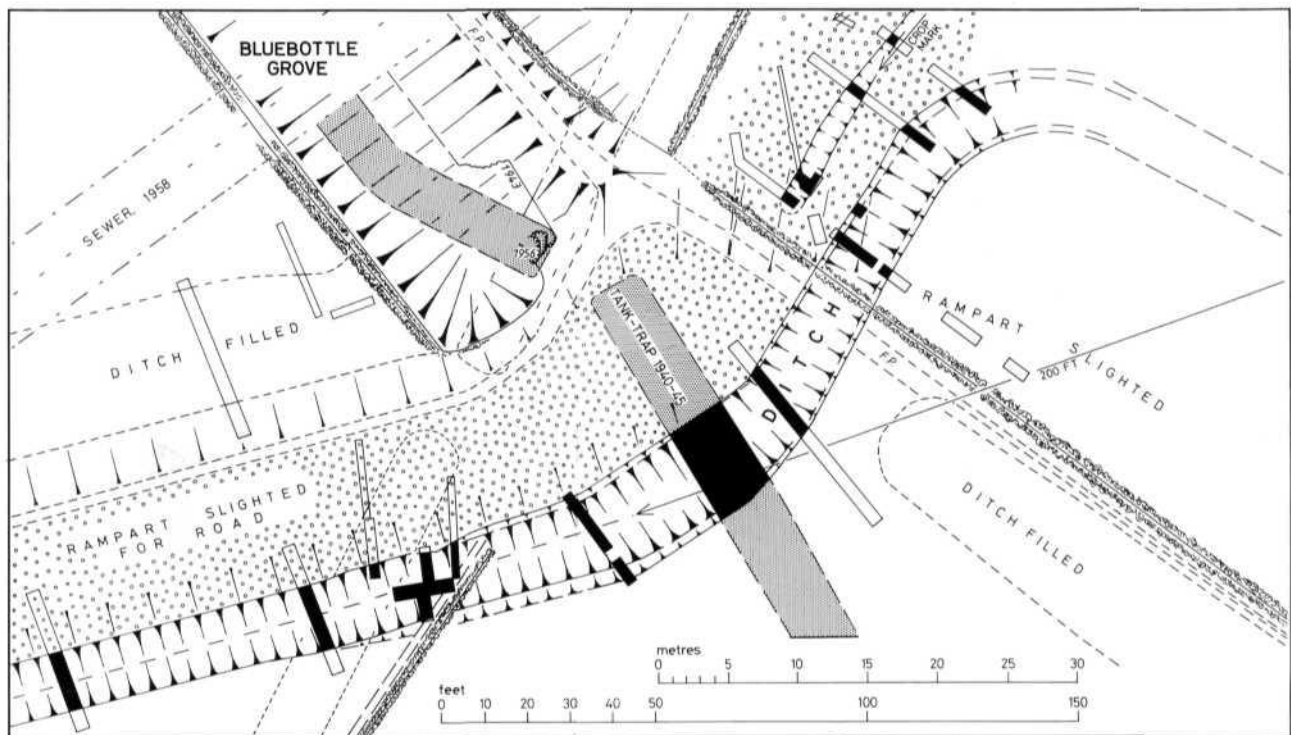
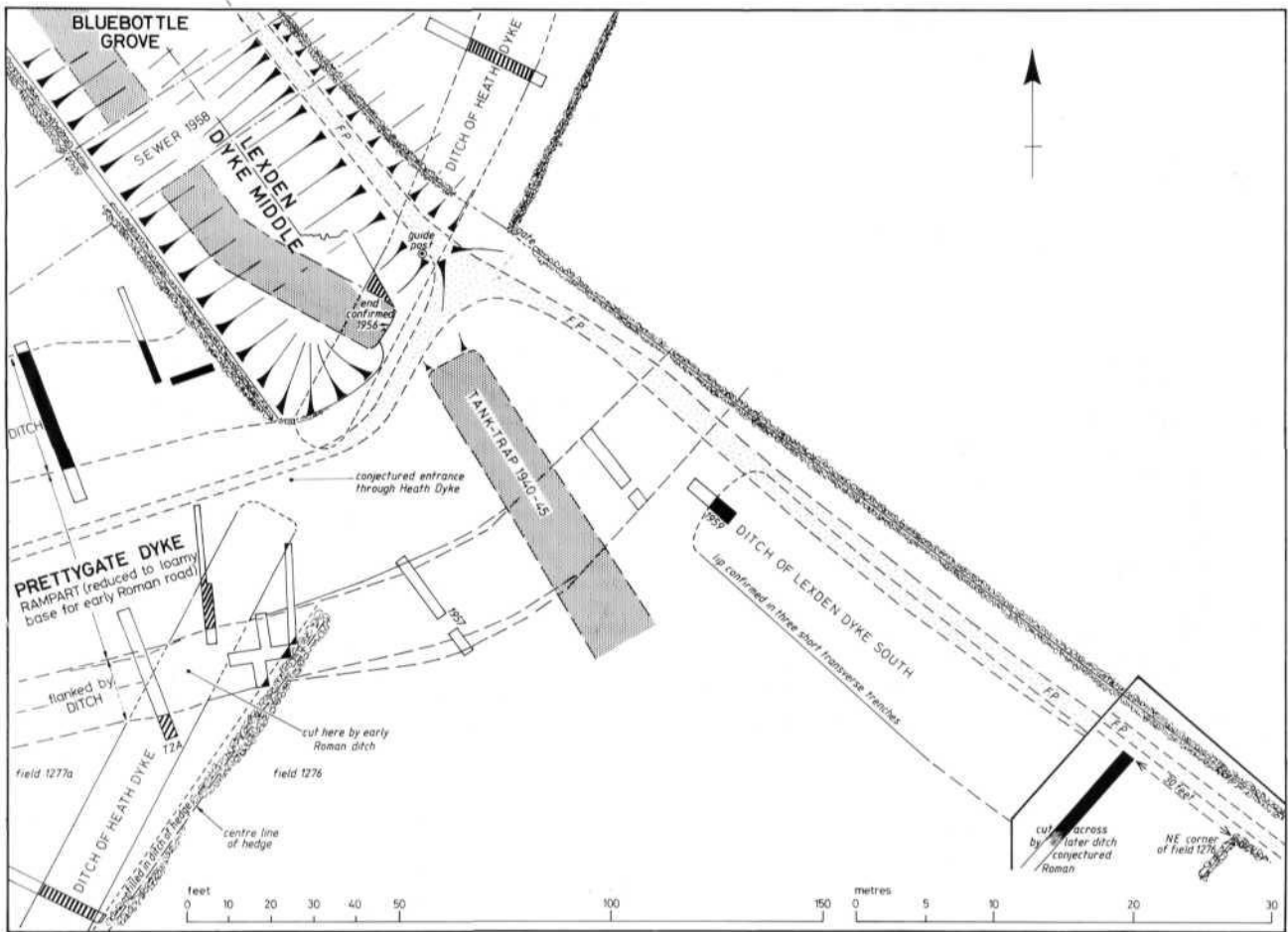


Fig 2.25 Prettygate Junction. Above: plan of the pre-Roman works. Below: plan of the Roman works. Dug 1956-8. (No 54 on Fig 6.1.) [Pages 48-50]

This might indeed account for what was found within the Lexden Dyke entrance, beside the bend, just noticed above, in the ditch where its size has become reduced and it swings away, behind where the rampart of Lexden Dyke South will have stood before its levelling down, towards the course for it fixed in the Grammar School playing-field. That course, as can be seen on Figure 6.1, at E15°N, is dead on the line that the ditch has had throughout beside the Prettygate rampart, lowered as intended for the road. Most unhappily, our trenching for it on beyond the bend was abruptly stopped by the Prettygate farmer, bearing down on us on the spot. But between it and the adjacent western corner of his field, search-trenches already had been dug by Mr Hall; and the southern end of two of them hit on a very much smaller ditch, which he could next line up, in both of two further trenches, with what he had noticed beyond: a crop-mark in the field, narrow and ascribable to just such a ditch, aligned about N40°E (not on plan). Might this have been at least a setting-out ditch, for a resumption intended, on this alignment, for the road? This notion could suit the case, made on pages 61-2, for the Roman Phase 2 arrangements, here already referred to, quickly cancelled still well within the conquest year of AD 43.

### 'The Rammers'

[Fig 2.26]

[PC: The dyke is known only from references in two 17th-century perambulations (Morant 1748, Bk 1, 92-3). In the earliest of these (of 1637) it is referred to as the 'Rampiers by Horkesley Heath', and on the other as 'the Rampers on Boxted and Horkesley Heath'. It appears from the perambulations that the earthwork extended westwards from a brook at the corner of Langham Park (which we are told was on the corner of Boxted Heath) to a point some way east of where the Black Brook crosses the Nayland Road. Neither point has as yet been closely fixed but the east point is probably where Salary Brook crosses the Langham Road, the west point being less precisely locatable. This gives a maximum length for the earthwork of 2.0 km (2,200 yds). It may have been longer if parts of it had been levelled or not used to define the Borough boundary. The dyke linked the ends of the valleys formed by the Salary Brook to the east and the Black Brook to the west so that together the three features protected a large arc of land on the north side of the river Colne.

CFCH prepared a plan to indicate the location of the dyke but seems never to have written any text. The numbers 1-9 on his plan (Fig 2.26) show that he intended to described the perambulations fairly fully where they crossed north of the river Colne. The dyke was probably not quite so far north since it ought to coincide with the position of the Borough boundary. Two asterisks have been added to his plan to show the two points described above and thus the likely line and maximum extent of the earthwork. The two points do not seem to have been linked by a stream in the way his figure shows.]

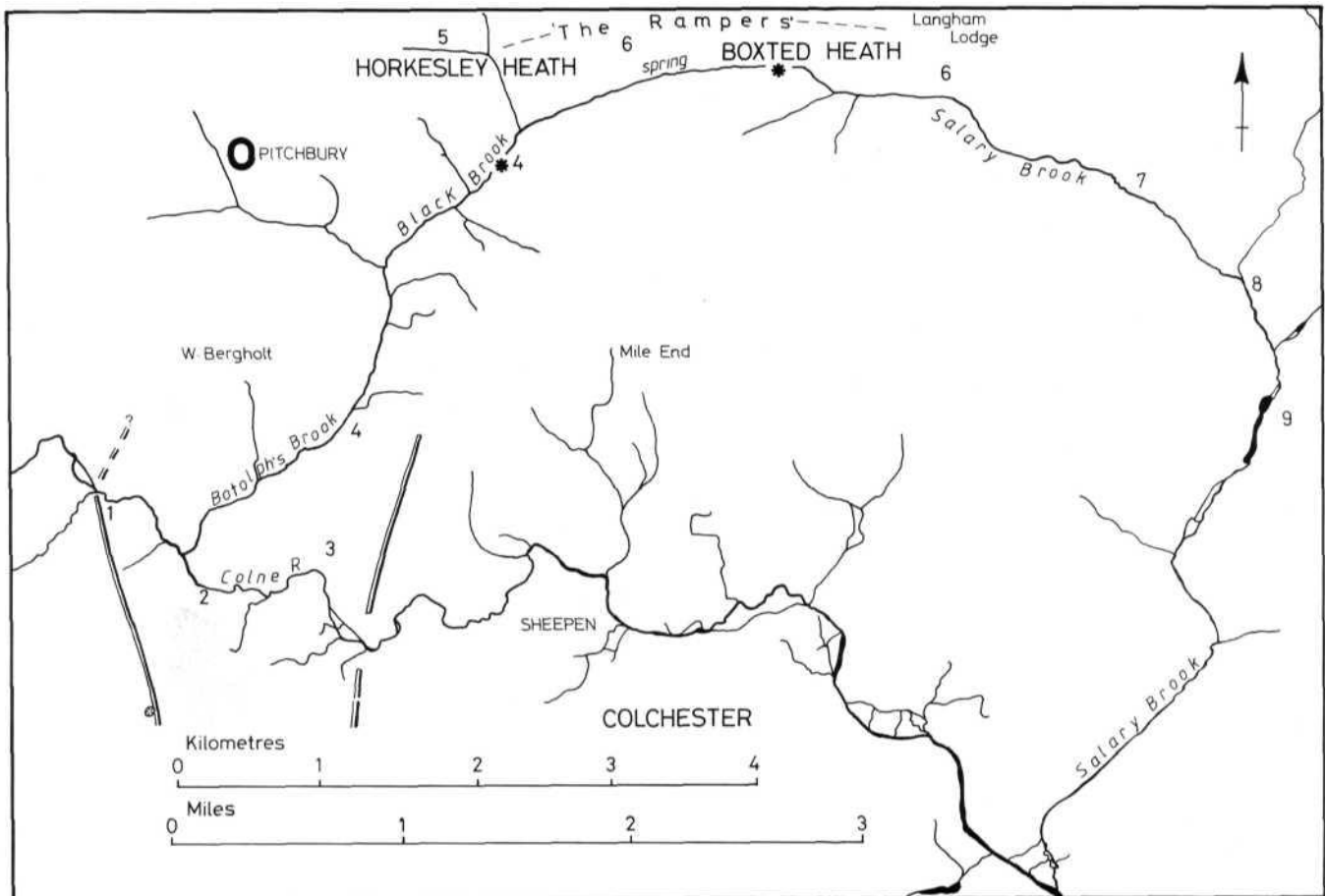


Fig 2.26 Location of the 'The Rammers', north of the river Colne. Two asterisks have been added to the plan to show the likely east and west limits more precisely.

### Sheepen Dyke

[For further details, see Chapter 3 (pp 70-84)]

As Figure 2.1 shows, Sheepen Dyke (described as Ditch 1 in *Camulodunum*) protects the Sheepen site behind it against the NW (or WNW) in its long main stretch, with an entrance each in Regions 1 and 3. It turns, at the hill-top end of its long main stretch, from a third entrance, to protect the site against the SW, in a stretch ('Ditch 1B') with ditch less regular and somewhat smaller, which ends above the valley that bounds the hill-top on the south. The valley-slope in front of the south portion of Region 5 was protected by lengthening the long main stretch (as 'Ditch 1A') as far as the 100 ft contour on the slope, some 500 yds (450 m) to the west. The date of this extension was later, but still pre-Roman. The 1930-39 excavations proved the course of all these ditches (*Camulodunum*, fig 2) and revealed depths of up to 8 to 10 ft (2.4-3.0 m). With a rampart of proportionate size (though leaving vestiges in only two places), the dyke was plainly a defensive military work which was originally V-shaped in plan, with sides roughly 750 and 300 yds (700 and 280 m respectively) long, set at an angle of about 100°.

The pre-Roman occupation within it, over an expanse that from north to south measured nearly half a mile (800 m), began on the hill-top and its flank about 5 BC, but in the northern parts below it, towards the Colne, it will have been started together with the dyke, about or very close towards AD 10. (See pages 81-3 for any previous use of a port, earlier within the last quarter-century BC, for receiving wine-amphoras landed from the river.) Occupation on the hillside, sloping up from that, can have been started rather after, but clearly the dyke's main stretch was designed as a unity, from the river-side to the hill-top settled already — where the south-westward-facing stretch, though rather weaker, would soon be needed for completing the design. The discussions of detail that lead to these conclusions — partly rejecting, partly accepting advances on *Camulodunum* lately offered — will be found below on pages 70-84. The

relation between Sheepen Dyke and the dykes outside it is discussed on pages 52-8 & 81-3).

### Shrub End Dyke

Shrub End Dyke North, as described below on page 52, was converted in the Roman period to form the Triple Dyke (pp 55-9). The triplification will have started a short distance north of the end of Heath Road (which joins Lexden Straight Road; p 159-160, nos 80-81) and stretched northward along this road's east side, soon becoming visible for quite a long way. Southward, and still beside the Straight Road, the ditch of the dyke remained single and its rampart of course was everywhere levelled in post-Roman times.

*Shrub End Dyke where always a single work*  
[Fig 2.27]

Before the Straight Road was lined with houses down to and past the Maldon Road, to which it bends from south to SE (into the part of suburban Shrub End known properly as Bottle End), the dyke's single ditch appeared in the 1933 air photographs, as a dark line flanking the road as far as its bend, along the edge of all three of the fields there.

A trench for a water-main was dug in 1963 in the spinney which was next to the bend in the road and which covered part of the ENE course of Heath Farm Dyke. Mr Brian Blake of the Colchester Museum observed, in the trench, the section of a slanting-sided ditch on the line of our dyke's. He thereupon measured some 65 ft (20 m) of the trench, fixing the ditch's outer lip at 52 ft 6 in (16.0 m) from the edge of the road's nearer kerb (Fig 2.27). Six ft (1.8 m) beyond its inner lip and cut into the natural gravel was a filled-up post hole, 7 in (18 cm) across and at least 30 in (76 cm) deep, and a short distance east and west of it were two stake holes indicated by a pair of dark vertical lines. Between

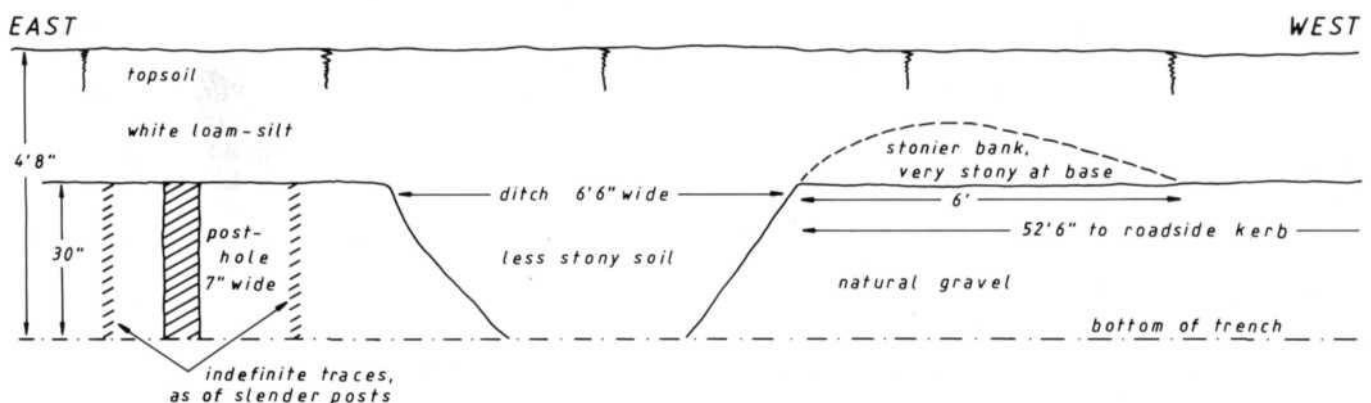


Fig 2.27 Section recorded by Brian Blake across Shrub End Dyke in 1963. (No 35 on Fig 6.1). [Pages 51-2]

the post hole and the lip will have been the berm of the vanished rampart, on which, no doubt derived from that, lay a silt of whitish loam. The ditch (filling not very stony) was perhaps 4 ft (1.2 m) deep at most. This is quite in proportion to the width between its lips, 6 ft 6 in (2.0 m), but both are surprisingly small. The position of the lip is assured, as is therefore the width of the ditch, because the next 6 ft (1.8 m) beyond the outer lip showed the old surface to be covered with a very stony gravel which was discernibly the base of a low but gravelly outer bank. Yet the ditch cannot be other than Shrub End Dyke's, showing this to be distinctly smaller here than would be normally expected.<sup>15</sup>

It must be added that farther back along Lexden Straight Road, in the NW corner of the next field north from the spinney, is the place where Hull, in 1936, carried out the excavation briefly recorded in his *Roman Colchester* (p 4), which partly involved our dyke. His account locates it only as at the junction of this with the earthwork that mainly concerned him, namely Prettygate Dyke, which is aligned on the south end of Bluebottle Grove, so at ENE. The junction is opposite the eastern end of what now is Dugard Avenue, but was then still Peartree Road — whence our name for it, 'Peartree Junction'. Since Prettygate Dyke had been fancied as the agger of a Roman road (our Road 2), approaching south-eastwards to reach it, a causeway for this across the ditch of our dyke would be expected; but as Hull's trench to find it was diagonal, and disclosed that digging for gravel had engulfed its southern part, this was not the place for measuring the ditch's width exactly. However, it was anyhow wider than down in the spinney. (See pp 46-8 for the excavation at Peartree Junction.)

The course of Shrub End Dyke beyond its crossing by the Maldon Road<sup>16</sup>, where aerial photography located it alongside Gosbecks Road, shows that it had bent from south to SW with the road-line still beside it; but as Gosbecks Road is not actually continuous with Straight Road, it has the dyke's ditch not on its inner side, but on its outer (on the Gosbecks side of the road), facing SW still. Where the lane branches off in that same direction, the dyke has been crossed by our Roman Road 5, leading down from the expected end of Lexden Dyke; but there is only one indication of its possible continuation further: the presence on its line of the road-side pond of Gosbecks Farm, which might be thought to represent its ditch. Even if so — and that is where the road bends away from its line — no trace of it has ever been found beyond. A continuance for it, and for Lexden Dyke too, as if to give the area behind them a protection on the south, is conjectured to have been intended, but never carried out because of a change in Cunobelin's policy. That possibility will lead us on soon to our Prettygate Dyke below.

### *Shrub End Dyke North*

The point at which Shrub End Dyke became Shrub End Dyke North has been stated at the opening of the section on Shrub End Dyke above. This point is very close to the east side of Lexden Straight Road where the dyke was triplicated to become Triple Dyke. The latter can be traced for a distance of about 290 yds (265 m) in all. The triplicated form appears on the surface south of the Lexden Road but is invisible for some 140 yds (128 m) north of the road, where it appears as a triple dark line on the 1933 aerial photographs. And these show it ending at Mott's Farm buildings, which stand right close to the Colne. Farther back towards Lexden Road, where traversing the open expanse called Hunter's Rough, was cut the section across it (pp 55-9), in 1961, that declared the triplication to be Roman, applied to a previously single rampart and ditch: the same Shrub End Dyke that we have seen south of the triplication's start. It is this disclosure of the Triple Dyke's true composition that requires it, as early Roman, to be reserved for describing below.<sup>17</sup>

### **Triple Dyke**

[PC: The Triple Dyke is now thought to have been the result of adding two ditches to the west side of Shrub End Dyke North in the Roman period. It is described below on pages 55-9 under Roman Phase 1]

### **The pre-Roman development of the dyke system**

[Figs 2.28-2.31]

The belief that occupation on the Gosbecks site, with the pair of short dykes that protect it on the west, had begun before the construction of any of the longer dykes, is presumed by Mr Crummy in his account of that site on pages 95-105 (introduced above on p 7-8), and this can be confirmed by its second line of westward protection, ie the south part of Heath Farm Dyke. For the middle and the north parts of this, throughout continuous, take it very much farther, to have its filled-in ditch covered over, near the SE end of Bluebottle Grove, by the rampart of Lexden Dyke; and running on northward beyond (Fig 2.28). Thus its purpose was to protect much more than the Gosbecks site itself, but a very big expanse of country, stretching south to the Roman River.

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15 The section (Fig 2.27) shows that the ditch was no more than about 4 ft (1.2 m) deep. This is far too small for a dyke and is more in keeping with a field ditch.

16 Since CFCH wrote this paragraph, the dyke has been shown not to continue south of the Maldon Road (pp 170-71).

17 The Shrub End Dyke is probably Roman too (see p 171).

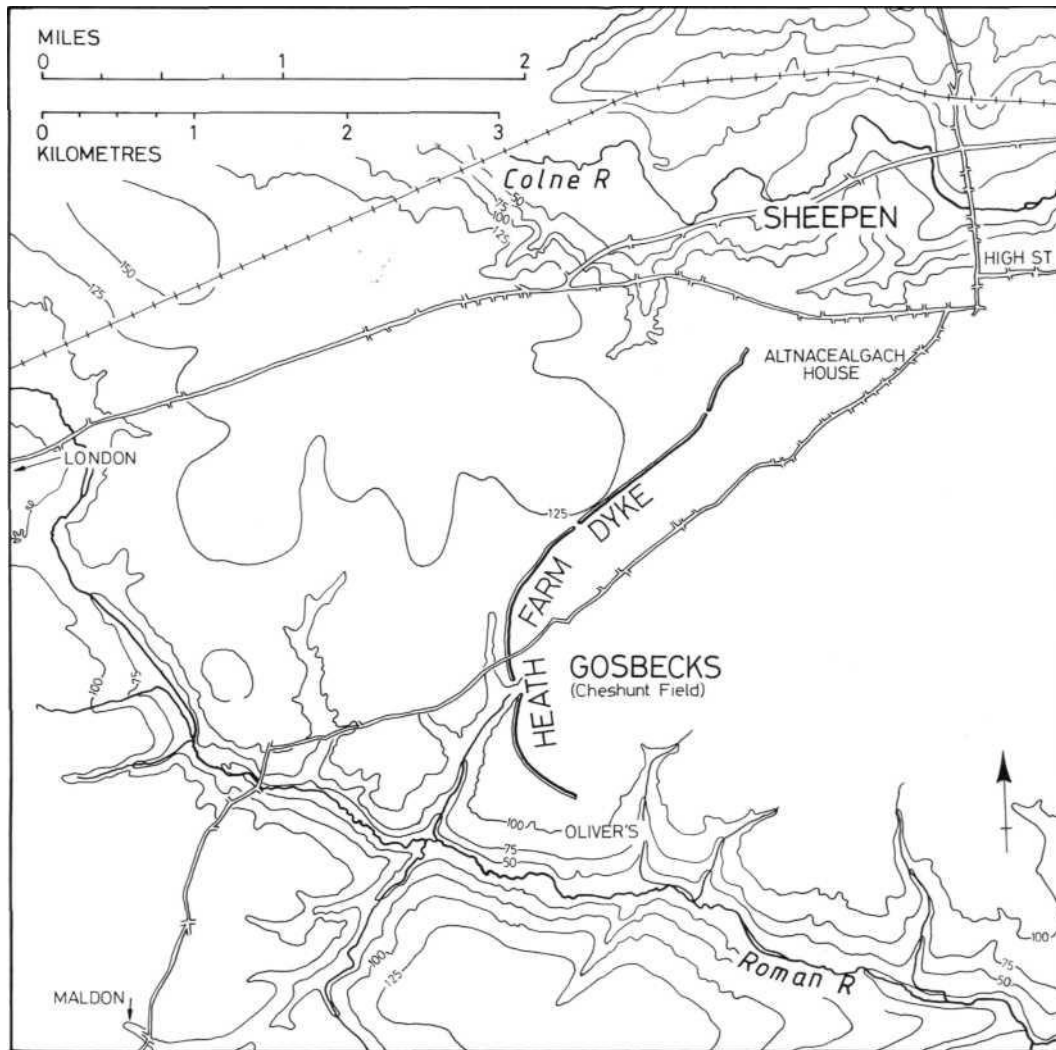


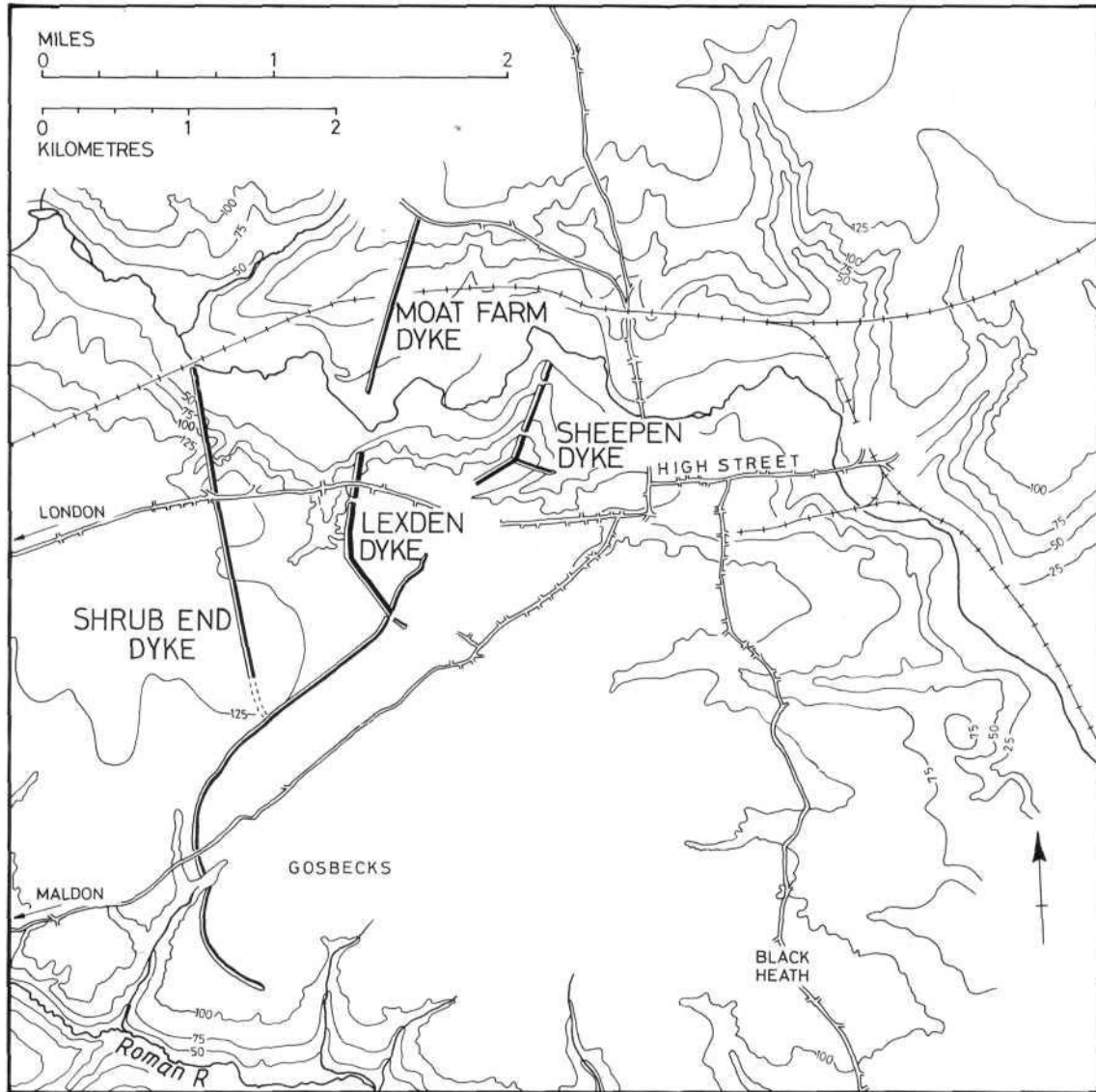
Fig 2.28 The development of the dyke system: pre-Roman. [Page 52]

As the Shrub End and Lexden Dykes both have an overall plan like Sheepen Dyke's, they clearly should share its initial date of AD 10 or so (Fig 2.29). The three thus formed a system with its base along the Colne; whereas Gosbecks looks to the Roman River, southward, Heath Farm Dyke further protects a great southward expanse of open land—a purpose that was cancelled through its cutting by the outer two Colne-based dykes, which also isolated Gosbecks. However, this was only within their system's first stage: at a time when probably neither had been completed with a south-facing front, there came a change of plan (Fig 2.30). This brought Kidman's Dyke, which began by reinforcing the protection of Gosbecks, but ended abutting on Shrub End Dyke (in Lexden Straight Road), thus linking Gosbecks up with the Colne-based systems—a link extended next by Prettygate Dyke, joining Shrub End Dyke transversely to Lexden Dyke. Now also, or soon, securing the south side of Gosbecks, will have come Oliver's and Layer Dyke,

facing west and passing across the Roman River; they will thus complete the works that belong to this second stage.

The change of plan that introduced it had no doubt a political intent: Cunobelin, author of the Colne-based works, was a Catuvellaunian intruder here; Gosbecks, older and therefore native Trinovantian, he first left out of his system but now brought in, to make for unity.<sup>18</sup> Lastly in a third stage, evidently late, came the plan for defending the peninsula complete, on the west in front of all the existing works, by the construction of Gryme's Dyke (Fig 2.31)—with Dugard Dyke behind it, short and near the middle—resting on each of the rivers upstream, where more than 2 miles (5 km) apart. Gryme's Dyke was designed in two parts. The longer, stretching southward, aimed at the middle, where the other, through keeping out in front of Gosbecks, had to meet it by a sharp turn inwards, at what now is Stanway

18 See pp 170-71 for another view on the relationship between Gosbecks and the Colne-based dyke system.



Green — where early surveys and the surviving portions of earthwork still leave problems (p 29). But as is certain now, the longer part's original build stopped short of the middle, leaving a gap — not filled until later in the century. A possible explanation would be a threat, not previously expected because it was from the east, from the sea, and called for the west-facing work to be halted, while east-facing dykes were built. These anyhow exist, and are Berechurch and Abberton Dykes. A Roman invasion, which might have been landed on the Essex coast, was certainly prepared by the emperor Caligula (AD 37-41), although in fact he called it off.

Summing up, it will be seen that the Colne-based system, taking its date from Cunobelin's Sheepen, c AD 10<sup>19</sup>, and shown to have cancelled the purpose of Heath Farm Dyke — so at first excluding Gosbecks, the older site, but afterwards bringing it in through the links of Kidman's and Prettygate Dyke — has given the key to the sequence. Once this is grasped, it becomes clear that Gryme's Dyke, and perhaps then with Berechurch and Abberton Dykes, must on every count come last (to leave the dyke system as shown on Figure 2.32 but without the Triple Dyke).<sup>20</sup>

Once the Roman army had captured Camulodunum

19 This is the date as originally proposed in *Camulodunum*, but CFCH below (pp 80-3) allows earlier occupation starting c 5 BC which is of a smaller scale and predates the Sheepen Dyke.

20 The constructional sequence of the dykes and their dating are still of course largely speculative. Some of the problems are discussed below (pp 174-8), where one of the many possible interpretations is put forward.



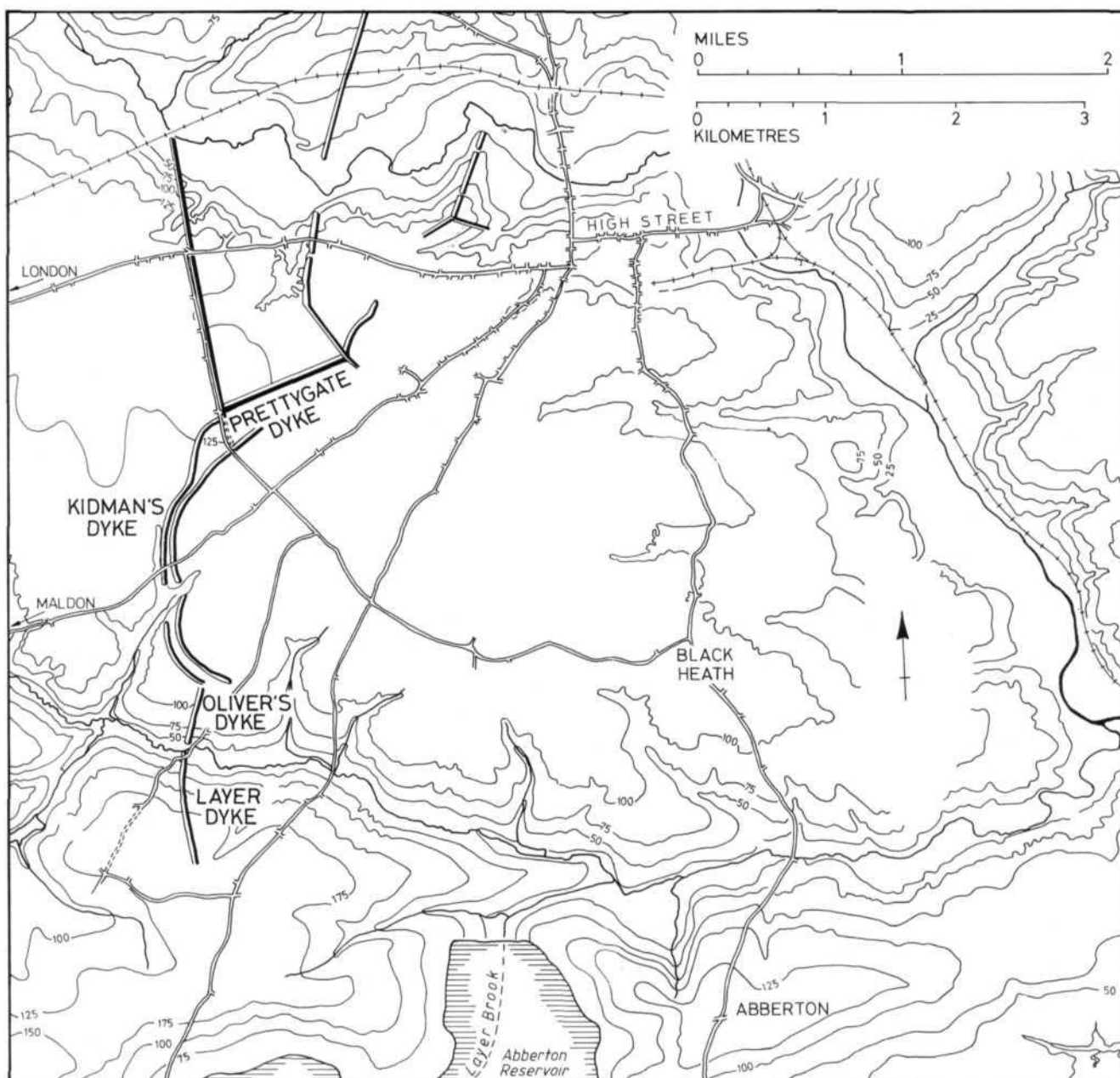


Fig 2.30 The development of the dyke system: pre-Roman. [Page 53]

(which would have been under Claudius himself and so still in the summer of 43), an immediate start will have been made on the destruction of the Sheepen Dyke since the site behind it was plainly the kernel of what Dio calls Cunobelin's 'royal seat' (his *basileion*; lx. 21,4). The levelling of the rampart (leaving only its base in a couple of excavated places) and the filling of the ditch with its materials will thus have been one of the first undertakings of our Roman Phase 1, however long a time it took. Several droppings of scraps of Roman military equipment (*Camulodunum*, 51, 335) show that soldiers were employed on this, less probably as labourers than as supervisors of the work of British captives.

### Roman Phase 1 (AD 43): treatment of the dykes

#### Triple Dyke

[Figs 2.32-2.35]

That Shrub End Dyke, along the east side of Lexden Straight Road, can be given a northern end (p 51). It must be there that Shrub End Dyke North will have begun, with the triplication first seen on one of the 1933 aerial photographs (p 22). A little north of Heath Road, in the belt of trees along Lexden Straight Road, it has its triple profile showing. For the next 275 yards or so (just

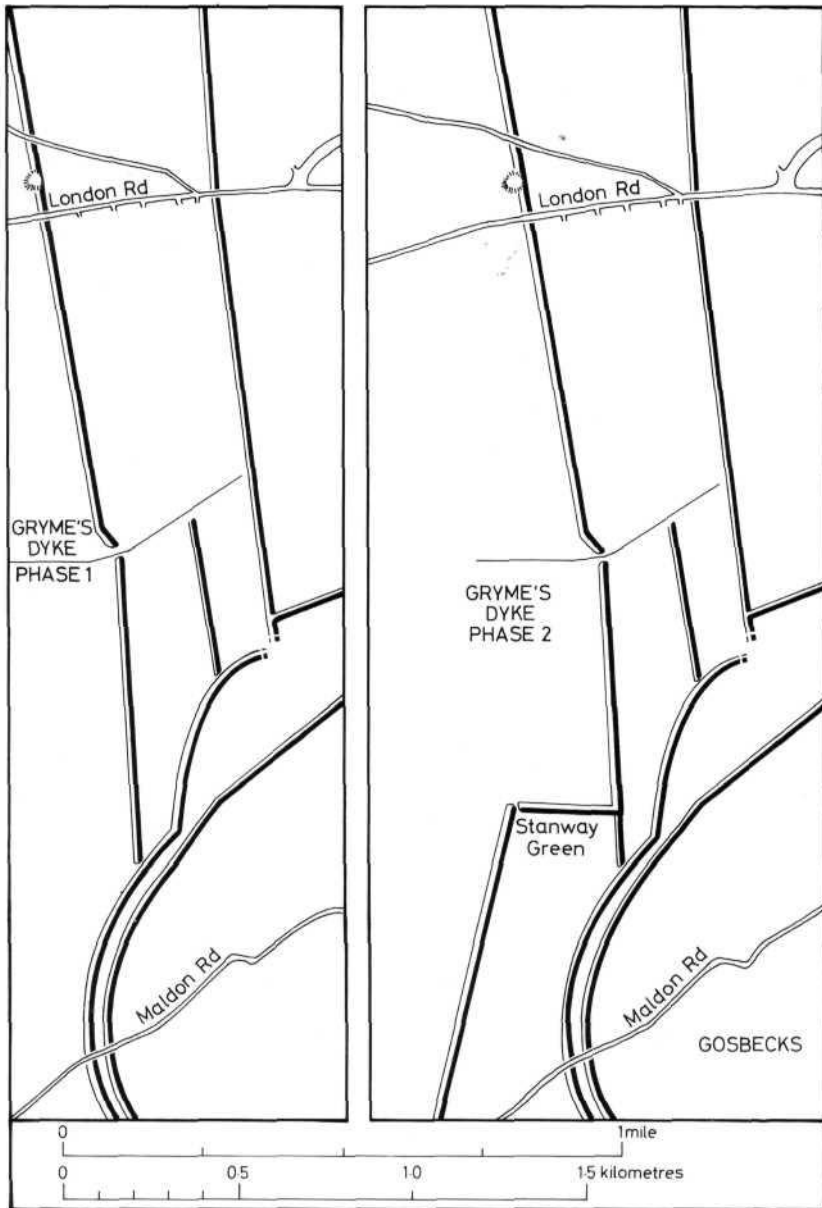


Fig 2.31 The development of Gryme's Dyke Phases 1 and 2. [Pages 53-4]

over 250 m), this profile remains well-marked: the Triple Dyke's only surviving stretch where that is so. Thence north to the Straight Road's ending on Lexden Road, beside the Halstead Road fork, it is levelled and almost all beneath houses and gardens; but in the field beyond Lexden Road's houses, the same air photographs showed again the triple ditches, now aligned 3° west of north, no longer N10°W, as along the Straight Road. From this slight change of line, one may certainly infer that where it comes at Lexden Road, Shrub End Dyke had had an entrance (as well have Lexden Dyke behind it), which was of course retained at the triplication.

The air photography starts to show the triple ditches about 117 yards (about 110 m) north of Lexden Road, and their continuance (with three short intervals only in their showing) straight on to Mott's Farm, beside the Colne just opposite the railway viaduct. The narrow oblong pond by the end of the lane running down to the farm must certainly represent the outermost ditch; and

on the right of the lane, the photography faintly shows the innermost. The Triple Dyke will thus have stretched for a distance of just one mile (or 1.61 km) from its start beside Lexden Straight Road to its end at Mott's Farm (Fig 3.32). The expanse between the Lexden Road houses and the fall to the Colne valley is open ground, spreading to the gentle eastern slope of Chitts Hill, and known as Hunter's Rough. Within it, at 227-230 yards (just over 200 m) from the north edge of Lexden Road, a section was cut by me in summer 1961 (aided by a team from the Colchester Archaeological Group headed by Mrs K de Brisay) straight across all three ditches. The trench was broadened to a breadth of 15 ft (nearly 4.6 m) to expose more of the natural surface between the ditches as well as behind the innermost one.

No disturbance of the natural sand was encountered; the banks it will have carried, doubtless similar in their profile to those besides the Straight Road, had had all

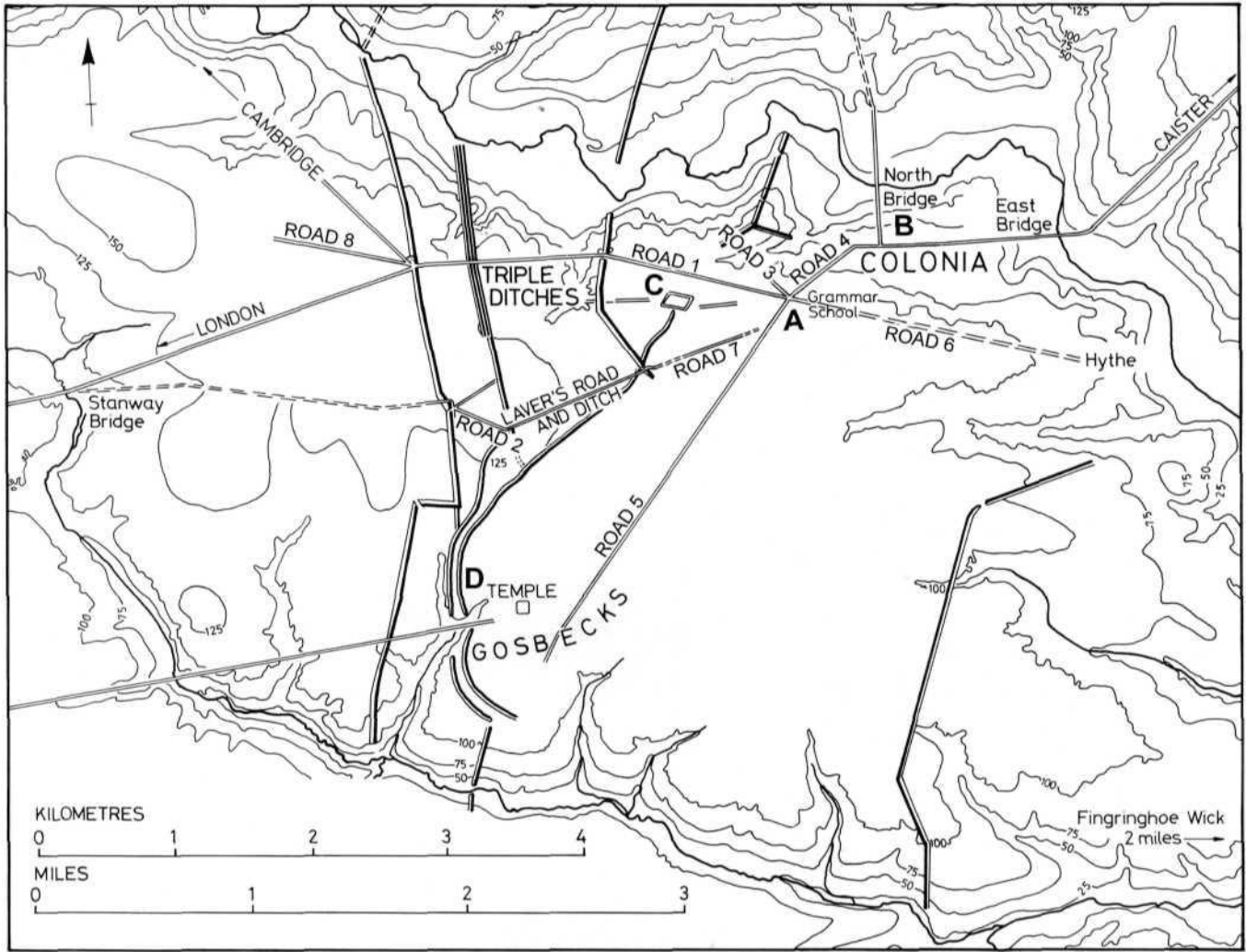


Fig 2.32 The development of the road system: early Roman. [Pages 55-9]

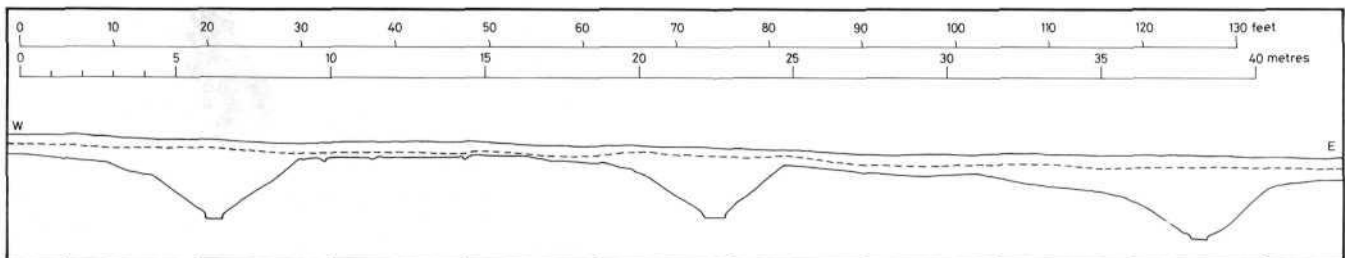


Fig 2.33 Profile across the Triple Dyke. Dug in 1961. (No 11 on Fig 6.1.) [Pages 55-9]



Fig 2.34 The 1961 section across the Triple Dyke.  
Above: viewed from the west.  
Right: the west ditch viewed from the north-west.  
[Pages 55-9]

their material used for re-filling the ditches. The profiles of these, however, gave the essential clue to the relationship of the Triple Dyke to the original single Shrub End Dyke North. From the section across them all, here drawn out as Figure 2.33, Mr Crummy was the first to perceive what it was: the innermost ditch has larger dimensions than the other two, which can only be explained by its having belonged to Shrub End Dyke North, while the other two ditches, and the vanished banks between them, had been added to effect the triplification. Of the original single rampart hardly anything remained; but the whole length of the section was 122 ft (37.2 m) measured across the three ditches. The original ditch breadth occupies the eastern 18 ft (5.5 m) of this (the depth of the ditch being 7 ft (2.1 m); in front of it, each of the spaces for the bank measured 34 ft (10.4 m) across, and the central and western ditches were 17.5 and 18 ft (5.3 and 5.5 m) wide and 6.0 and 6.5 ft (1.8 and 2.0 m) deep respectively.

The Roman date of the triplification suggested here by the flattened base of all three ditches<sup>21</sup>, is confirmed by the position that its start will have had: this is the point, beside Lexden Straight Road, from which there must have run, slanting off to WSW in a dead straight line, the ditch that had its other end cut through the rampart of Gryme's Dyke Middle, ie our palisaded earthwork (pp 59-60).<sup>22</sup> Not only where we sectioned the latter was the pottery in its filling certainly Roman, but through it was cut the side-ditch of Roman Road 2. And as that road, though never completed further on behind Shrub End Dyke, belongs to Roman Phase 2, the palisaded work must belong to Phase 1. So too then will the Triple Dyke: with the palisaded work diverging exactly from its starting-point, the two together would be parts of the same design, undertaken directly on the capture of the British defences.

The purpose of the Triple Dyke, viewed in that context, can only have been protection on the west, from any counterstroke by Britons, of all the ground behind it: down to the Colne and back to include the fresh water supply of Lexden Springs. For within that ground, in the tented encampments used wherever urgency required them, would the army — or much of it and possibly some of its Commissariat — best have been given its quarters. The labour of the triplification (no doubt performed partly by prisoner labour under army supervision) would save it that of fortifying individual camps for its units, in this earliest phase — quite late in the summer already. These units might be expected to have been two legions; any special bodyguard for Claudius will have departed with him after his very short stay, and the legions would be the Twentieth and probably the Ninth, which in the second phase will go to the inland side of eastern Britain, while the Twentieth stayed here (as later sequels will imply). The protective dyke's triplification, apparently unique, suits the

inherent pre-eminence of legions; so will the water of Lexden Springs, as the purest available supply. (On encampments for the army's auxiliary units, see pp 60-61.) For the long strip of ground outside the Triple Dyke, protected on the west by Gryme's Dyke North, must be considered in this same context; but it involves the earthwork here to be separately treated below, that of the palisaded earthwork.

[PC: CFCH's site notebook records the discovery of fifteen iron hobnails (Fig 2.35) in the fill of the central ditch. 'They were all together in such a way as to suggest their being *in situ* in their places in what had been the sole — now wholly vanished — of a leather boot or sandal.' They were found at 84 ft 4.5 inches from the west end of the trench, 39 inches in from the north face, and 74 inches down. The find-spot cannot be marked on Figure 2.33 since it does not show the west limit of the trench. However the measurements do suggest that the hobnails were within a foot or so of the bottom of the ditch.]

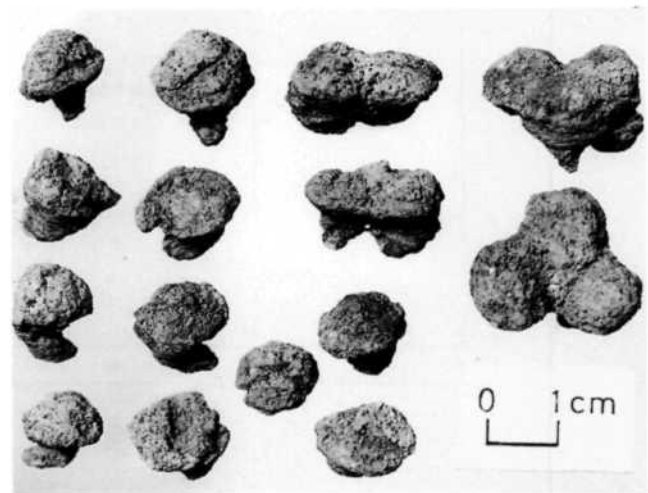


Fig 2.35 The iron hobnails found in 1961 in the central ditch of the Triple Dyke.

### The palisaded earthwork

[Fig 2.36]

How this work supports the Roman Phase 1 dating of the Triple dyke has already been seen above; that the two belong together, topographically viewed, can be supported both from its placing and the expected use of the ground that it protected on the south: the long strip mentioned just now, stretching north to the Colne. From the placing of its eastern end, at the start of the triplification of Shrub End Dyke North<sup>23</sup>, we may turn to consider its relationship with the Gryme's Dyke entrance, at the NW corner of what now is the Dugard Estate. Its ditch's western end, cut through the rampart of Gryme's Dyke Middle (Fig 2.36), is 130 ft (40 m) south of that entrance. This allows sufficient space, on the right-hand

21 See p 174 for flattened bases as indicators of date.

22 It is now certain that the 'palisaded earthwork' could not have linked up with the Triple Dyke but would have reached the line of it where it was single, ie at the Shrub End Dyke (p 172).

23 See previous footnote.

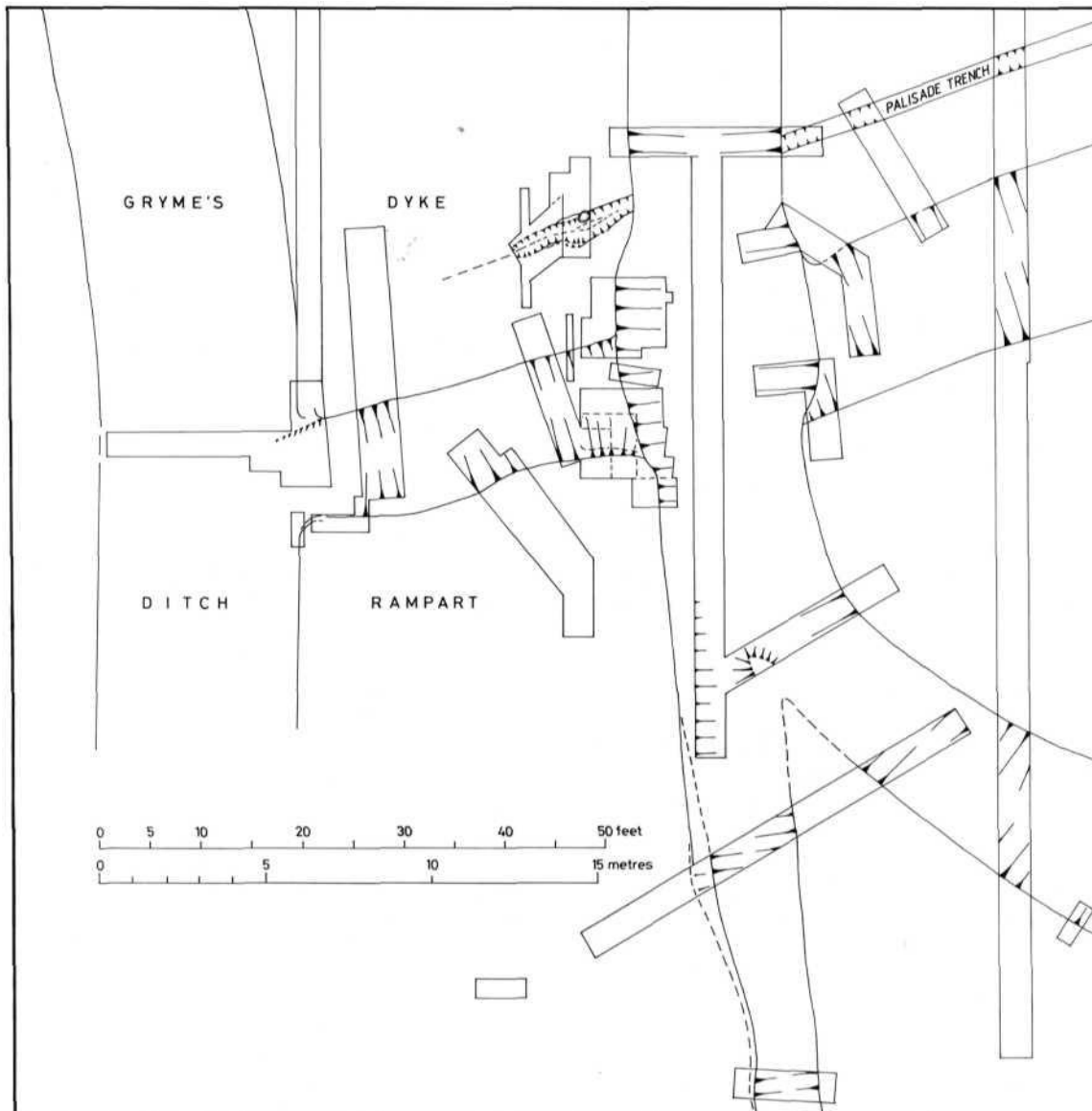


Fig 2.36 Plan of the 1946-58 excavations just inside Gryme's Dyke, in the NW corner of the Dugard estate. (No 19 on Fig 6.1.) [Pages 59-61]

side of the inward way from the entrance, for the breadth of the rampart supported by the palisade in front, beside the ditch, and for the usual short interval between its back and what in normal forts, or camps, was the *intervallum* roadway — in the present case the way from the Gryme's Dyke entrance, which in this earliest Roman phase will have kept its double gate and gateposts, into the southern end of the long strip of ground stretching north, between Gryme's Dyke and Shrub End Dyke North (now in process of triplication). Within that stretch, beside or on the line of Lexden Road, the vanished entrances through both will doubtless have been maintained likewise, awaiting their levelling, in the next phase at earliest, for Roman Road I. The northward distance from those to the Colne brings the total mean length of the stretch to a mile and a half (some 2.4 km); in mean breadth, between the two dykes, it is a little more than 400 yards (365 m).

While our palisaded earthwork protected the southern end of this, its northern end, open to the Colne, provided for water-supply from the river. And with this we must consider again the Roman army's commissariat: whether or not there were wagons, it must chiefly have used pack-animals, and of these the total numbers will have been huge. For however much of the supply-trains might have been kept further back, at the presumable base at Chelmsford for the final advance upon Camulodunum, the troops that made this advance — and captured the place with the Emperor briefly in command — would have to stay here, and be kept supplied, until their numbers could safely be reduced, which would only be at the end of our earliest phase, as we saw when considering the legions. But besides the commissariat we have to remember the auxiliary units. Just as sometimes on battlefields auxiliaries were put in front of legionaries, to be the first



for the enemy to engage (eg Tacitus, *Agricola*, 35, 2), so here it could be opportune to station auxiliary units, perhaps more especially cavalry, in this long strip of ground, as the first defensive line and also as a guard for commissariat. Carts and wagons would be parked in the south of it, while in the longer stretch, open to the Colne on the north, would be the picketed lines of animals — less often mules, most probably, than stout requisitioned British ponies. And as for watering at the river, its slanting course from New Bridge down to Mott's Farm, which gives at least 1,000 yards (915 m) of water, allowed the men their drinking water (and any to be taken for storage) well above the animals, while its use for the men's sanitation would be drawn from a well. (Compare the case of the Hod Hill garrison in Dorset, for which Sir Ian Richmond's luminous little study of river-water use, on the Stour, should stand as a classic (Richmond 1968, 66-7, with 71-2 on the Roman water-gate).) Downstream from Mott's Farm to the Lexden Springs inflow, the river's course was no doubt less winding than now, but will still have been not much shorter than a mile; thus its water, on from Lexden Bridge at any rate, could have been clear enough again to be fairly drinkable; for animal watering, next below, and for men's sanitation lower. That of the Springs themselves, the purest, we have accounted for already.

On the total number of animals which would be expected to serve the commissariat, and the quantities of fodder they would need (as would also the cavalry horses), besides the rations for the men, the most recent estimations are those of John Peddie (1987, 28-32, 183 ff, and 197, using Breeze 1983, 10-14 and Richmond 1982, 7-15). The figures he gives for the whole army of conquest will allow us, for our expected two legions plus auxiliaries (say some 17,000 men), transport animals totalling more than 2,000, made up at any one time of those here and those on the way to replacing them. Even if these others might be two-thirds of the total, this would mean some 750 at a time waiting here. And although this proportionate figure is speculative only, it may well be a minimum: the calculated total stems from Peddie's estimations, and the detailed and annotated table on his page 31 completes what perhaps is his book's most memorable chapter.

At all events, any large numbers for transport animals, for carts as well, and allowance for auxiliary troops besides, must suit the extent and location of the long strip of ground here proposed for them. Moreover, the palisaded earthwork, giving its southern end protection and also ingress from the Gryme's Dyke entrance, can only be of Roman Phase 1. Cutting through the Gryme's Dyke rampart, and itself cut through by the Phase 2 Roman road's side-ditch (Fig 2.36), it can have no other date; and this accords with a design for it in one with the Triple Dyke, the building of which has no apparent purpose outside of this same Phase 1. The phase turned out to be brief; Plautius and his staff, finding that the outer native dykes could be treated to suit the army and its services, will surely have pleased their Emperor by what they would do with them. And if this is not to be matched elsewhere, as it seems, our explanation of it should be none the less entertainable.

### Prettygate Dyke reversed

[For the digging of a ditch on the south side of the dyke, see Figs 2.24 & 2.25]

Prettygate Dyke, where it starts at Peartree Junction, branching inwards off from Shrub End Dyke Middle, is about 440 yards (400 m) south of the start of Shrub End Dyke North and thus of the Triple Dyke; and its end, at Prettygate Junction, is twice that distance south of Lexden Springs. The area within it, south of the Triple Dyke, was protected on the west by Shrub End Dyke Middle still a single work only. Yet no contemporary earthwork is known that would intervene between this area and that protected by the Triple Dyke, where we have positive Phase 1 encampments for the expected two legions. This southerly area, with only single-work protection, may accordingly best be suggested for the encampments of auxiliary units which were not required for guarding commissariat, as proposed above, in the long strip of ground directly to westward. Much further, indeed, from the water of the Colne and of the Springs; but unless left empty — the only alternative apparent — this suggestion does seem to be the best for it, at least within the duration (actually short) of our Roman Phase 1.

### Roman Phase 2 (AD 43):

#### evidence for inferring a quickly-cancelled military base

#### Purpose for such a base

Not a great way north of Colchester and the lower Colne valley was a border between the Trinovantes and the Iceni. This people had not surrendered, as had others, during Claudius' visit, so that what would now have been planned for was a theatre of war with them. Dispositions for troops, to serve such theatres of war in Britain elsewhere, have in the last 30 years become increasingly clear (Todd 1968; Webster 1970; Frere & St Joseph 1974; Webster 1978; and Salway 1981, 95). In the 1974 publication of the fortress at Longthorpe, on the Nene to the west of Peterborough, this in its early period was shown to be of roughly half-legionary size, for part of a legion together with a unit of auxiliaries, forming thus a *vexillatio*. Such 'vexillation fortresses' were mapped there, and listed by Frere (1987, 211); though few may be as early as Longthorpe, they were campaign-groups' bases for wintering — their sizes, usually c 20-30 acres, show how the groups would vary in strength. This purpose also explains the adaptations recently found, for such a group including cavalry and its horses, in the legionary fortress of c AD 55-67 at Usk in south Wales. Any of less than 20 acres, possible still, should be at least three times the size of a fort for a single auxiliary unit, or for a composite garrison of roughly equivalent strength. These smaller forts, whether farther from the fortresses or nearer, are of

sizes that vary from less than 3 acres to 5 or more, but seldom more than 6 (one of 10 is abnormal). Suggestive general maps of the dispositions under Claudius are in Webster 1970 (180, fig 1) and 1978 (92, fig 4); that in Frere 1987 (p 56) is more widely pre-Flavian. An Icenian war in the very first years of conquest might be served by more bases than one, but our Colchester area was at all events an obvious choice. It was easy for keeping provisioned, by land and also by sea, and was held from the outset already, by the camped-out troops that are implied by the treatment of the dykes explained above (pp 55-61) as of Roman Phase 1, the outset of which, as reckoned here below (p 67), being datable to August 43, directly on native Camulodunum's capture.

The native inhabitants had two chief sites of concentration: at Sheepen and inevitably at Gosbecks, one to north and one to south of the west-east line of the Roman advance. Thus while at Sheepen the immediate task was that of reducing its dyke to ineffectiveness, the west-east line could have a road laid out for it (Road 1 in our numeration, p 68) along the eastward course of a native trackway: both alike heading for the tidal Colne's waterside at the Hythe. But the Roman surveyors of the line for Road 1, at the outset, prior to any planning of a regular fortress, would need to sight a line to the Colne that would serve for an expected advance across it northwards. And if a force for this were to occupy a 'vexillation-fortress', closely south of Road 1 because the ground was there more level, they would most usefully start their line from it opposite a gate through the north side of such a vexillation-fortress. If the line were sighted on the part of the Colne where now is North Bridge, a direction for it at N37°E would let it start from Road 1 at a point soon afterwards used for two branch roads that were actually built, and for a third, of which the known long stretch, farther off, is directly aligned on this same point and at the same 37° of inclination. This point on Road 1, thus of cardinal importance, we have called 'Point A' (see Fig 2.32). But the only possibly credible trace of a side for any vexillation-fortress, though on a west-east line just south of Road 1, is an unfinished ditch which excavation could not pursue, owing to modern use of the ground, eastward farther than some 300 ft (90 m) short of A. Called 'Camp ditch' by its discoverer, A F Hall, we discuss it below (pp 64-6). Its unfinished state, like that of the earthwork traces to be described below (pp 62-4) and claimed as of intended smaller forts, shows of course that any plan involving a vexillation-fortress was quickly given up, so that our Roman Phase 2 for it was brief. Nonetheless, Point A's position does call for explaining; and while the conjecture offered here for it may be dismissed as too theoretical, one has otherwise to rest content with not explaining it at all.

Yet that Claudius ordered Plautius to subdue the unconquered tribes, as was natural, Dio records (Ix. 21, 5). Beginning on the Icenii, being the nearest, has to be likeliest, so that war-preparations for this would have been the promptest. And their coming to terms soon after

and still in 43, letting any war-base here be quickly cancelled, is historically admissible in view of the recognised sequels. To these we shall soon be coming on, under Roman Phase 3. Any fort or forts elsewhere in the Colchester area, smaller than what Hall expected here, so of auxiliary size, whether unfinished, never occupied, or held for a longer time, could have been meant to be elements themselves in such a base for an Icenian war.

## Auxiliary forts for Roman Phase 2

### i) Unfinished fort under legionary fortress

Within the oldest part of Colchester, and predating its legionary fortress, are the traces marked by Crummy of the 'earliest fort??' (CAR 3, 5, figs 4-5; repeating Crummy 1977, 69-70, fig 3). One is in *Insula* 10, a deep ditch running south to north, the otherwhere *Insulae* 12 meets 20, a turf rampart expected to have paralleled the ditch. Though the ditch was sealed only by rubbish, from Boudica's sack of the *colonia* that had followed the fortress, the turf ran beneath and across a known street of the fortress itself. Crummy's words on both were 'here... regarded as having been part of a fort, which... presumably preceded the main military base', ie the fortress. He added, 'However, the true situation may have been more complicated'. But the true situation should be that this fort, meant for auxiliaries, was left unfinished, and superseded at once by the fortress. As this itself can be reckoned undertaken before the end of AD 43, it is no wonder if only two traces of the fort have been recognised. They are marked B on our Figure 2.32 (CAR 3, 4, fig 4). In size the fort could have measured about 400 by 600 Roman feet (*pedes Monetales*; cf CAR 3, fig 6), within the ditches, or some 2.3 hectares, barely larger than the fort to be noticed below at Gosbecks.

### ii) Unfinished fort west of Altnacealgach

[Figs 2.37 & 6.13]

It was Hull who came upon the portion of this that he then explored, and published in *Roman Colchester*, pages 271-3 (numbered 17 on his pl 43 and C in our Fig 2.32). In 1939 he dug in what was then field 1222, where a projected new school might overlies an imagined extension of Sheepen Dyke, resuming its Ditch 1A (p 51). The field was the next one west of the grounds of Altnacealgach House, lying south of Park Road. Finding nothing, he dug in the field west again, 1225. And in two cuts here, at roughly N40°W, he discovered what his section (*Rom Col*, 272, fig 117), here reproduced as Figure 2.37, shows unquestionably as a Roman military ditch, with shovel-trench at base<sup>24</sup>, 9 ft 7 in (2.9 m) from the modern surface, and allowing

24 See note 21 (p 59) on the dating value of this kind of ditch profile. The presumed shovel slot here is about 3 ft wide, which seems



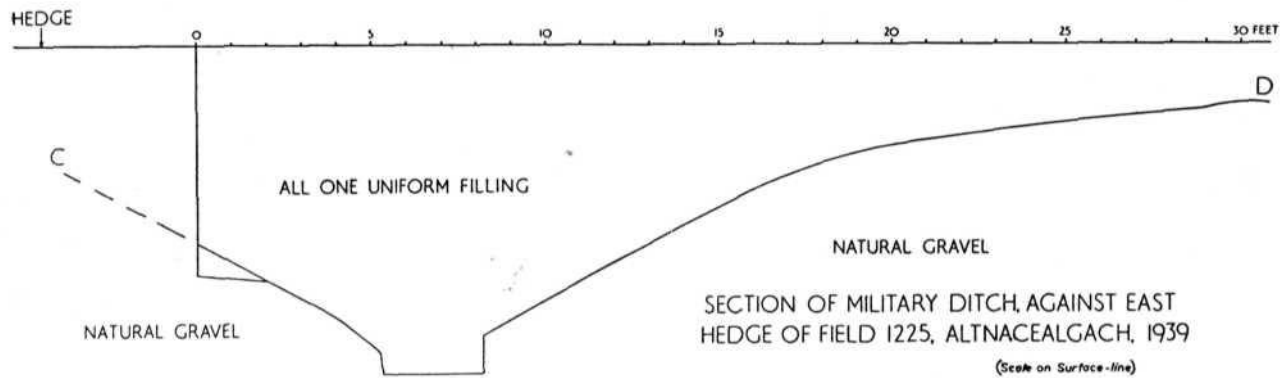


Fig 2.37 Section across the 'military' ditch at Altnacealgach, dug by M R Hull in 1939. Reproduced from Roman Colchester, fig 117, courtesy of the Society of Antiquaries of London. [Pages 62-3]

for the contemporary surface, an original width of not less than 32 ft (9.8 m) (though not all the trench was carried right down). The filling was a uniform mixture, and small early Roman sherds were lying in the bottom. It was aligned N35°E, but at its southerly end was bending in a curve, which a test trench confirmed, on to an alignment approximately W20°N (*Rom Col*, fig 116). A corresponding bend, on the north and west sides of the field, was made by the long narrow plot 1224, its surface being sunk as if for a similar ditch beneath. In that case, Hull could reckon the area intended within to be 2.84 acres (1.15 ha), and its shape to be rhomboid, with angles of 105 and 75 degrees, rounded in the usual Roman military manner, whether all of them were finished or not (*Rom Col*, 273). The size would accommodate a quingenary cohort; the rhomboid shape, however, was harder to explain.

Its north and south sides could have been meant to be parallel with the line of the Roman Road 1, running less than 200 yards (180 m) away on the north, but why should the other two sides have been set so slanted? The likeliest answer seems given by the duplicated ditch of Heath Farm Dyke North (p 32), running up to end at the SE rounded corner of the rhomboid, on a line that differs scarcely (by at most 2 degrees) if at all, from that of the slanting side beyond. If we assume the twin ditches of the dyke to have been traceable still on the surface, at the rhomboid's Roman date, the slant can simply have prolonged their observed alignment.

The date's being Roman is certified, as stated here above, with our Figure 2.37 of Hull's section across the ditch of this side of the rhomboid, by the flat-based shovel-trench sunk into its bottom, characteristic of Roman military ditches — and here moreover yielding some early Roman fragments of pottery. The shovel-trench's presence, in a ditch of this size, rules out the agricultural explanation suggested in 1977 by Crummy (1977, 90). That the intended fort was never finished was disclosed by a sewer-trench cut in 1955 across the eastern half of its expected area, when no ditch was found for its north or south sides; only an east-west ditch that was discovered in the SE corner of

this field 1225 remains puzzling (*Rom Col*, 273, note 1).

Yet even so, the plan for the fort can have included an annexe on its east side. In 1985, when the school building project was revived for extending the Colchester County High School for Girls (fronting on Norman Way) into the adjacent field 1222, Mr R J Martin, the acting clerk of works for this, observed the exposure of a set of three parallel ditches, which Hull thereupon inspected, taking measurements and notes (see no 57 in our catalogue). Not sectioned down to the bottom, so that their depths remain unknown, their surface widths were around 4 m (13 ft), with intervals of nearly 7 m (about 22 ft), making 25.5 m (84 ft) across the set (see Fig 6.16 on p 127 which is based on Mr Martin's plan). Being aligned, W38°N, at almost 90° to the fort-ditch ahead of them, they would have ended on it, close beside its corner, if carried so far; but as Hull in 1939 saw nothing of them there, one may expect that they too were left unfinished. Their measurements would suit their designing to protect a bigish annexe to the fort, most likely for a wagon-park. And the non-completion of both can point, here again, to their having been meant to be part of a war-base: quickly countermanded when the Colchester fortress was decided on, and therefore still within 43.

Apart from the ditches, and at the field's south end, at A and B in Figure 6.16, was a line of post holes, with a ditch outside its end and pottery dated by Hull to the 2nd century (see no 57 on p 127). Of the few sherds found at the triple ditches' site, none need have come out of them. In the east of the field was the amphora-burial found in 1907 (*Rom Col*, no 21 on pi 43 and p 293). Second century, on the basis of pottery, was the deep pit at C of our Figure 2.37 and also D, while at W what looks like a wall-footing (band of ragstone chips) probably relates to what are noticed here last: the findings of an excavation in the field in 1955, prior to the clearance for the school, by Mr (now Professor) John Wachter. His report is given here on pages 156-8. To summarise, nothing other than the unfinished triple ditch can at all approach the estimated early date for the unfinished fort at Altnacealgach.

much larger than might be expected.

iii) *The fort at Gosbecks*

[Fig 5.4]

This fort, in Well House Field (Stanway; Point D on Fig 2.32), only discovered by aerial photography, is described as fully as is possible without excavation. In area some 2.2 ha (5.5 acres), it is almost square, with at least two rounded corners. One side follows the line of Heath Farm Dyke Middle, where it has Kidman's Dyke Middle along its outer side. Whether this part of the Heath Farm Dyke rampart had been levelled for the fort to cover with a normal west rampart, or whether it still remained standing, for the fort's north and south ditches to abut upon, needs excavation to decide. Its size would suit perhaps a mixed garrison, horse and foot, and it could have been meant to serve the war-base planned and quickly cancelled, as we suggest, in 43, and then have been retained for what its purpose will have always included: a watch on the inhabitants of Gosbecks, whose main enclosure lay just SE of it, 350 yds (320 m) off. Our introducing it here, and more thoroughly describing it below on page 67 under Roman Phase 3 may thus be readily understood. Yet whether the unfinished two forts (under the legionary fortress and west of Altnacealgach) were the only adducible other intended components of a quickly-cancelled plan, remains to be enquired, with a vexillation-fortress in mind, in the next section here.

iv) *The supposed camp on the line of Shrub End Dyke*  
[Figs 2.10 & 6.1]

The quadrilateral enclosure in the 18th-century surveys, at their southern end for Shrub End Dyke, and also what the first of them, Lufkin and Smith's, called a 'Roman Camp' with its 'entrance' at their point 4, have both been noticed here above. So has the 'Roman Camp' marked at that same point by Henry Jenkins (1842, pl 30, whence our Fig 2.10), with at its corner the 'Roman milliary or beacon', the 'supposed site' of which the Ordnance Survey maps were still naming there in 1921 (p 20). From what actual traces these might have been inferred, one can only guess (again p 20). Thus any notion of a real Roman camp, or rather fort, which might appear to suit our context here, along with the foregoing, can have its dismissal now re-affirmed. The most that might be imagined is a fort just begun and quickly abandoned unfinished, like that under the legionary fortress, but nothing positive allows even that.

v) *Features explored at the Grammar School: re-consideration*

Of these, disclosed almost wholly by A F Hall, those relevant here are on the one hand Roman roads; the main one, heading eastward, our Roman Road 1, will be introduced first; attention to the other four (Roads 3-6) will follow our dealing with the features of importance on the other hand: Roman too, but of ditch-like digging in the

gravel beside Road 1. The Roman roads in the district will be reviewed below on pages 68-9. Meanwhile, Figure 2.32 shows the five which enter the Grammar School grounds, and in part the figure supersedes that published by Hull in *Roman Colchester* (p 5, fig 1), of Hall's exposures of all his ditches. Of Road 1 it will here be enough to say that its course with three tracks, and a flanking ditch of its own at least on the north, was given it only after it had served with a single track, the metalling of which, under that of the middle of the three, was that of the road in its primary Claudian form. The three-tracked form is of later 1st-century date (*Rom Col*, 274 and Section 3 in fig 2).<sup>25</sup>

Its fairly small north ditch ran parallel with its middle track's edges; outside of its south track, however, was a ditch that was larger, in the stretch that Hall cleared, and was not quite parallel. Its alignment was E11.5°S, so if prolonged that way with its full width open, it would have had to encroach, fairly soon, upon the south track's margin. The gully or slot that appeared within this track, running 10 ft (3 m) north of the ditch (his CD/10-CD/8 on *Rom Col*, fig 1), whether equally early or later, was parallel with that, not with the road. That the ditch's line was planned before the road was first metalled seems implied by their difference in alignment (3.5°). Both would be early Claudian, but if the ditch were planned to bound an enclosure of the size of a military camp, its alignment would be given it not by the road, but by the enclosure's whole design, laid out from the centre — no matter if this made the north side's line not parallel exactly with the road's. The suggestion advanced in 1947 of a 'camp' (*Camulodunum*, 20) disregarded that. But how conjecture might further support it is suggested below (pp 65-6).

What has first to be firmly stated is that none of Hall's findings sufficed, by themselves, to show any sign of an area's becoming enclosed. Hull was right to conclude that they had failed to (*Rom Col*, 274). The one large ditch could have marked an enclosure's north side, but a proof would need some sign of it farther south. None was found when a long trench was cut there in 1928, SW from Wellesley Road and west along Creffield Road and Queen's Road (*Rom Col*, 274; *CMR* 1929, 23-4), and signs of a military presence have been absent everywhere.

Of the 'camp ditch' itself, Hall secured the main alignment: its north lip twice at his CD/7 and both lips west from CD/8 to CD/10. They were wide enough apart for the hoped-for military ditch, some 16 ft (or 4.9 m), but the profile obtained between them, 5 ft (1.5 m) deep, was not of the military V-shape. It was rounded, so if meant for a V could have never been finished. The material filled into it (earth from the south side, sandy from the north) was sealed by rubbish in which the earliest sherds of pottery were middle 1st century, and this again by a second rubbish layer with oyster-shells, broken Roman tile, and late 1st/early 2nd-century pottery. I inspected the whole of the ditch's exposure and dated the pottery.

25 The three-track road could be formed by the convergence of three, or maybe two, different roads as shown on Fig 6.1.

Farther east, at CD/8, the ditch was interrupted as if for a narrow crossing. Hall exposed the ends of the ditch formed by the interruption. The crossing had a vertical face to the east, while the west edge was taken down in wide steps. In the brown layer over the top were two coins of Domitian (AD 81-96) (*Rom Col*, 273).

While east beyond CD/7 old modern foundations had disturbed the ground, farther west beyond CD/10 there was a further discovery. Hall, here at CD/1, found the tip-lines not continued, but instead the area crossed by a slanting cut, as if representing the north lip of the ditch turned to the SW. The edge of the cut, with bottom at 5 ft (1.5 m) down, was vertical. If therefore this belonged really to the ditch, it can only have been thus cut down by subsequent digging. Such an operation could also explain the deformation already described of the ditch on either side of CD/8. Parts of the unfinished ditch can thus have been used as a quarry for gravel; and gravel would be needed in the late 1st century, for installing the main road in three-tracked form. But if the ditch were never anything but a quarry for digging gravel, why is the main alignment of its lips not truly parallel with the road's, but 3.5° off it? The difference must either be dismissed unexplained or as fortuitous, or else the purpose of the ditch, cut so early between straight lips, should have been military — which the profile was never to show, because the ditch was never finished.

The south-westward slant of the lip, at CD/1, was taken as the bending of a rounded NW corner for a military enclosure, but it would prove one only if the ditch continued southwards. It could not be continuously cleared that way, as the CD/1 trench had to be one of a very small number, cut in the back garden (no 17 Beverley Road) of Mrs P Green JP, by her generous permission. So the next one south was dug 10 ft (3 m) away, supervised for Hall by a visiting friend of Mrs Green's: he was Reginald Smith, the Society of Antiquaries' director. Under 6 ft (1.8 m) of soil that seemed disturbed there was ancient filling; but pottery in it, at 7.5 ft (2.3 m), and near the bottom (10 ft; 3 m) (this of Collingwood form 37, *Rom Col*, 40), showed it was late 1st century at earliest. (All this is summarised by Hull, *Rom Col*, 274; the garden is the next one west from the School's Gurney Benham House.) So here again, later than a date for any military ditch, there had been digging, and presumably for gravel. Yet 80 ft (24 m) farther south, beside Gilberd House at CD/11, Hall's east-west trench, though long, found nothing at all.

But the CD/1 trench, begun about 30 ft (9.1 m) west into the garden, had to be prolonged for some 10 ft (3 m) east before it came upon the ditch-lip; till then there was natural gravel, at just over 4 ft (1.2 m) down from the surface and pierced by a single post hole. As this was outside the enclosure's NW corner that Hall was expecting, he gave it no further attention, and neither did Hull. Yet the failure to prove a corner may allow of a different explanation. The slant of the lip, at the end of the stretch of the ditch, might indicate an entrance. Its causeway of natural gravel could have carried two posts, flanking the approach to its intended timber gate,

the eastern of them in the hole that Hall uncovered. He did indeed cut a pair of parallel trenches at CD/3, just north of CD/1, across the line of what would have been the ditch's north lip if it had not made a SW slant; and while one met only an intrusive inhumation-grave, the other, to west of it, did find a lip. But in the little available space he could not test its ditch any deeper. So Hull could guess it to be only the south side-ditch of the Roman main road, here just clear of the 'camp ditch' slanting away at CD/1. Yet the road's south track, which it should bound, would then have had a ditch of 30 ft (9.1 m), not matching the 20 ft (6.1 m) width which is certain for the north track — and probably also for the south track a short way westward (as indicated by the locations of the road-side grave numbered 26 in *Roman Colchester* (pi 43), the centurion's tombstone near it, and that of Longinus, farther west (*Rom Col*, 253-4, pi 43). And Hall found nothing 23-24 ft (7.0-7.3 m) south of the Longinus tombstone, in his trench across a line prolonged from the known north lip of the 'camp ditch', at CD/9. The lip at CD/3 can then have belonged to a gravel-digging only. The obvious place for gravel when more was required would be the road-side anyhow, whether in the 'camp ditch' itself, or in an entrance-causeway through it. Hull wondered twice (*Rom Col*, 273, 274) 'whether it is an unfinished military ditch or a spoil-trench for gravel'. But the two are not exclusive of each other. They would be likelier successive.

We have already assigned to our Roman Phase 1 (that ensuing directly on the capture of the native stronghold) no regular camps or forts but only earthworks added to or altering some of the dykes. Yet while those remained in being — for only by them could the army be protected — two such forts, we have seen, were begun and never finished. One of them left traces to be covered by the legionary fortress (p 62), more than five years before spring 49 when its replacement, the first *colonia*, was put in hand (p 68). The other fort, near Altnacealgach (pp 62-3), left a ditch which is unquestionably military. So these require a Roman Phase 2, that with the fortress coming next, in Phase 3. In Phase 2 we can also put the starting of the fort beside Gosbecks which was clearly for keeping the natives there under control. But throughout the wide expanses around old Colchester, and within its delimiting dykes, no other Roman military enclosure has ever been brought to light, and there never having been any can now seem certain: none on any aerial photograph (whether vertical or oblique), and none perceived in the disturbances due to the spread of modern housing nearly everywhere. And if the 'camp ditch' was really to have been the northern ditch of one, nothing more of it has been found. Yet a quick cancellation of the plan agrees so well with that of the forts at Altnacealgach and under the fortress, that to assign this also to our Roman Phase 2 can seem allowable. Since together, had all been completed, with the 'camp ditch' site made a vexillation-fortress, they would exactly have suited a war-base. What more probably would this have been designed for by Plautius, in 43 as soon as Claudius had departed, than an Iceni war in 44? Hall's findings, in

the grounds of the Grammar School and near them, recounted in detail above, can just allow the 'camp ditch' the place that we suggest in that design, once one sees it as first unfinished, and afterwards deformed by quarrying for gravel. But the possible entrance at Hall's CD/1 (p 65) need not have been meant to be its only one. Beyond the ditch's easternmost exposure, there might have been intended a main north gateway; and if distant from CD/1 about 430 ft (131 m), this would be the gate above conjectured to explain why our Point A, directly outside it on the line for Road 1, was chosen as the start of lines for four branching roads, all but one of which were built in Roman Phase 3. These were Roads 3, 4 and 5, and they are dealt with here below (pp 68-9), as also is the much-debated Road 2. Meanwhile here, the 'camp ditch' question should now have been sufficiently re-considered — whether or not the result be sufficiently convincing.

### Roman Phases 1 and 2 in sequence: archaeology and history

The upshot of the foregoing sections may first here be summarised. The Iceni had not surrendered as soon as Camulodunum fell, for as we shall see, they still had their arms in 48. Claudius in 43, when some of the tribes were subdued by force and some others by surrender, had them *all* disarmed (Dio, lx. 21, 5), not the conquered ones only, as in the Loeb translation (the Greek has 'taking away the arms of *them*', meaning clearly 'of them all'). And Plautius, Dio says, was instructed to subdue the remainder by Claudius in person, when he ended his very brief visit. Of these, as the nearest to Colchester (north and north-westward) were the Iceni, war with them must have been immediately planned. It would need, here at least, a campaign-base, and thus presumably (from the ensuing case of Longthorpe) a vexillation-fortress, to which could be added some auxiliary forts. Yet any such fortress' traces suggestible here would show it cancelled when barely begun: as at the Grammar School discussed above. And of attendant auxiliary forts, the traces available for two of the three show the same, while for the third's being finished and occupied there was an evident further reason: keeping watch on the natives at Gosbecks close behind it. The plan for a base will thus have been dropped, and dropped soon after its inception on the emperor's departure. So when had he departed?

The army's sailing to Britain from Gaul, intended for spring 43, had been delayed (Dio, lx. 19, 2-3): mutiny of the troops had retarded it. But the Greek word *xpoviav* (chronian) for this means simply 'belated', not necessarily 'late in the season', as in the Loeb translation. Once landed (Dio, lx. 19, 5 to 21, 2: plainly in Kent), though at first the natives hid, two battles won and next a harder one (plainly on the Medway) took Plautius to the Thames; and then (as pre-arranged in case of such troubles as he met there) he sent for Claudius. The account in Dio goes on to say that much preparation had been made for the

emperor's response, the assemblages including even elephants to emulate Caesar in 54 BC (Stevens 1959; Hawkes 1978, 170-71, n 3). Suetonius' *Life of Claudius*, 17, 2 (written only 70-75 years after, when Tacitus' lost account could be newly available), says that his journey to embark at Boulogne, made at foot pace, started from Marseilles; Dio (lx. 21, 2-3) has it directly follow his sail there (by Ostia) from Rome, started when Plautius' message had reached him, but with time for his duly arranging for affairs in his absence. As it lasted six months, and he returned when the next year's consuls had on January 1st taken office (Dio, lx. 23, 1 with their names), he would have left in the very first days of July, reaching Plautius on the Thames in just under six weeks (if at some 15 miles per day across Gaul); yet this does bring him here already quite late in the season. The advance into Essex, however, which led to the capture of Camulodunum (after one battle according to Dio, lx. 21, 4, but without one says Suetonius (17, 2)) was rapid, for he was only in Britain at all for 16 days (Dio, lx. 23, 1). He can then have departed at the end of the third week in August. By then, the immediate dispositions for the troops guarding Colchester, those of our Roman Phase 1 (pp 55-61), will have been in course of carrying out — to last thereafter till early in Roman Phase 3, when the legionary fortress was ready enough for occupation (p 67), and doubtless the fort beside Gosbecks (p 64) still retained. But what may be seen as Plautius' base for an Icenian war would be put in hand directly the Emperor had gone. Then why so quickly cancelled? The answer must surely be this. The Iceni, made aware of his plan, decided not to fight. They sued for peace-terms. We know that they did, from Tacitus' retrospective record of their doing so, 'a powerful people, and one not bruised (as yet) by battles, because they had voluntarily entered into alliance with us' (ie become officially *socii* of Rome) (*Annales*, xii. 31, 3). Their doing so had doubtless been already recorded by Tacitus, in the same lost book into which he had put, as in xiii. 32, 3 he says he had (though not where), the award made to Plautius in Rome — after his return there from Britain at the end of summer 47 — of an *ovatio de Britannis*: full 'triumphs' were for emperors, but an 'ovation' was the next highest honour. The Icenian submission would have been among the events narrated there, for Plautius' years in Britain after Claudius had left it. But this still allows the submission to be put not very long after: within 43. Our putting it then, admittedly, rests on inference, from the traces of works abandoned when barely begun. Yet one of them (p 62), in both its parts, lay in the area of the legionary fortress that covered it over: and that this can itself have been started within 43, as it dates as a whole *c*43-*c* 49, is a result of Crummy's published excavations on critical sites that is past disputing. And this, after our Roman Phase 1 on the morrow of the conquest, and Phase 2 for the unfinished works, gives us next our Phase 3 for the time of the fortress.

We have said that until it was fit enough for allowing occupation, the Phase 1 dispositions for the troops will have been maintained. Yet nowhere in the known expanse behind Gryme's Dyke has there ever appeared any trace of what might have been expected: normal

Roman temporary earthwork camps, or even one such.<sup>26</sup> All that we have instead are adaptations of some of the dykes. Yet these, and their retention till Phase 3 had had its start, are less surprising once we remember the lateness of the season. When the Icenian submission can be claimed, it would be early autumn. Starting on the legionary fortress, all the better if October were fine, was anyhow vital for getting the men moved into it as soon as ever possible. Waiting for Claudius, though some of the army might be busy elsewhere meanwhile, had seriously delayed not only their final quick advance, but everything that followed in the months till the end of the year. And as we believe that that included starting on a war-base, only to have it stopped through an early-autumn Icenian submission, one may all the more see reason why the Colchester evidence, inferred as for 43 from August onwards, can stand for a special and perhaps unparalleled case.

### Roman Phase 3 (AD 43-49): legionary fortress and contemporary works

#### i) Legionary fortress

The remains of the fortress underlie the Roman and later town. It has been extensively treated in other CAR volumes (especially CAR 3, 1-9 & CAR 6, 7-13) and elsewhere (especially Crummy 1988, 24-41).

#### ii) The fort at Gosbecks

This was a discovery of aerial photography, which in June 1976, confirming traces previously photographed, made it certain that here was a fort; certain no less from the aerial photographs taken independently, in the same summer of 1976, by Mr J N Hampton of the National Buildings Record (Air Photographs Unit) and Dr D R Wilson of the Cambridge Committee for Aerial Photography (Wilson 1977, pi 13). Dr Wilson's description need not be repeated here, but in one respect does need emphasis; the hedgerow along the fort's west front, on the line of Heath Farm Dyke Middle (pp 30-32), is impinged on by the curves of its defences as they start to round the front's two rounded corners. (In Crummy's article in the same volume, his maps of the Gosbecks area (Crummy 1977, 88-9, figs 13-14) showed all that was known of it when he wrote, but had to have the newly-found fort schematically added.) Wilson was inclined to think that those curves simply abutted on the back of the dyke's rampart; but as Kidman's Dyke Middle had been built outside and along it (p 33), the rampart might have been levelled then already, and now been covered by the fort's having its own earth-and-timber one. Excavation alone can

decide. The fort was almost square, with sides of approximately 150 m, about or nearly 470 *pedes Monetales*, and in area therefore some 5.5 acres (2.2 ha). This would be rather ample for a *cohors milliaria* (of 800 auxiliary infantry), so more probably was for a mixed garrison of infantry and cavalry. The close-range purpose, plainly, was to control the native occupants of Gosbecks, with their tribal shrine (as Wilson 1977). For comparisons of size elsewhere, we have the first fort at Templeborough (South Yorkshire), probably already in the later 40s, again 5.5 acres (Collingwood 1930, 30-31), and the somewhat larger fort on Hod Hill (Dorset), of 6.86 acres (2.75 ha). The garrison there was mixed — auxiliary cavalry (just over half of an *a/a quingenaria*) and a cohort of legionary infantry (Richmond 1962, 79-84) — but the composition of such garrisons will naturally have varied, and where mixed might have all been of auxiliaries, horse and foot.

Although the fort has not been excavated when these words are being written, the aerial photography shows that it was completed and occupied. Thus while nothing prevents our regarding it as planned to be an element in a base quickly cancelled elsewhere, this fort will have been purposely retained as an outlier in conjunction with the legionary fortress, which replaced, from late in 43, whatever other plans were cancelled. The length of its occupation can at present only be guessed; Wilson gave 43, without doubt rightly, for its beginning, and it is likely to have lasted at least until the *colonia* was established in 49 (from a decision that will have been made in the foregoing winter). The small force that Tacitus (*Annates*, xiv. 32, 3) records as being in Colchester in 61 (unless in 60: p 70) is unlikely to have been as distant from it as this (it might have been in the *colonia* itself, as the word should mean); how, or whether, it had to do with this fort is thus uncertain. The site is fully 2.25 miles, or 3.6 km, SW of the *colonia* and previous fortress.

#### iii) Sheepen Period 3, with the unfinished aqueduct

Period 3 at Sheepen, coming directly after the Sheepen Dyke had been levelled (dated by it and described in *Camulodunum*, 73-5, fig 13; 76, pl 107), can be recognised now as corresponding to the period of the fortress. Of its features — Sheepen Road 1, occupations over the filled-in ditch of the dyke, and many others — the most interesting in this connection is Ditch E3, in the site's Region 2. Undoubtedly an aqueduct for water from Sheepen Springs, it yet cannot ever have reached a destination where the water would have been used. It cannot have been taken much farther to the east than its excavation showed, since it was absent from our Section 23 running west from the filled-in dyke-ditch, our Ditch 1, while the rising southward of that would have negated its necessary gradient: a fall of 1 in 80 (so measured over 80 yds (74 m), the length of it explored). It was therefore abandoned incomplete, to be at once re-filled with

26 Or practice camps unless the supposed fort at Altnacealgach and the triple dyke near it (p 127) fall into these categories.

rammed gravel. Pockets of refuse in this had no pottery later than the site's Period 3, while Period 4 tile-rubbish, widely strewn hereabouts (from a kiln in that Section 23), came only on the top of its filling — as again on that of the adjacent large hollow E1, likewise of Period 3. It will therefore have been planned to serve the fortress, and been abandoned when the *colonia*, in 49, was established instead.

### **Roman Phase 4 (AD 49-61): the first *colonia*, with the contemporary Sheepen Period 4**

For the *colonia* (with sites outside but very close to it) and its structural relation to the fortress, Mr Crummy's major works have already been cited (p 67). These, and their use at other points in the present report, make any summary here superfluous — as does his own (Crummy 1988). For Sheepen, the 1970 excavations by Rosalind Niblett (1985), where Sealey's account of the amphoras led to its expansion in his own (Sealey 1985) added notably to knowledge of the site's Region 4, given in *Camulodunum* from the excavations of 1930-39. That the whole Sheepen site's Period 4 corresponded to the dozen years' life of the first *colonia*, from early 49 to Boudica's revolt, was there shown first from the dating of its stratified material, and then from its general character (*Camulodunum*, 34-8). That the destruction in which it ended had to be the work of that revolt, dating the consequent Period 5 to AD 61 (*Camulodunum*, 38-43), can still hold good for the destruction itself, but has needed amendment in at least two other respects. Andrew Fitzpatrick (1986, from his lecture to the Limes-Kongress in 1983) contended first, quite justly, that the military metal-working then destroyed by fire, in Area A of Region 3, at and around the furnace-site A4 (*Camulodunum*, 39-40, 91-3, with 336-40 by Hull on the copious relics of soldiers' equipment), need not all have been in hasty preparation to fight the revolt, but will far more probably have been started earlier on, and only intensified on the approach of the tribesmen in revolt. There had been legionary soldiers here, along with the *colonia*'s veterans, though not so many at the time of the revolt. They had backed the veterans' brutal treatment of the natives, for some time before then — most likely ever since the *colonia* was founded (Tacitus, *Annates*, xiv. 31, 3; 32, 3). Of this, and Fitzpatrick's other points, he kindly let use be made, before his lecture's publication, in the brief review of the site in its Roman periods (2 to 5 and the post-Boudican 6) in Hawkes 1985, which is part of an article of wider scope by Professor Malcolm Todd (his 1985).

### **Early Roman roads**

[PC: This section was never completed. All of CFCH's Roads 1-8 are shown on Figures 2.32 as well as those without numbers. Road 1 is discussed further on page 64 (including footnote 22) as well as below.]

We have seen that the surveyors had in Roman Phase 1 sighted lines for Road 1 and then for Road 2. The laying of Road 1 could be started at once; that of Road 2 was started next, in Roman Phase 2, up to Prettygate Junction but never farther. Its sighting-line, however, prolonged from the line of Prettygate Dyke, would hit Road 1 behind the SE corner of the Grammar School (Point A on our plan Fig 2.32).

Road 3, from this same Point A, was laid NW aligned at W43°N (*Rom Col*, 8). Hall in his excavations came upon it at his point C/1 (on fig 1 in *Rom Col*, 5), where it was pierced by a later cutting of unknown date (near the Gurney Benham grounds' eastern limit: *Rom Col*, 5, fig 1). He followed it on from there, taking a section at C/2, and passing a second narrow later interruption till he had exposed it altogether for 200 ft (60 m). The section at C/2 (no IV in *Rom Col*, fig 2) showed its surface metalling to be a pebbly gravel concretion 22 ft 6 in wide (6.9 m) laid on a bed of washed loam, which at both its edges appeared to have been revetted, and on the south sloped down into a shallow and flat-bottomed side-ditch which Hull disregarded in his text. After the latter was written and his plan had been drawn, Hall found a deeper ditch on the north side (*Rom Col*, 273, n 3), seeming irregular, probably a spoil-ditch dug for gravel. This, near his point C/3, could have been the deepening of a side-ditch originally as shallow as was the southern one. Road 3 at first would then have had twin side-ditches, both of them shallow.

This would contrast with what Road 1 was found to have, where uncovered at 26 Lexden Road (Holbert 1966): a shallow northern side-ditch only. An exposure at Stanway showed a northern ditch deeper, and 12 ft wide (3.7 m) (*Rom Col*, 13, no 165). Of its ditch at St Clare Road nothing more is known (*ibid*, no 158, pi 93). And Road 2, the next earliest, laid in Roman Phase 2, had only a southern ditch, purposely deep (p 48). Roads proved to have had twin shallow side ditches are that at Iron Latch Lane (*Rom Col*, fig 2, Section II) and that on Rayner's Farm (Road 5), on the way to Gosbecks (*ibid*, Section I). The total length of the latter (not less than 2.25 miles — nearly 5.3 km) seems more than would have been finished within the very short Roman Phase 2, when preparing war with the Icenii had so little to do with Gosbecks, that being Road 5's primary destination. As for the twin-ditched road at Iron Latch Lane, which was pointing at the upper Colne, so on towards Cambridge, the time for this would again be Roman Phase 3, when the war had been averted. So if both these twin-ditched roads are no earlier than that, the same can seem reasonably likely for Road 3 also.

As recorded by Hall in *Roman Colchester* (p 8, note 2), his attempt to find traces of roads north of Lexden Road came to nothing; nor were any found by Hull on Sheepen Hill beyond the intervening valley, in the Area G explored by trenching in 1934 (part of the Sheepen Region 5; *Camulodunum*, pl 110). Yet he might just possibly have missed it, obscured by traces of later occupation, if at the brow of the hill, about the 110 ft (33.5 m) contour, it had turned to the NNE and passed

through the Sheepen Dyke's SW entrance, giving access thus to the whole of the area within. Hull was certainly right to doubt its being meant for a road to Cambridge, seeing that that should be implied by the road that he sectioned at Iron Latch Lane (with the twin side-ditches noticed above).

Road 7 had its beginning at Prettygate Junction, where its single ditch was found, in the 1957 excavations, showing it starting from Road 2 at the point where this was discontinued. The ditch passed through what had here been the Lexden Dyke entrance, between the end of the line along Bluebottle Grove and the starting-point of Lexden Dyke South, where it made an abrupt double curve to allow it to do so (best seen on Fig 2.25, p 49). Excavation had to be stopped at the end of the curve, but this was exactly on the line, E13°N, of the ditch found by Hall some 660 yds (603 m) away in the Grammar School playing-field (field 1266) and exposed for a further 66 yds (60 m) (no 179 on *Rom Col*, pl 43). That was before the excavations at Prettygate Junction, being part of an attempt to show that Road 2 ran farther; it was not, though it was almost, running on the Prettygate alignment of that, so whereas both Hall and I (who inspected it with him) saw it as a road-ditch, Hull thought it 'possibly that of a minor rampart' (*Rom Col*, 4). Not till the ditch through the Prettygate entrance was found, in 1957, could it appear that we had been right.

The field (1266: Hull's '1266a' was here a slip) is that in which Hall, and his then headmaster at the school, conducted the excavation of the temple of Silvanus (*Rom Col*, 236-40). Probably the woods here implied by this deity were standing when Road 7 was laid through them. The road was inclined at only 9° from the Prettygate line of Road 2, yet for how long the whole of Road 2 was left alone after being stopped there in Roman Phase 3, until a Road 7 could prolong it, can only be guessed. This has no known traces beyond the temple field, but if the line of it there was prolonged unchanged, it would touch Butt Road (past the north of the Roman cemetery: CARS) 30 yds (27 m) uphill from Essex Street (*Rom Col*, pi 44). And that is where it would be met by the line of Road 6, projected straight on east from the County Hospital. If thence an onward bend to about N40°W be supposed, it would have entered the *colonia*, crossing the Crouch Street ditch, by what after the Colchester Period 4 became the Head Gate (CAR 3, 11-16, figs 7 & 9). As no entrance here existed before that period, which is reckoned to have been about AD 80, these guesses would offer that date for both the roads; but the *colonia* is still very far from their known remains, and nothing more of them now can be usefully said.

As for the road that ran just north of the Lexden Tumulus, exposed across Fitzwalter Road from near the south end of St Clare Road, on a course E2°N, which Philip Laver sketched for 200 ft (60 m) farther (*Rom Col*, 13, no 166 on pi 43), we may call it Road 8, and remark that this course would have hit Road 1 just beside the Longinus tombstone (*ibid*, no 24), just west of Beverley Road. Since it would have crossed, as Hull

pointed out, the area west of Altnacealgach House where he located the rhomboid fort (his no 17), assigned here above (p 62) to our Roman Phase 2, it would anyhow be later than that. But in the other direction Hull was in error when he let it pass through a gap in Lexden Dyke: he forgot that in the Lexden Park excavations of 1932, this was found to be no true gap, but only a dip in the dyke across a natural gully (pp 41-2). So it seems that at present this road must be left unexplained.

## Boudica's revolt and the ensuing second *colonia*

(at Sheepen, Period 5 and Period 6)

The destruction brought to the first *colonia* on its capture by the rebels, and to Sheepen at the end of its Period 4, has been briefly noticed above (p 68), and will be treated further below (p 70). At Sheepen, what calls already for mention here is the site's re-fortification by the ditch enclosing more than half of it, Ditch 2 in *Camulodunum*, which in Region 3 cuts through the destruction-debris, and throughout its southern stretch was found to be backed by the trench for a timber palisade, with the emplacements of a four-poster gateway through it, and interruption of the ditch in front, in Area Z of Region 5. All these, of Period 5, were in *Camulodunum* (pp 55-6, fig 4) taken for the work of the victorious rebels (so too in the accounts of each of the region's excavations). But Professor Malcolm Todd (1985) showed the gateway's plan to be so typical of Roman forces that the whole re-fortification must be assigned to the Roman forces, which on the rebels' defeat will have quickly re-occupied the *Camulodunum* territory. This includes, in Area A of Region 3, the ditch for a *clavicula* running out from Ditch 2, though as this is here continuous the entrance implied would have crossed it by a temporary bridge. (See *Camulodunum*, 92, 94-5, fig 20; and for the Region 5 gateway, pp 117, 120, figs 32 & 34, and pl 110.) But nothing upsets the *Camulodunum* account of the various features of Period 6, which soon ensued — including the dwelling-floor laid on the re-filled Ditch 2, close to the gravel-pit north of Region 3 (*Camulodunum*, 95, fig 21): of oval native type, so assignable to non-rebel 'trusties'. So the end of this period, completing the sequence of the six, still stands near to AD 65.

(For the second *colonia* that followed the destruction of the first, see Crummy 1977 & 1988, CAR 3, and CAR 6. For the date of AD 61 adopted here for the Boudican revolt, see Carroll 1979, as against Sir Ronald Syme's AD 60 (Syme 1958), adopted by others including Webster. See page 70 below.)

[PC: The chapter was meant to end with a section headed 'The completion of Gryme's Dyke Middle' but this does not seem to have been written.]

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### 3: Sheepen: dyke and pre-Roman occupation

by CFC Hawkes

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#### Sheepen Dyke: its purpose and plan

[Fig 3.1]

The ditch of this dyke had its course well established, in the 1930-39 excavations, by clearances, total or partial, and by thirty full sections at intervals along it (*Camulodunum*, 16-17, 23-7). Along its main front it was seldom less than 30 ft (9.1 m) wide, in places more, and was 8-10 ft (2.4-3.0 m) deep. With a rampart proportionately big — found destroyed except in two stretches where its base remained — the dyke was plainly a military work of defence, whereas Heath Farm Dyke's purpose had been protective and scarcely more.

The main front faced NW or WNW, but was completed at its south-west end by a stretch that bends south-east (where the ditch was less regular and somewhat smaller), which ended close to the stream in the valley here bounding Sheepen Hill. Thus the plan as a whole was L-shaped, with the angle being 100°.

As the hill is a broad peninsular bluff jutting east-north-east from the plateau, till it falls on its northern sides towards the Colne, the longer of the L's two arms runs up from the riverside and over its top, from which the shorter slants down to the stream; so defence was continuous against all westerly quarters. At a time that was proved to have been later, the longer arm was lengthened further south-westwards, to defend the area in front of the shorter arm. In *Camulodunum*, figure 2 (facing p 46) gives the whole pre-Roman layout, and here is reproduced as Figure 3.1. Behind, stretching nearly half a mile from north to south (800 m at least) was the occupied expanse which was open to the Colne and on the east throughout, and had access through the dyke at three entrances: a north-west, a western main one, and a south-western at the angle of the 'L'. The occupation, as *Camulodunum* presented it in 1947, would have been started when the dyke was built. But when was that, and had there been anywhere a partial occupation, independent of the dyke so perhaps before it?

#### Stratigraphy and chronology

The general pre-conquest occupation is naturally of Sheepen Period 1. Within it, the dyke was constructed and began to accumulate primary

ditch-silt, which finally rose to a considerable average thickness. Over it, forming a layer again lying thick, was the material thrown down from the destruction of the rampart of Sheepen Period 2. The finds in this material will be noticed here below; and it had filled the ditch, along most of its stretches, up to, or up towards, the height of its lips. Over it, and seldom absent, were two successive levels of occupation-earth (p 79). One was of early post-conquest period, ie Sheepen Period 3, while the other was of Period 4, when the site suffered sudden ruin, of a thoroughness which (in accord with associated finds) assigned it beyond all doubt to Boudica's revolt. And that was placed by Tacitus — his famous chapters give us our earliest account of it — in a year that he guarantees to be AD 61 by stating the names of its consuls in Rome (*Annates*, xiv. 29, 1). The sole authoritative manuscript (xiv. 37, 5) supports his spelling the queen's name as 'Boudicca' just once, the miss spellings elsewhere being all corrupt. (For 'Boudica' as better, see Jackson 1979, 255.)

The date 61 for her revolt, long accepted universally (as in *Camulodunum*), was questioned indeed by Sir Ronald Syme (1958, 705-6) in favour of 60, as loading 61 with too many events. He was followed unquestioningly in this by Frere (1987 edn, 70-71, with 78, note 37), by Miss B R K Dunnett (1975, 48) and by Webster (1978, 1980, 1981), while Salway (1981, 116-17) has at least renewed Sir Ronald's questions. But the fresh review by K K Carroll (1979) shows 61 still possible, even if not with final certainty, and credible all the more because explicit in Tacitus — whose father-in-law Agricola, as a young military tribune, had served in Britain throughout the time of the revolt (*Agricola*, 5, 1-4). So we are keeping 61 here for the outbreak, which at once brought rebels to Colchester to sack the *colonia* (*Annates*, xiv. 31-2), and equally then for the destruction at Sheepen that ended its Period 4 (*Camulodunum*, 56, with especially 91-3, 335-6), though Fitzpatrick (1986, 37-8) has rightly observed that the military metal-working wrecked in it (found in the fill of Site A4 and the adjacent pits: *Camulodunum*, 92, fig 20, close to the entrance in Region 3; and 336-40 with figs 62-3, pis 101.2-104) should have been active from early in Period 4, not simply (as Hull and I believed) right at its end. Periods 5 and 6, directly ensuing, show the aftermath. Thus the Roman sequence at Sheepen covered barely 20 years (or little more), if the dyke's destruction is agreed to have been ordered at the conquest in 43. One is bound to expect it anyhow among the very first steps to be taken, for rendering the British capital area defenceless.



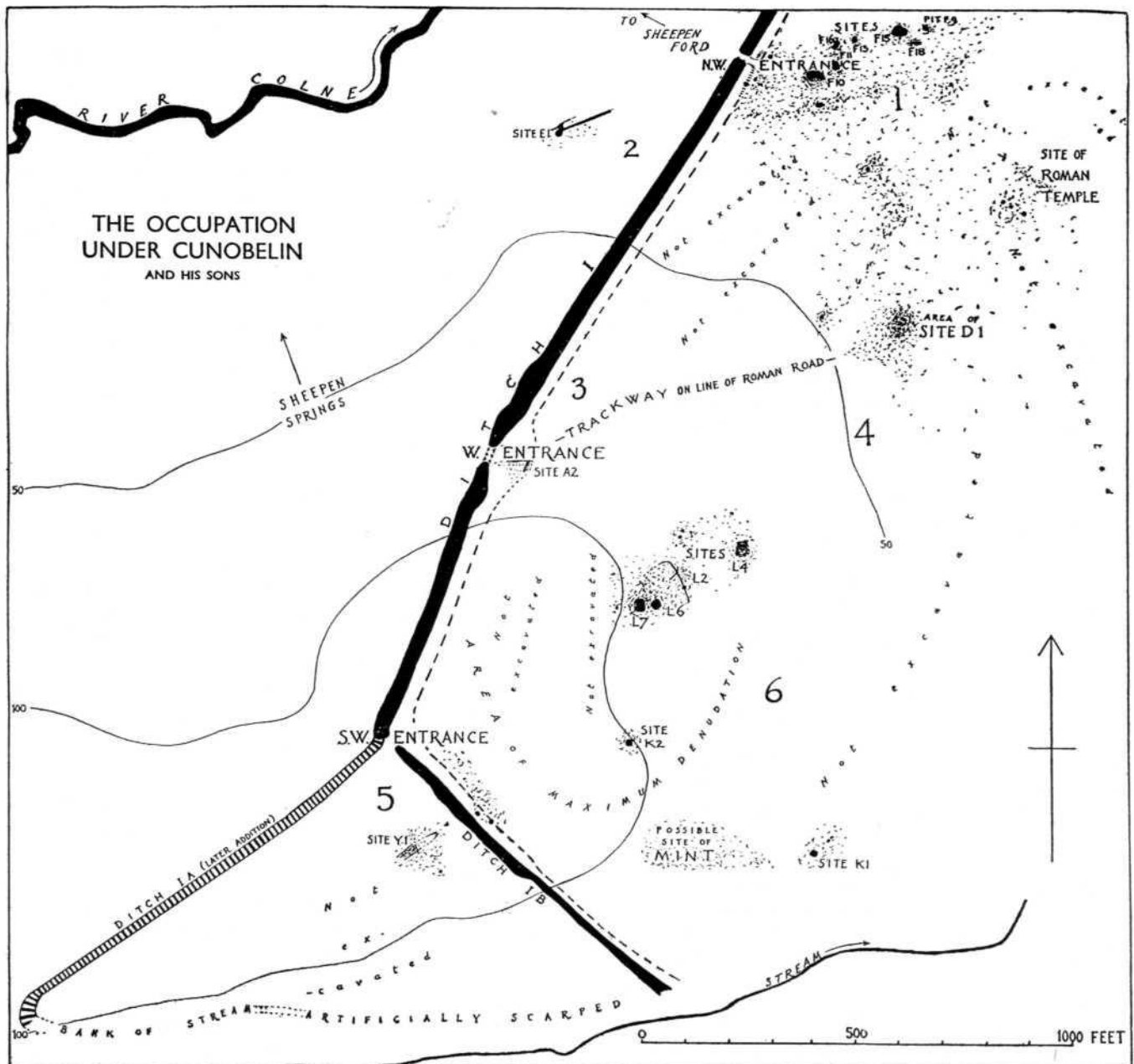


Fig 3.1 Plan of the Sheepen site in Period 1. Reproduced from *Camulodunum* (fig 5) by permission of the Society of Antiquaries of London. [Page 70]

While the dyke still stood, there fell into its primary ditch-silt, to be amply represented by the finds from it, much material including purely native pottery, but also native pottery somewhat Romanised in fabric, and pieces of various kinds explicitly Roman and therefore imported. So when had the dyke been built? This could not be decided from the purely native pottery: taken by itself, here or anywhere on the site, it might or might not have preceded initially the somewhat Romanised fabric. Yet clearly this itself would reflect the Roman imports' influence; the date had thus to be inferred from those imports themselves.

### Old and new comparative dating

For dating them, the Continent's best sites were in western Germany, especially forts or fortresses there established under Augustus, behind and along the Rhine — but also beyond it to the east, where the native Germans, after a while, succeeded in forcing the garrisons back to the river. Of the forts thus abandoned, the one most fully explored was still, at the time of the Sheepen excavations, that at Haltern, on the river Lippe; the main report on it was printed in 1909 (Koepf *et al* 1909). In that, its abandonment was put at AD 16,

after the three campaigns led beyond the Rhine by Germanicus Caesar (step-grandson of Augustus, who had died in 14). That date of 16, taken in *Camulodunum* to be firmly fixed, let us date our earliest imports, pottery and brooches, to the years preceding it. Not certainly before about 10, which we believed to be well within the period of Haltern's occupation, notwithstanding that in 9 the Germans had slaughtered, in the Teutoburg Forest, the Roman governor Varus and all his legions. For it seemed a possible consequence of this that their supplies, more especially of pottery, would in part have been deflected into trade with Britain. The Germanicus campaigns were to avenge the disaster of Varus, and only at their end would the garrison of Haltern have been withdrawn. Such was our main case for giving the Sheepen occupation a start about 10. Its coinciding with the date which numismatic opinion could offer, for the first issuing of coins from here by Cunobelin, made an attractive secondary case for our belief that it was he who established Sheepen as *Camulodunum*. Our presentation of the stratigraphy and chronology from that beginning until Boudica in 61, (detailed in *Camulodunum* on pps 27-41) was amplified in our 'Synopsis and conclusions' on pages 44-56. But however much else still stands of all this, 16 for Haltern's end was quite certainly wrong.

This was quickly perceived by Hull, after studying the newer work done in Germany, east of the Lower Rhine, on all the known Augustan military sites. More of them, both there and in the south, have been found and explored; and from Switzerland to the Netherlands, mostly along the Rhine itself, five at least of the major sites already known have had further attention. And for Haltern, where the newer excavations were conducted in the 1960 and 1970, the consequence has been to change the date of its abandonment, on all the evidence old and new, to a year that can only have been AD 9, when the Varus disaster itself will have brought it about. As for the date of the principal Haltern fort's foundation, the evidences converge on its being most probably 7 BC. The place for it could have been chosen already in 8, after the Germany command had passed, on the sudden death in the previous September of Augustus' stepson, Drusus, to his younger brother Tiberius, the future emperor, who in consequence evidently altered some of his army's dispositions. (One change was to abandon the fortress at Dangstetten. Since the Roman frontiers in Germany were all reviewed in English by Professor H Schönberger in his 1969, and since 1972 when Professor Colin Wells published his study of the German policy pursued by Augustus, fresh studies in the early Roman history and archaeology on the Lower Rhine and Lippe have been numerous.

On individual military sites, they have included that of Roedgen where Schönberger and H G Simon (1976) showed a rather short occupation probably begun in 12 BC, and that of Neuss in its earliest phase (corresponding to the earliest at Xanten, *Vetera*), by Elisabeth Ettlinger (1983), based on its Italian-Arretine terra *sigillata*, indicated a start-date which is no later than 15 BC and possibly even as early as 20. She

proceeds to estimates of date for some other sites too; up the Lippe, beyond the Rhine, the chief two are Oberaden (primary reports were Albrecht 1938 and 1942, with Loeschcke on the pottery), now to be dated from 11-8 BC, and Haltern, this from at earliest 7 BC to its abandonment in AD 9, as stated here above. Its modern chronology is embodied succinctly in S von Schnurbein's 1981, which covers all known Roman forts in the Lippe/Rhine region. This ensued on the review of the whole early Lower German frontier by Dr M Gechter (1979), and for Haltern was supported by Professor von Schnurbein from its plain *sigillata* in his publication of 1982 which was subsequently reviewed by Wells (1985). Within the 94 pages of von Schnurbein 1981, Haltern is treated on pages 33-44 (the dating again at p 94) and 44-78 with 93-96; on pages 87-92 von Schnurbein considers the altering of military dispositions throughout this frontier, when Tiberius was given the German command, after Drusus in 9 BC, as mentioned above, had unexpectedly died. (For the special case of the dating-value of pottery afforded by amphoras, see pp 73-4.)

The result of all this for Sheepen is that one now has a wider perspective for considering dates for its occupation's start. The Haltern main occupation-span, 7 BC to AD 9, whether all or only partly valid for matching Sheepen's earliest *sigillata*, is certainly valid for matching its earliest other imported tableware, the Gallo-Belgic, for there was some of this already at Oberaden, 11 to 8 BC (Loeschcke 1942). Moreover, in the land of the Treveri, behind the Rhine, its platters have occurred among the grave-goods of a martial nobility, in Luxembourg in tombs between Goeblingen and Nospelt (Thill 1966, 1967a, 1967b; cf Werner 1978), certainly before 15 BC, though the dates cannot cover more than 10 years.

Platters show adoption of eating at table, in the Roman manner (von Petrikovits 1970; Wightman 1985, 48, 142, 341 note 75, 351 note 25); likeliest thus by such an elite class first, by commoners gradually after, and presumably then about 5 BC by Britons — or at least hardly later, for the flat blackish platters in micaceous ware (Cam Form 1) now are recognised to be primarily not from Belgic but from Central Gaul, and to have come into production there earlier. That matter will be best discussed below (p 80): the matter now in question is an absolute initial date for Sheepen. Hull, through his later years to 1976, had used the evidence from brooches for his corpus of every ancient brooch in Britain (now in process of posthumous editing, its pre-Roman bow brooches volume being Hull & Hawkes 1987), yet of course, in applying it to Sheepen, he had had most regard for the pottery. On this he left no written comprehensive statement. But the initial date for the site at which I know him to have finally arrived was close to 5 BC, or at the most not later by more than two or three years. We ourselves, as will soon appear below, shall only accept that dating for a part of the site, and (strictly speaking) as allowable rather than compulsive. Mrs Niblett's conclusion, from her own and her contributors' work on the finds from her 1970 excavation, mostly in parts of the site's Region 4 till then

untouched, is that the date should be suggested to be slightly later, c AD 5 (Niblett 1985, 3). So too (at least provisionally) agrees Sealey, on the 1970 and other finds of amphoras (noticed below; 1985, 108 (leaving aside pp 13 and 104 on Cunobelin)). This date of course still just precedes the abandonment of Haltern.

## Evidence from imports of amphoras

[Figs 3.2-3.3]

Meanwhile, such re-dating had been proposed from a different quarter. Amongst the imported pottery are pieces of amphoras, of types that have complete examples elsewhere, found in graves. These types in Britain, with contemporary Gaul, had their first full study in English, published in 1971, by Dr David Peacock. The Italian type imported holding wine, Dressel 1, in the later of its two main versions, Dressel 1B, appears in the middle 1st century BC<sup>1</sup>, and was most in use within the century's later half. In distribution it follows the prior 1A in Gaul, most widely in the north and about the Rhine, and in Britain on the Channel but decisively north of the Thames, in Hertfordshire and around it, and also in eastern Essex including, of course, the Colchester region (Peacock 1971, 173-9, 182-5, fig 36; Peacock 1984, 39). By 1982, Patrick Galliou's map (1984, fig 10) could be fuller; Fitzpatrick's (1985, fig 3) now is fuller still, more especially for north-east Gaul. Intrinsic dating of amphoras is sometimes given by inscriptions in paint on them, *tituli picti*; and the date of the latest one known on a Dressel 1B is 13 BC.

So this is what we have to compare with the evidence from Sheepen, where the form is Hull's Cam 181 (*Camulodunum*, 251, pi 69). His find-statistic for it was a count of fragments (either single, or two or more joining and found together): uniformity of fabric made a count of whole vessels, on the strength of such fragments alone, unacceptably risky. Of the 46 recorded, 25 were found unstratified; the pre-conquest Period 1 had certainly 5, possibly 6, but the remaining 15 were from later contexts, Periods 3 and 4 having at least 12 (*Camulodunum*, 280). The end of Period 1 was unavoidably AD 43, and the rest had to cover not less than the 20 years after. Thus the incidence of these fragments, of a type of the century before, needed explaining — as was first pointed out by Peacock (1971, 178-9). The 1970 excavations added 5 more of Dressel 1B, and 2 of the equally Augustan Dressel 20 (Sealey 1985, 90-112).

Already in the years BC around 13, the date of the last known *titulus*, other amphora-types were competing with Dressel 1B, so that its production might be inferred, by then, to be waning. Yet its 46 pieces in Hull's count for Sheepen made fully 10 per cent of the total of the

others (Cam Forms 182-186), which is most surprising if this occupation had really begun about AD 10. Peacock accordingly moved for beginning it earlier, anticipating the case for this from the new German terminal date for Haltern. And he let me remind him of the native-ware sherds, found 1930 in Region 1, in and mostly under the base of the Sheepen Dyke rampart, in the stretch where its clean laid loam, escaping the Roman destruction of the rest, was best preserved (see my Area F plan, *Camulodunum*, fig 5, and Sections 3, 7 and 8 (figs 7-8), all here reproduced as Figs 3.2-3.3). We had thought of them as all dropped by the rampart's builders, yet they might have been left by a prior occupation hereabouts, and this is what Peacock surmised. It might then be at a time before Cunobelin's coming to power, when the Trinovantes here were still under different rule. And with Hull's earlier dating for the main occupation's start, or just with Mrs Niblett's and Sealey's, only ten years after it, Peacock's local prior occupation would be earlier than either — and would all the more nearly suit amphoras of Dressel 1B. But the fragments of those lay in contexts later than their own proper date; and wherever this is seen to be the case with any amphora site-finds, one can explain its occurrences in three more general ways, none exclusive, but perhaps acceptable side by side.

## Amphora site-finds later than date of manufacture

First, it is obvious that such fragments may survive as broken rubbish, till any time after the breaking of the parent vessel. This of course is true for any, though vessels as large as an amphora leave more fragments than are likely from smaller ones. Secondly, and true in particular for amphoras, once emptied of their wine or any other original contents, their bodies — with neck or without — can be used as containers for something else: when perishable, all the better if sunk in the ground for keeping it cool (from excavations, at any rate abroad, there have been some examples). Such reuse, with the amphora not broken up until afterwards, will lengthen the time between its first being emptied and its ultimate breaking into fragments. Thus in the rich grave at Mount Bures, north-west from our site some 6.5 miles (10.5 km), meticulously explored when found in 1849, and published with measured illustrations by Charles Roach Smith (1852), one of the six amphoras laid in it, a Dressel 1B, had only its body; the handles and neck had been broken away at some unknown previous time. (One other, found crushed to pieces, cannot be identified.)

The third explanation is applicable only to amphoras for wine, such as Dressel 1. Some were kept sealed, by the producer or by dealers, for their wine to mature for

1 Now generally thought to be substantially earlier (C Haselgrove).



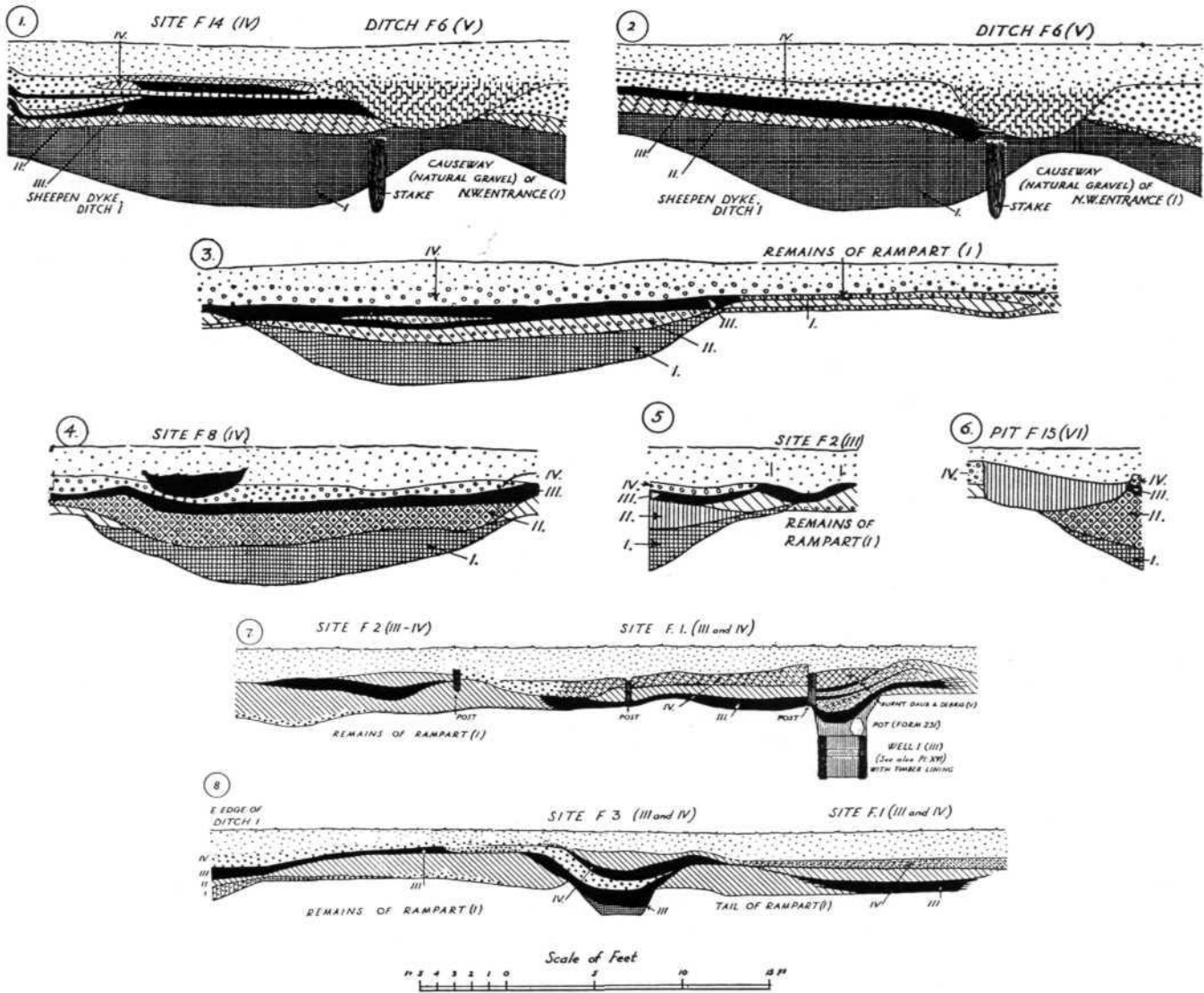


Fig 3.3 Sections 1-8 in Region 1 of the 1930-39 excavations at Sheepen. Reproduced from *Camulodunum* (figs 6-8) by permission of the Society of Antiquaries of London. [Page 73]

Dressel 1B, ie some time within the fifteen years that concluded the century. Moreover there began, in its last dozen years or so, a northward exporting from Spain, of various products in amphoras of several types, more known now than to Peacock (1971, 168-71) when preparing his 1971 book. Just then (dated 1970) appeared their treatment by M Beltran (carried further in his 1977); R Pascual summarised his own Catalan series, with his type-numbers, in 1984. These have been aided, like modern amphora studies in general, by mineralogical analyses of fabrics, eg David Williams 1981 (for pieces found in Britain), and as used by Sealey 1985.

As for the Spanish amphoras' contents, though some held wine, while the globular type (Dressel 20, Cam 187) held olive oil, the most distinctive were salted fish, or the salty relishes or sauces made from it: *garum* more especially (other names are *liquamen* and *nutria*);

Ponsich & Tarradell 1965; Zevi 1966, 231). Peacock (1971, 168-9, 171, with fig 35, 5-8) gives these amphoras' types most commonly found in Britain: Dressel 7-11, Cam 185B and 186A-C (Cam 185A was perhaps for olive oil). Spanish probably also are two of the fabrics used for the 'Koan' type (Dressel 2-4), as Sealey has pointed out (from petrology by Williams) for these at Sheepen. But Sealey confirms to me what Peacock declared (1971,183): there is nothing Spanish among the Lexden Tumulus amphoras, so that a date for the tomb might very well be prior to the whole of our Spanish importations. Peacock's consequent suggestion (1971, 178-9), and equally Ann Birchall's (1965, 254, proposed from the metalwork), that its date should be in the later 1st century BC, is strongly supported from the contrast manifest at Sheepen, where Sealey's statistics (1985, 18) show Spain's contributions to the total amphora count to be 45.2 per cent, and to the total reckoning of contents by volume

to be as much as 63.5 per cent. And in the region's second richest grave with amphoras, Mount Bures (p 73 above), where four were Spanish, the only certain other one in the grave's total of six was the Dressel 1B reused without handles and neck. That its date was within the Sheepen Period 1 is shown by its many Gallo-Belgic platters and cups. Of these the tumulus had none. Among the very few pieces of pottery certainly present among the tumulus grave-goods, there appears the micaceous fabric noticed above (p 72) as primarily Central Gaulish. Otherwise those pieces are native, all but one being copies, most likely indirect, of butt-beaker forms, from the Continent. But all the fabrics and forms present recur in quantities at Sheepen, so that a pre-Sheepen date for the tumulus depends not on these, but on the strong negative evidence given by its lack of any Gallo-Belgic tableware, and of any Spanish amphoras. And now, on pages 88-94 below, any placing of the tumulus in history (and indeed all aspects of it) must, since 1986, stand in dependence on the account of its archaeology, new and thorough, by Dr Jennifer Foster (1986). Her conclusions, while put in terms of high probability rather than of certainty, can at least allow of its being judged prior to the start of the Sheepen general occupation. Belief in this, however, demands that the amphoras Dressel 1B — themselves all made before approximately 10 BC — shall have been buried in the tumulus scarcely later than that, after previous importing from Italy sealed full of wine, whereas at Sheepen, if Sealey be believed, they were imported later, emptied of that, as containers for different products. For if their context at Sheepen were a prior occupation, or if the general occupation's Period 1 began as early as the tumulus, the other imports which could then be expected at Sheepen should be equally early, ie definitely earlier than 5 BC or the years just after (according to Hull), or at the latest earlier than Sealey's tentative AD 5 for the starting of the general occupation. Since the sherds in and under the base of the Sheepen Dyke rampart (p 73) were native and not allowing of fine chronology, Period 1 could not — still in 1985 — be subdivided.

As for Sheepen's pre-Cunobelin British coins, those pointing to Addedomaros were three, one in silver and the other two in bronze (or a copper-alloy anyhow), like Dubnovellaunus' three, the three of Tasciovanus/Verulamium, and the one other of his time (Allen in *Camulodunum*, 135-6). Introducing them (*Camulodunum*, 133-5) I judged them, being relatively few, to be survivals; and I would hold to this still —with, whatever degree of updating, now, for the start of Sheepen's general occupation. The case for a subdivision on different grounds, recently proposed, and the accompanying case for re-interpreting Sheepen Dyke, will be dealt with below; so I come back now to the Sheepen Dressel 1B amphoras. That these, represented by their numerous fragments, were imported already reused for secondary contents, nearly all after the Claudian conquest so upwards of fifty years old, may indeed be not impossible; but it isolates those in the tumulus, as apparently alone in having come with

their wine still intact, no less than that in the associated 'Graeco-Roman' amphoras. And the personage entombed can have drunk such wines for some number of years before he died; if any were 'vintage', their amphoras of course would have been still older. Lowering the tumulus' date still seems hardly likely, from its lack of any Spanish amphoras, and Gallo-Belgic tablewares to match their presence at Sheepen. So can anything else be suggested for Sheepen, that could offer a dating in the period most likely to be that of the tumulus?

### **The Sheepen pieces of amphoras Dressel 1B**

Sheepen borders the highest reach of the Colne for a sea-going ship; and nobody doubts that such ships brought the imports attested in its general occupation. The tumulus' amphoras must have been landed somewhere and, likeliest then, it would seem, on this reach of the river. A regular settlement receiving such amphoras, but nothing else from abroad to appear among its finds, could only be called an absurdity, as by Sealey, though supposing its occupation to have been full-scale. If conceived as just a landing-place for ship-borne goods — not, of course, throughout the year, but on suitable occasions in spring or summer — its other finds need only be pieces of native ware. For of the ship-borne goods, in so far as possibly traceable on the site, nothing durable need have been broken there except a proportion of the amphoras, broken up, or broken at the top, for decanting their wine into smaller containers more suitable for retail sales. Those kept whole would go direct to the wealthy consumer, such as the personage entombed with amphoras and other durable possessions in the tumulus. Wherever he had lived, it will have been somewhere that was away from Sheepen and had its own supply of water close at hand. Presumably it could have been at Gosbecks, in the inner quadrilateral enclosure called by Crummy the 'farmstead' (pp 97-8). But wherever it was, it would hardly have been too far from the Sheepen water-side for portages of wine-filled amphoras to reach it after landing there — any more than was the tumulus itself. This guess at explaining some, if not necessarily all, of the Dressel 1B pieces at Sheepen, as from amphoras landed there, during the years represented by the tumulus, not to be carried away intact but broken for decanting their contents, was passed by me to Fitzpatrick for his possible use. He embodies it briefly in his own consideration of the matter in his 1985, towards the end of its long note 4. He keeps in this my suggestion for the Camulodunum Site E1, in Region 2, which is even nearer to the river than most of Region 1. Site E1 is a site of habitation, with attendant ditches, pre-conquest but lying outside the Sheepen Dyke and west from it by about 40 yards (37 m; *Camulodunum*, 74-5, Section 25, pi 107). With native ware and

ironwork, and a little Gallo-Belgic, it had part of an Italian Arretine cup of Cam Form 12 (the uncommon Loeschcke Form 9) bearing the stamp of the potter C Vibius. This on Italian *sigillata* is known from the early Roman frontier only twice: at Vindonissa in Switzerland and Vechten in the Netherlands (Hull in *Camulodunum*, 192, 194, & pi 41; for Vechten, cf van Giffen 1952, 32, 35, & 38 for occupation as early as Haltern, which Vindonissa (Wells 1972, 50-51) likewise had). Thus here at Sheepen it can suggest an already early date for Site E1, since under Tiberius (AD 14 onwards) this Vibius moved to South Gaul (*Camulodunum*, 194, from Oswald 1931). To conjecture this site as inhabited earlier still, by people who then could have joined in meeting the occasional visits of ships — beginning before its apparent isolation by the dyke — would suit the landing of amphoras first within the proper time of Dressel 1B, when in any case the tumulus has pieces both of this and of 'Graeco-Roman' amphoras. The native sherds in and under the rampart of the dyke, in a stretch of it in Region 1 no farther from the river, could perhaps have been dropped then too. But none of all this would touch any more than a part of what was occupied later. The shelf of rising ground above Region 2 might have offered more evidence, had it not been masked by the buildings of Sheepen Farm.

### Recent questioning of the Sheepen internal chronology

This has been advanced by Dr Colin Haselgrove in his study published in 1987 for south-east England on the archaeological context of Iron Age coins there, which he partly broadens, for Sheepen, beyond this subject's immediate limits (Haselgrove 1987a, 163-71; pp 362-79 lists the coins from the Sheepen excavations of 1930-39 and 1970; his Hoard 10, pp 273-4, is that from Pit D9, dug in 1930)<sup>2</sup>. His argument starts from the distributions, both of British and pre-Flavian Roman coins on the site as a whole, and shows how they differ between its six numbered regions, its occupation being wholly in the last of the periods that constitute his 'Late pre-Roman Iron Age' (Cunliffe's term, abbreviated LPRIA). This is Period iii in his ceramic-archaeological chronology (Haselgrove 1987a, 60-63, table 4.1), but Period III in his coin-dating system (pp 94-6, table 5.6), which he divides into phases. Those in Period III are Phase 7, dated 20 BC to c AD 10, and Phase 8, thence onward until Cunobelin's death, shortly after AD 40, which put an end to his sequence of coin-types — the earlier being Haselgrove's E81, E82.1 and E83.1, the later ones E82.2 and E.83.2. Novel as are both these phases, and his short-hand notation for coin-types (the E means eastern England, in which the numbers

put the types into sequence), his treatment of the Sheepen site's chronology calls for mastery of them both.

Moreover he brackets together, as 'northern', Region 1 (its Area A now covered by Cowdray Avenue, Area H all the rest of it north of Sheepen Road), Region 2 (Area E), and Region 4 (its Area D, thus called '4D'). 'Southern', contrasting with those, are the hill-slope Regions 3 and '4L (one perhaps may add '6L); and lastly Region 5, on the hilltop and farther slope downward, with Region 6 beside it (thus really '6K'), are called 'the extreme south'. It is seemingly by a slip, at the foot of page 167, that Haselgrove says that Region 5 has 'virtually no pre-conquest occupation', for he points out on page 170 that here, 'on Sheepen Hill itself', can have been overlooked by the authors of *Camulodunum* the earliest occupation — to which he adds sites L2 and L6-7, which are away on the hill's east slope. He surmises that its scale would not be large; and this, in the concluding next paragraph, is just what he finds to be suggested by the coins and early brooches, together with the absence of imported *sigillata*. He also allows that in Region 6K, directly east of Region 5, the metal-working debris, in Pit K4 including slab moulds — accepted as for casting flans for British coins (*Camulodunum*, 129-33), with the approval of Derek Allen and the help of analyses (British Museum Laboratory) — was buried at the conquest from a British mint on the site, presumably therefore of Cunobelin. And he adds (p 167) that the high proportion from Region 6K of Roman silver coins, republican and other pre-Claudian, could be explained by their having been imported for melting down to give the mint more silver.

As for the imports of Roman pottery at Sheepen, pre-conquest in dates of manufacture, and any in bronze such as brooches, he finds them not at their height until the time of Tiberius, any late Augustan being fewer for example than at Braughing-Skeleton Green (Partridge 1981): consistently thus with being after c AD 10. In the northern regions he finds the imports to be probably mostly of the time of the regions' predominant coins, which are of early Phase 8 (one may guess it as before about AD 25/30), whereas in the uphill 'southern' Region 3 and 4L, the predominant coins are of later Phase 8, and the imports may be mostly no earlier. Thus he reaches the notion of a shift of occupation-intensity, uphill, at a suitably late Tiberian time. His proposals for differentiation, as here and for the hilltop regions, within what before him had appeared to be indivisible, its Period 1, are the first of their kind to be argued from analysis of evidence; they partly gain support from the 1970 excavations, by Rosalind Niblett (1985) in a part of *Camulodunum* Area L in Region 4, and on the hilltop are consistent with the findings that he could not know of, made in another 1970 excavation, and further in the following year, in what now is the Kiln Road area (pp 131-7).

<sup>2</sup> Also Haselgrove 1987b where the author considers the distribution of Sheepen coins by region and stratification and with reference to sites elsewhere in Colchester.



His presentation does include some inconsistencies: that noticed above for Region 5 may be only a slip, but there are others. He says (foot of p 168) that the insignificant losses of coins in the earliest occupation 'conflicts with all the other major sites with early imports'. Presumably this refers to Braughing (his pp 171-5), St Albans-Verulamium (his pages 175-8) and any intended amongst those noticed (his pp 178-80), all being north of the Thames. Silchester is not mentioned (but see his page 66, table 4.3). But this is inconsistent with Sheepen being probably not large-scale (his p 170), so that it would not yet be a major site at all. Region 2, furthermore, where 'coins are virtually absent' (his p 164), is never considered again. This is the Area E (misprinted F in *Camulodunum*, 170), already noticed, where the 1931 excavation was all outside the dyke and west of it, downhill from the buildings of Sheepen Farm which overlooked it directly from the south. Yet we have seen that the finds from Area E begin early enough to have a bearing on the question in the adjacent Region 1, of the amphora fragments of Dressel Form 1B.

## Interpretation of the dyke

[Figs 3.2-3.4]

Haselgrove moreover, among the contentions in his same 1987, has salient points that one has to call particularly dubious, in part at least for what may here be considered first: misreadings of evidence of the 1930 excavation given in *Camulodunum*, in its account of Region 1. These concern the stretch of the Sheepen Dyke in Area F that now covered over by Cowdray Avenue, the Colchester inner bypass. Its ditch (*Camulodunum* Ditch 1) was cleared right out — under J N L Myres, its primary discoverer, and then under me — with just two narrow intermissions to allow of drawing sections (*Camulodunum*, 58), from lip to lip for a 72 foot (22 m) length, extended on its inner side to 100 ft (30.5 m). Beside it, along this side, we found a spread of clean loam, 25 to 30 ft (7.6-9.1 m) wide from its toe-line fronting the ditch to its tailing-off behind. The plan Figure 3.2 here reproduces *Camulodunum*, figure 5 (facing p 57). This site plan also shows the lines of the sections taken through it and briefly described on page 60 in *Camulodunum*. Section 3 on figure 7 (here Fig 3.3) cut straight across the loam and showed it to be 6-12 in (15-30 cm) thick, but Sections 7 and 8 (in fig 8 and here Fig 3.3), cutting slantwise farther south, revealed it to be up to 24 in (60 cm) thick. Farther north, where a causeway of natural gravel crossed the ditch (*Camulodunum*, 58, fig 6, Sections 1-2; Fig 3.3), the spread of loam was interrupted to leave a corresponding space of rather more than 7 ft (2.1 m) (subsequently cut by the narrower ditch F6, which is Ditch 2A of the Sheepen Period 5) exposing the firm level surface of the natural gravel on which, over all the rest of the stretch, the spread of loam had been laid.

As the causeway, though found reduced in height by wear and denudation, had been revetted on at least one side, leaving the stumps of two stout supporting timber stakes (same Sections 1-2), and could have had no purpose unless to give passage across the ditch, the spread of loam must have been interrupted to give passage through what ran beside the ditch — and this would have been not just this loam but an obstacle standing upon it. That the Sheepen Dyke, like the rest, had had a rampart, was confirmed by our sections of its ditch in Region 3 and on into 5, totalling seventeen. In all of those its silt is overlain by a layer of material, plainly thrown down from the destruction of the rampart by the Romans at the conquest, and thus in the Sheepen Period 2. It follows that the spread of loam had formed the rampart's base. And this has never been questioned, I think, before Haselgrove's preferring to dispute it.

It is this spread of loam, as has been seen on page 73, in and under which, in the dirty gravel that in Section 3 was 4 in (10 cm) thick over the natural gravel surface, were found the sherds of native pottery — grog-tempered in today's correct terminology — surmised by Peacock as left from an occupation prior to the rampart. Haselgrove claims it (foot of his page 169) as not so probably the rampart-base as 'a truncated cultivation horizon'. The first of his several reasons for this is the presence on the ditch's outer lip, in Section 3, over a thin dirty gravel layer above the natural, of the tapering edge of a spread of clean loam, 5 to 6 in thick (13-15 cm), just like the wide one on the ditch's inner side. This, for Haselgrove, makes it likelier still that the loam represents cultivation, its spread being prior to the ditch and cut into two by it. Yet that the inner side's spread was in fact the base of the rampart, as expected, need not be upset if the outer side's had belonged to a small outer bank—a 'counterscarp bank' in the language of General Pitt-Rivers — which our Section 3 stopped short of cutting all the way through.

So too (same fig 7 in *Camulodunum*, 60; here Fig 3.3) did Section 4, where on both the ditch's lips it had spilt on to the silt, and Section 6 where the outer lip was cut by the bottom of a later pit (F15). In Section 5, where the inner side's loam had been partly pushed out over the edge of the rampart — destruction material in the ditch, this plainly was due to its hollowing from above by the intrusive later site F2, the blackened occupation-earth of which is very clear in the section. And in Section 8, already mentioned for the thickness of the loam spread shown in it, the dirty gravel under the tail of this had all been cleared away, and had left it under the forward part very thin — as again in Section 7 — and over-spilling the ditch's lip into the silt: misunderstood by Haselgrove, at top of his page 170, as a 'Period 1 deposit' preceding his loam 'cultivation-horizon'.

It is true that the *Camulodunum* account of all the six sections (pp 60-63) was brief for the first two periods: its readers were expected to have read the account of both for the site as a whole (pp 27-32, with the key-stratification diagram, there fig 1, here Fig 3.4). Also the



notice on page 60 of the pottery beneath the loam in this (with a less amount actually in it) referred readers back to pages 27-8, where the *Camulodunum* forms recognised are stated, namely Cam Forms 217, 218, and 271 (note 1 on p 27). But the presence of similar loam outside the ditch, though Sections 3, 4 and 6 do show it, was nowhere mentioned; and the first to remark it has been Haselgrove. Presumably he would say that even as a 'cultivation-horizon' the spread of loam would be interrupted opposite the causeway. Yet a 'cultivation-horizon' ought to be of ploughsoil, not of clean-laid loam. Nor need the causeway, if 'left for an existing trackway' (top again of his page 170) help to make the dyke any later than it anyhow should be.

Still to be noticed is what he says (same paragraph on p 170) of the four British coins (all copper-alloy *aes*) from the Ditch 1 infilling here assigned (as in *Camulodunum*) to re-deposition from the rampart (and thus of Sheepen Period 2). These were listed on page 161 as follows:

- no 42, Cunobelin: Region 3 south of Section 40, at 5 ft (1.5 m), C022, M246
- no 103, Cunobelin (impression): Region 1, near Section 4, at c 3 ft (0.9 m), C05, M251
- no 129, ?Cunobelin: Region 3, near Section 41, at c 5 ft (1.5 m), C022
- no 133, illegible: Region 3, near Section 40, at c 5 ft (1.5 m), C022.

They are recognised by Haselgrove as late in Cunobelin's reign, the types of all (save possibly one) being his E82.2. He identifies 103, from its clay impression, as Mack 251 (his p 364); it is there his find C05. The other three, his find C022 (his p 367) are no 42 which he fixes as Mack 246, no 129 which he gives as perhaps an E81/2, and no 133, illegible still. He quotes only two of them as found in the ditch c 5 ft (1.5 m) down, but in fact all three of them were; the coin from c 3 ft (0.9 m) down, no 103, is the Mack 251 in his find C05. More serious is his inferring that if they did 'derive from the rampart', this itself would be shown to

be late. For it has never been claimed that everything found in the infilling, pottery or anything else, including coins, got into it from having been embedded in the rampart itself.

For one thing, in each of three places in the primary filling was a Roman coin (*Camulodunum*, again p 161). The silver one (Roman no 4, a Republican *denarius* of c 87 BC) might perhaps have been on the site before the conquest, though Republican silver was current still in Claudius' time (C H V Sutherland in *Camulodunum*, 152-3). But the other two are *aes*, one illegible and small (Roman no 260, /E3), the other an *as*, perhaps of Claudius himself (Roman no 189). And Roman *aes* of his time, beginning with that of the year of his succession, AD 41, was the first of any to be brought to *Camulodunum*, with the army in 43 (Sutherland, *Camulodunum*, 158-9), whence I took it as fair to say the same for these two which then — the *denarius* also — will have been dropped when the ditch was filled in. The same I took to be true likewise of novel Roman pottery, two brooches and even four dozen pieces of Roman military metal equipment (*Camulodunum*, 30-31 & 161-2). Two other pieces of military equipment, the Claudius *as* no 137 (probably of AD 41, pp 147 & 162), three coins of Cunobelin (nos 43, 75, & 108; pp 138-40 & 162), and a little early Roman pottery were all found in the filling at two places in Region 5, where the backfilling of the ditch needed completion a year or so after 43 (*Camulodunum*, 32 & 162). The likelihood that its primary depositing did belong to that year, was confirmed wherever it was covered by a darker layer representing occupation, which in turn was covered by a second one. The evidence for the dating of these two layers to Periods 3 and 4 respectively, divided at AD 48-49, is exhaustively summarised in *Camulodunum* (pp 32-8) and need not here be repeated.

Against all this, any notion that the British coins found in the filling had come from the body of the rampart, and being late themselves, would make the rampart later still, cannot stand when so very much else in the filling was not pre-conquest at all. Deriving any from the body of the rampart, therefore, becomes a speculation too slender to affect the rampart's own date. Yet even so,

### DIAGRAMMATIC SECTION

#### A KEY TO CHRONOLOGY

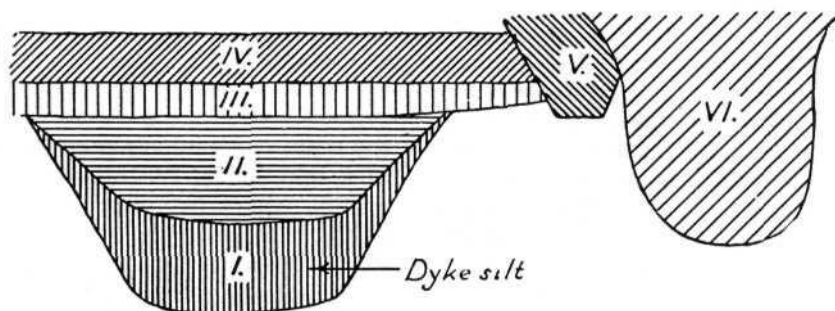


Fig 3.4 Diagram of the key stratification for the 1930-39 excavations at Sheepen. Reproduced from *Camulodunum* (fig 1) by permission of the Society of Antiquaries of London. [Pages 78-9]

questions remain. Errors by Haselgrove need not exclude there being errors in *Camulodunum*. Were we right to believe that the Sheepen Dyke was coeval with the Sheepen occupation?

So far, everything dealt with here has concerned *Camulodunum*'s 'key stratification': that of the dyke's deposits and connected major features which together declare a consistent sequence of periods, 1 to 6 (see again p 27 there, with diagram fig 1, here Fig 3.4). Within it, two historical dates are firm, and consistent with them are the datings afforded by finds: that of the conquest in 43, dividing Period 1 from 2, and that of Boudica's revolt, whether in 60 or 61 (p 70 above), dividing Period 4 from 5 (with the directly following Period 6). This approach, explained with the diagram on the same page 57 in *Camulodunum* in very precisely-worded terms, extends for each of the regions also to non-key stratified sequences, and also to separate treatment of each region's features arranged by period, whether from stratifications or only from finds (or just by analogy). And it is there, among the datings not fixed from a stratification but inferred (most often) from finds, that errors in *Camulodunum* may now be discerned or suspected. Since in general here we are concerned with Period 1, and in particular now with relating the start of occupation to a date for the dyke, this is where the question of errors most needs to be faced.

### Sheepen's pre-Roman occupation now divisible into stages

Throughout the forty years before Haselgrove's 1987, we have seen that Period 1 had been treated as an undivided whole: the first to see a sequence of stages within it is Haselgrove. But this has led him to something more: the concept of a hilltop occupation 'on no large scale', contrasted with a general or main one. Not to have attempted the distinction before may perhaps be called an error, but the prior call upon us came when the new work on Germany, first known to us only some twenty years ago, had altered the case for c AD 10 as our starting-date, fixing the abandonment of Haltern at AD 9. For as explained here above (pp 71-3), our Sheepen dating rested first on the imports, essentially of pottery, shared here with Roman forts on the German frontier, and with Haltern in particular. For as Period 1 was being treated as a unit, import-dependent dating should apply to the whole of it. Belief that an overlap with Haltern, of several years, was required by our earliest imported pottery (late Augustan) was upheld by Hull's final starting-date, c 5 BC or barely after, and has been kept by Niblett's and Sealey's, c AD 5. Yet any late Augustan start-date, for Haselgrove, ie any pre-AD 14, though allowable, appears not strictly required (his p 169). For late Augustan potters may have been at work

till later than that, and alongside some of the Tiberian; thus the principal time — he says '*floruit*' — for the importing will be that of Tiberius, AD 14 to 37. But never has this been denied; the question has been for how long, before that, might even a little importing have been begun.

That of the Dressel 1B amphoras stands apart — Haselgrove explains them like Sealey; it is the 'Arretine' *sigillata* pottery that makes the question here, with the pieces bearing stamps of its potters. Those occurring also at Haltern came most from the 'northern' or hill-foot areas (Regions 1 and 4D). He includes them along with the Tiberian there, though not with the later Tiberian that mostly comes from the uphill Regions, 2 and 4L (his 'southern'), now accompanied by more South Gaulish *sigillata* than the 'northern' have. And this uphill shift he confirms from dates of the respective regions' lists of British coins. So far so good, and this is introducing stages in the Sheepen main or general occupation. Different is the case of his 'extreme south' of the site, the hilltop Region 5 with its eastern flank in Region 6. There, as we have seen already, pre-conquest *sigillata* is virtually absent: totally so from individual habitations such as Site Y1, Sites L2, L6, and L7 with their pits, and Site L4 (*Camulodunum*, 118-19, 122-4). With their native wares, however, mostly abundant, imported Gallo-Belgic was found in some quantity — mostly *terra rubra* and *terra nigra* platters and cups. (Not, however, the matt and mostly micaceous platters Cam Form 1, recognised now to be primarily Central Gaulish.) Though frequent on the site elsewhere, both pre- and post-conquest (*Camulodunum*, 207-213, & 215-21), their continental contexts allow their importing already c 5 BC — as Hull well knew when he came to that date for the start of all Sheepen's occupation. Haselgrove, too (his p 170), though not mentioning the Gallo-Belgic ware, has noticed some early brooches here, of which one is from Site L2 (*Camulodunum*, 308 & pl 89,3; Hull & Hawkes 1987, 183, pl S3, 0004). And this 'extreme south' is where we have seen him suggesting occupation in it early, but on no large scale: not the main occupation as yet, but a lesser one, only on the hilltop (one must add, and its eastern flank). With all the native ware here, there was no *sigillata*; but the early date which he proposes because of that, was not too early for the presence of the Gallo-Belgic. The natives up here can have preferred it, indeed even after *sigillata* became available, as obviously cheaper. Quite different was the north and north-east of the site, nearer the river, by which presumably all the imports will have been brought. There, they included most of the 'Arretine' *sigillata*, clear sign of the main occupation's greater wealth; and on pieces of it had ten of the ware's dozen Latin-alphabetic graffiti, which (whether cut by natives or traders) show the wealthy at least being touched by Romanisation (*Camulodunum*, 49, 191, & 284-6). Thus wherever imported pottery was entirely Gallo-Belgic, occupation may take its dating just from that; and this allows the dwellers in the site's 'extreme

south' to have been settled there already c 5 BC — granted that in social level they were relatively modest. So Haselgrove here can be right; and to enhance his distinction of theirs from the general occupation, in character and also in starting-date, we have the fact that their imported pottery was all Gallo-Belgic. With the Dressel 1B amphoras, here just mentioned, we have already considered the northerly Area E, in Region 2 just west of 1, where occupation might have had a still earlier local start, connectable then with those amphoras' landing from the Colne, and equally the native pottery under the rampart of the Sheepen Dyke, in the adjacent Region's Area F. But now again, that subject must return us to the Sheepen Dyke itself.

Our upholding the *Camulodunum* interpretation of the excavation evidence needs no repetition. It is Haselgrove's suggested date for the building of the dyke, at the latest of the stages he has discerned within Period 1, that needs consideration before we go further. His case for a shift, at the stage preceding the building of the dyke, of the occupation's main intensity southward up the hillside, from Regions 1 and 4 into 3 and 4L (his p 169), must call for acceptance. But the dyke is a major defensive work guarding nearly all the occupied area: the northern regions (Region 2 alone being left outside it), the uphill regions where its principal entrance was, and then beyond much of the hilltop, where it turned south-eastward as Ditch 1B and where there was a mint for British coins (Haselgrove 1987a, 167). Yet this extreme south — site of the old Late Bronze Age settlement (p 3) — is just where we now can have an Iron Age phase starting earlier than the main one, and continuing too while the latter, ampler and wealthier, spread towards it after starting in the north. Haselgrove's reasoned contentions have here been admitted thus far, but there still remains his date for the building of the dyke. Why delay this till the whole expanse it was to guard had got settled occupations? Shouldn't the dykes have encouraged their spread by being built before it ever had begun? But from this we must turn to its relation with the dykes outside it.

### Sheepen Dyke and the dykes outside it

Mr Crummy's chapter on Gosbecks (pp 95-105) can have left no reader in doubt of that site's prime importance in the history of *Camulodunum* as a whole. And already its summary (pp 7-8) in Chapter 2, and that of the account of the dykes to the south-west and the west (Chapter 2), has led to our showing that Heath Farm Dyke not only made the site a supplement to these dykes, but after stretching far

out from Gosbecks, north of east, ran onward later to be covered over, at the south-eastern end of Bluebottle Grove, by the rampart, still upstanding, of Lexden Dyke (pp 48-50). The latter west-facing rampart and ditch, bent along there to face the south-west, is one of the two — Shrub End Dyke, running in front of it, being the other (pp 51-2) — that leave the Gosbecks site right away outside them.<sup>3</sup> Since that site in its Roman phases stood in a contrast, just as before, with the whole development of *Camulodunum* on the Colne, it is natural to see it throughout as apart from the powers that effected that, the first of them known to history being Cunobelin's, whose sons, and so he himself, belonged to a royalty from farther inland, the Catuvellaunian. Thus the contrasting position of Gosbecks will mark it as a centre for the region's own folk, the Trinovantes. And the dykes' topographical order must indicate more. Whereas Sheepen Dyke, at its south-west entrance on the hilltop, turns south-east as Ditch 1B and ends on a natural obstacle — the stream in its steep-cut valley at the foot of the slope — both Lexden Dyke and Shrub End Dyke, after likewise turning south-east, end abruptly, just where taking them farther would have given them a south front, which prospection has failed to trace. One sees that it could well have been intended, but something else is what topography indicates next.

It is the earthwork line that reverses those dykes' exclusion of Gosbecks from their system: that of Kidman's Dyke (pp 33-4), which begins by reinforcing those that protected Gosbecks on the west, and then turns northward in a curve that brings it in to join Shrub End Dyke at a right-angle, underneath Lexden Straight Road, thus linking the protecting of Gosbecks with that of the Colne-based *Camulodunum*. And next again, on Shrub End Dyke a little farther north, Hull's excavation of what we are calling 'Peartree Junction' (pp 46-8) showed the start from it, inwards and facing NNW, of Prettygate Dyke (pp 46-50 & 61), which as shown by the excavations recorded below (pp 48-50: Prettygate Junction') ended in a sharp-angled join with Lexden Dyke. And so, well after the rampart of the Lexden Dyke was built, close by and over Heath Farm Dyke's ditch-fill, Prettygate Dyke served to extend the Kidman's link between Gosbecks and the Colne-based system.

Accepting, then, Gosbecks as always native Trinovantian, one is left with the Catuvellaunian power as the context for the Colne-based earthwork'. Sheepen Dyke, guarding their heart-site, can thus be confirmed as what excavation goes to show it: the work (for it anyhow need be no earlier) of Cunobelin. So we now can discern two phases — from topography — in his reign: a first one, with Trinovantian Gosbecks left outside the system, and a second one, with Gosbecks brought within it. To this we may add the extending, from

3 The construction of Lexden Dyke South and Shrub End Dyke South may not have left Gosbecks isolated from the rest of *Camulodunum* to the degree that CFCH believed (pp 170-71).

4 See pages 172-8 for an alternative view.

Gosbecks southward, of protection against the west by means of Oliver's Dyke and Layer Dyke, carrying it down to and on beyond the Roman River (pp 46 & 34 resp). And with this third phase comes the decision to defend the whole peninsula, between that river and the Colne, by constructing Gryme's Dyke. The length between ends of Gryme's Dyke is almost 3.5 miles (5.5 km); but in its middle there comes, south of Dugard Avenue, a stretch which a cross-section, cut in 1977 (pp 109-115), must show to have never been built until after the conquest. The dyke's being still, in AD 43, not quite complete, should agree with the topography in showing it as altogether late (even if partly a two-stage work) (pp 29 & 56). And if this same late phase be allowed for our other such long line of earthwork facing east towards the sea — Berechurch Dyke with Abberton Dyke which is its southward continuation from the Roman River (pp 24-6 & 54) — that additional undertaking might help to explain the intermission in Gryme's Dyke.

To which of our three phases, then, should Sheepen Dyke belong? Haselgrove's date for it, 'late Tiberian' (his p 170), so about or after AD 25 or 30, would place it in the third — this being matchable, broadly at least, with the last of his Sheepen stages. Yet that phase's major feature on the west, Gryme's Dyke, lay outside of everything else. The interior linking of Gosbecks to the Colne-based system, by Kidman's and further by Prettygate Dyke, would thus have been needless had they not come already in our second phase. Of the Colne-based dykes to which they linked it, Shrub End Dyke has its main long line nearly north-south, Lexden (with Moat Farm Dyke beyond the Colne) has it nearly NNE-SSW; and each leads then to a gently obtuse-angled bend, approximately south-east. Since each of the two has a second-phase dyke abutting on to it, the Colne-based phase that they must belong to is the first. Sheepen Dyke, the Colne-based system's innermost, and, proportioned just to protect a large occupation site, has its main line almost exactly NNE-SSW, and then an obtuse-angled bend — which is sharp because at an entrance — nearly SE. These two lines — if we omit the later Ditch 1A, running off south-west from that entrance to protect more ground (p 51)— have together a length of about five-eighths of a mile (1 km). But what matches Sheepen with Lexden Dyke, and Shrub End Dyke too, though rather less closely, is its plan; the obtuse-angled bend in this is of 100°, and although it is gentler in the other two, the likeness is plain.

The inference in 1947 drawn from that, and from Sheepen's being the farthest in from the west, was that 'Sheepen Dyke completes the main dyke system in its final form' (*Camulodunum*, 23). But this was when knowledge of the dykes outside it, and certainly of those beyond Lexden Dyke and Shrub End Dyke, could point to little more than a priority for Gosbecks — then referred to by field-name only, as 'Cheshunt Field'. Heath Farm Dyke's filled-in ditch beneath Lexden Dyke's rampart in Bluebottle Grove, though observed in 1940 by A F Hall, was thought to be Prettygate's (*Camulodunum*, 12); and elsewhere

too there was misunderstanding of the sequence. And that its final form, with Shrub End Dyke and Lexden, had in Sheepen Dyke 'a last line of defence', letting it seem to be last in time, was unproved; it was also unsuited to its dating (*Camulodunum*, 29), along with the start of the whole site's Period 1, to c AD 10. For the whole of the system's final form (same page 20) 'should be the work of Cunobelin', and the date for his accession (page 29 still) is the same: 'c AD 10'. So what is to be drawn from re-considering the whole matter now?

That accession-date for Cunobelin has already been seen to be approximate, although its limits should be narrow. For though AD 7, recommendable as the date of the flight of his precursor, Dubnovellaunus, is not explicit in its only record (which was made by Augustus to whom he had fled) but is inference from that record (p 88), yet it can tell towards making an earlier date unlikely [unless only by a year or so — see p 94]. Any years of interregnum after the flight would leave less room for allowing it a later date, which otherwise Cunobelin's accession must itself set a limit to. And Cunobelin's issues of coins, by their huge known volume, even though he lived until a little after 40, are agreed to mean a reign of hardly less than thirty years. Yet he came in here as a Catuvellaunian intruder. So his first years, round about 10, would be the time for his starting on defences, for his power-centre based on the Colne, which would shut the old Trinovantian Gosbecks out. And this is just what the dykes' topography shows. What then should be the time for his starting on their innermost, Sheepen Dyke? Shorter of course than his outer two, so quicker to complete; but it looks as if neither of those had been completed, on the south, when he changed his mind and had Gosbecks, on the west, linked in: by Kidman's Dyke with the one, and next by Prettygate Dyke with the other. Gryme's Dyke, finally, he planned to be a westerly defence for the whole peninsula; it was only just short of completion at the Roman conquest. Would he be likely to have left, till that final phase or near it, his Sheepen centre still without a dyke of its own? Yet this is what Haselgrove would like.

That the main occupation was most intense at first in the north, nearest to the river, and was next extended by a shift of weight southward up the hillside, we have seen him inferring quite properly from datable finds. And that a less intense occupation, on the hilltop and its eastern flank, we have found confirmable likewise. But we have had to ask why, at Cunobelin's accession, he should not have built his dyke in his very first years to guard in advance the whole site for a main occupation. Moreover the design of Sheepen Dyke, with its obtusely-angled bend, broadly repeats those of Lexden Dyke and Shrub End Dyke. Both of them were shown here above to belong to his first phase, so it would be strange to have Sheepen Dyke really belonging to his last, when Gryme's Dyke was planned on such strikingly different lines. Thus from the Sheepen site on the one hand, and on the other from the dykes outside it, enough can be drawn to let the date most likely for Sheepen Dyke be an early one. That should mean close after the year to which the coin and

other evidences point for Cunobelin's accession, and which, though for starting an undivided Sheepen Period 1, was propounded in the *Camulodunum* of 1947, ie about AD 10 or towards it. The case propounded here for it naturally differs, in some respects.

## Points in conclusion

No longer need Sheepen Dyke be guessed the last of the obtuse-angled three to have been started: all in design are like enough to one another to have been parts of one overall plan. And for making the intruder Cunobelin its author, in the first of his policy's phases, we have the evident purpose of the two exterior dykes: defending the Colne-based area for himself while shutting out Gosbecks, itself on other grounds inevitably native Trinovantian. Even if Sheepen Dyke were not the first of the three to be begun, it must long have been finished when the other two, having extended so much farther, were apparently stopped unfinished on the south — when his policy ceased to require the exclusion of Gosbecks, and brought the results we have seen to have belonged to his second phase.

Again, in the chronology from finds, the date of the abandonment of Haltern, corrected to AD 9 from the old 16, no longer comes certainly after the beginning of the main occupation at Sheepen — though AD 10 for this, being only approximate, might perhaps allow it a year or two still before that. When Hull gave it 5 BC or a date just after, and Niblett and Sealey one later by no more than ten years, each was fitting it into the Haltern date-span as now corrected — just as in 1947, before the correction, we fitted the date we then propounded, an approximate AD 10. And notwithstanding the correction, this is what again is propounded here — whether or not the approximation may cover some year just before, and perhaps that of Dubnovellaunus' flight which is guessable as AD 6 (p 94). As Haselgrove has insisted, types of *sigillata* (stamped or otherwise) found at Haltern can still have been exported elsewhere till somewhat later. This pottery could have arrived in Britain to offset some of the shrinkage of business with Roman Germany after the Varus disaster of AD 9 (p 72 quoting *Camulodunum*), and indeed it may have come in greater quantities when Tiberius in AD 14 had succeeded Augustus (p 80: Haselgrove again), to whom, with Dubnovellaunus in favour, Catuvellaunian Cunobelin here could have hardly been welcome. And that a Sheepen occupation could have begun c 5 BC, as I followed Hull in proposing in my 1983 (page 12), but which now has become too early to be seen as the main one, it is acceptable still as that of lesser scale on the hilltop, distinguished as such by Haselgrove, and now further (also on the eastern slope), page 80 here with allowable support from its imports of Gallo-Belgic tableware.

Occupation beginning thus in Dubnovellaunus' time, it would be only less early than the smaller and perhaps intermittent occupation, down by the river, if that can be connected with the landing of amphoras (Dressel 1B) and there known from fragments, suggested now to have started in the time represented by their Lexden Tumulus counterparts (p 88, Fitzpatrick 1985). While that is at any rate guessable, the hilltop occupation's being begun before the main one, so about or little after 5 BC, may be conceded a higher probability. The main one itself can be left to begin (as propounded in *Camulodunum* but now with a wider perspective) towards or close about AD 10. Sheepen Dyke would then be ordained, very soon, by Cunobelin the newcomer.

## Sheepen Dyke and the Sheepen occupation

We have given a full discussion of this in this chapter; what remains to follow here is a concordance, for the dyke's successive features, with their description in the 1947 *Camulodunum*, and notes on recent publications that deal with the Sheepen occupation, especially that of the 1970 excavation by Rosalind Niblett.

- a) Discovery and general description of the dyke, numbered as the Sheepen site's Ditch 1, from excavation 1930 onwards: *Camulodunum*, 23-6.
- b) Stratification, starting with its ditch and the traces of its rampart, and continuing with its primary silting in the site's Period 1: *Camulodunum*, 27-30, but with dating of the period's start, 'c AD 10', now replaced by one fifteen or a dozen years earlier, here pages 80-81, with possible previous use of a portion of the site, for unloading goods shipped up the river.
- c) Detail of dyke excavations, with plans and section: *Camulodunum*, 57-9, 71-2, 77-83, 85-7, 108-11; secondary extension south-west, as Ditch 1A, *Camulodunum*, 109 & 111; primary extension south-east, as Ditch 1B, *Camulodunum*, 112-15, 118, & plates 106-110.
- d) Remains of turf, timber and clay to reinforce the gravel of the rampart: *Camulodunum*, 87, as here page 78 above.

I contributed a brief review of the site (Hawkes 1985) in its Roman periods (Periods 2 to 6) to an article of wider scope by Professor M Todd (1985); it owed many points to discussion with Andrew Fitzpatrick, who kindly let me use his lecture of 1983, now embodied in Fitzpatrick 1986. In both, some parts of the 1947 account are reconsidered, leaving aside, however, the site's pre-Roman period, with Sheepen Dyke, which has been noted as fully reviewed in the pages above. For Roman amphoras as already then imported, see further Fitzpatrick 1985, 332 citing the Lexden Tumulus for this.

Parts of the 1947 account are re-considered also, with some revisions and added improvements, by Mrs Rosalind Niblett (1985) on her important Sheepen excavation of 1970. This was in and to the north of the 1947 Region 4 (six sites and some further cuttings) and in Region 3 (one site and a further cutting), all in locations previously unexplored, towards or at the foot of the hill's north-eastern slope towards the Colne. Some of the revisions, dealing with the finds, arise in parts of the report contributed by others, eg Dr Webster (pp 114-16, metalwork; cf Fitzpatrick noted above) and Dr Paul Sealey (pp 98-111 & figs 53-6, on the Roman amphoras; cf Fitzpatrick 1985), Sealey's 1985 being an expansion. These matters have been summarised on

pages 73-8, and others briefly in Hawkes 1985 (Dr Sealey, like Fitzpatrick, having kindly allowed the use of his work in advance of publication).

Mrs Niblett conducted her Sheepen excavation, with many helpers, as Director for the Excavation Committee that preceded the Colchester Archaeological Trust, and she mentioned its findings briefly in the book that she published, under her maiden name B R K Dunnett (1975, 23-6). Its subject was all the land of the Trinovantes, pre-Roman to latest Roman; see its pages 19-23 for an account of the dykes, and pages 16-19 below for its treatment of the Lexden Tumulus.

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## 4: The Lexden Tumulus: viewed in archaeology and history

byCFC Hawkes

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### Local preliminaries

This famed round barrow (for that is what 'tumulus' means) rises now within Colchester's westward spread towards Lexden, shared between the gardens of two houses in Fitzwalter Road (south side, nos 30 and 36). Successive maps, from the 17th century on, mark its position, and Hull, listing these in his *Roman Colchester* (p 252), mentions one of c 1845, as showing it then already within the bounds of Lexden Park, formed earlier as the domain of Lexden Manor.

North of it lies the area where (and first in 1904) have occurred the cremation-graves that form the Lexden cemetery (1st century AD, and of its first half mostly), the earliest seeming of the years around its start, all described by Mr Crummy below on pp 164-9 (his Grave 10 is directly beside the tumulus). That well-known for its British mirror came from the area's north-east edge, near Lexden Grange. Moreover two amphora-finds, though neither with recorded grave, should yet be connected with the cemetery; and they both are of form Dressel 1B (Cam 181) which ceased manufacture (in Italy) c 10 BC. Survivals, in excavated graves later than that (by however much or little) are very rare (on the site-find fragments at Sheepen, see pp 76-7). These two amphoras, which are hard to explain if each was really not from a grave, might thus mean a date for the cemetery's start little later than the date we shall be giving for the tumulus, from which the numerous amphora-fragments represent the same forms, with only one other, as we here shall be seeing below. One of the two, with rim stamped HI.. (*Camulodunum*, 251 with 214, fig 45,7) came from about the area's western edge ('just inside Lexden Dyke', 1932); the other, found 1823, whole except for rim, came seemingly from within it, at a spot which on Ordnance Survey maps is marked as east of St Clare Road, about 250 yards (some 230 m) north of the tumulus —though called 'perhaps' from the tumulus itself in *Camulodunum* on page 251, and without the 'perhaps' on page 13, note 3. This 1823 find (Colchester Museum accession number PC 700) is number 3 in *Roman Colchester*, 292, with its Ordnance Survey find spot marked on map plate 43.

On the other hand, number 11 there (*ibid*) is of the later-lasting amphora-form Cam 186 (Dressel 7-11), which is south Spanish and is conspicuously absent from the 1924 tumulus finds (p 88 below). It is undoubtedly the amphora stated to have been found in 1860, with other pottery, 'in' the tumulus (Ordnance Survey maps, with some details in Laver 1927, 241-2). Dr Jennifer Foster

(1986, 1), though surely too cautious with the qualification 'probably', alters 'in' to 'near or on' the tumulus, and might be right. For it was seemingly in 1860 that Wellingtonia trees were planted, some around it and standing today, and one on its summit which had to be removed from its hole there in the course of the 1924 excavation (all in Foster 1986, pls 3-7). But whether the planting of this, or (less likely) of one of the others, disclosed the amphora and pottery, they anyway must have come from an intrusive grave which thus would not affect the date of the original main grave.

The amphora, in the Colchester Museum (PC 702) is the third of the three that were given in 1871 by the owner of the park, Mr Errington. Of the other two, PC 700 is the Dressel 1B found in 1823, well away north of the tumulus as stated above; PC 701, of again that form, but with no recorded find-spot, might then be supposed among the pottery found, with other things also thought Roman, in some fields adjoining the park when they were taken into it sometime in the 19th century. Hull (1958, 252), noting that, wondered whether PC 702 might really have been also among that pottery. And while its coming from the tumulus tree-planting in 1860 must seem more probable, that pottery and the other things were found when 'several tumuli' were levelled when fields were being added to the park. Not known from any earlier attention, perhaps they were too low to have attracted any, their levelling and yielding those finds went unrecorded until, in the Essex Archaeological Society's *Transactions*, 12 for 1912 (1913), this account was prefixed in the Morant Club's report on its exploring, in 1910, the neighbourhood's uniquely large barrow called The Mount, a good half-mile away to west of the tumulus. This found it to be greatly disturbed, yielding nothing but broken Roman tiles and oyster shells.

### The excavation and accounts of it

None of the earlier finds need affect our estimation of the burial under the tumulus. That must stand on what was found in its excavation in 1924. The report of this by P G Laver, in *Archaeologia* 76, 1927, written after the untoward death of his brother H E, whose leading part in the work he admiringly acknowledged, formed the basis of the three much briefer accounts written later: by the present writer in *Camulodunum* (13 with frontispiece map); by Hull (1958, 253, noting also the

finds of 1860); and by B R K Dunnett (1975,16-19). But these, and P G Laver's report itself, are all now superseded by the thorough re-assessment, of the excavation as published and of all of the finds, forming the work by Dr Jennifer Foster (1986).

For this — 215 pages of text and illustrations — the Colchester Museum gave her access to the finds, unpublished as well as the published, and to the original excavation-archive documents, nearly all from the hand of 'Ted' Laver (the H E introduced here above: a master mariner, newly retired at the time), whom she shows to have really been the work's one executive director. His death in 1926 left its publication to his brother Philip (P G) — at whose own death in 1941 this material passed to the museum. From their letters, and from Ted's many hand-written site-notes (also corrections to these when typed), and his sketch-plan and any sketch-sections, she soon found the published report to have shortcomings, some of them at critical points, which she thereupon set herself to remedy wherever she could. Though the excavation methods of the earlier 1920s sometimes limited her success, as she freely admits, and have left some questions open, yet her industry and judiciousness have let new meanings be advanced from the evidence — as even a very brief summary here should show.

### **Dimensions of the tumulus and natural formations under**

The dimensions obtained by Ted Laver from his measurements, taking the mound as approximately circular, made its diameter 90 ft (some 27 m), but this is clearly an average, as Foster's quotations from his site-notes imply (her pp 21-3 and 31 ff). Her own plan (fig 7, p 22) shows how his two radial sections — referred to by the letters E and W, but in fact aligned nearly NE and SSW — cut across the surrounding ditch and that the diameter to cover the width of this is approximately 100 ft (30 m). He reckoned the mound to be 9 ft (2.7 m) high above ground level, but excavated sections found it a little less than 7 ft (2.1 m), above the sandy loam that overlay the natural gravel (see drawings reproduced as Foster's figs 32-3, pp 32-3). The thin 'black line', beneath the heaped-up gravel of the mound and lying on the loam, which he took to mark a surface of turf, buried and decayed, and of turves purposely laid where it slanted down in the central grave-pit, has been found instead (by Dr Martin Bell from a sample still preserved in the museum) to be probably of humic down-wash out of overlying layers (Foster 1986, 148-9). This implies that the loam was skimmed of natural turf before the mound was built on it, and, more importantly, negates the belief that the line's downward slant has to indicate the original edge of the grave-pit.

### **The grave-pit**

The plan of this in the published report (Laver 1927, fig 3), reproduced as Foster's figure 9 (her p 26), and location-plan of Laver's sections (the lower plan in Foster 1986, fig 7), gave it an oval or elliptical outline, 29 ft (8.8 m) by 18.5 ft (5.6 m). On his own plan (Foster's fig 14, opp p 36) the similar outline sketched is somewhat smaller. From his sketch-sections and site-notes (reproductions and transcripts, her pp 31-46, figs 11-19), it appears that he used for it, though in places inconsistently, the slant of the black line wherever available, but on the west (really almost south-west) used conjecture which the published plan has altered into its own. Besides this, Foster's discussion (pp 44-7) finds other discrepancies, especially on the north (really almost north-west). She infers that the grave-pit's original edge need never have been anywhere found at all. For her preferring a pit of approximately 26 x 26 ft (8.0 x 8.0 m), or slightly oval at most, and 5 to 6 ft (or 1.5 to 1.8 m) deep below ground surface, we turn to her Chapter 5 reconstruction (pp 159 ff). Such a size would mean that it extended farther north than the excavator thought. But whatever was the actual shape of the area dug for it, more important is the plotted distribution, within it, of the grave-goods. And to compare with that recorded on the published plan (shown in Foster's fig 9, p 26), we have her exactly-reconstructed distribution (fig 43, p 168, with p 169). The area that she shows this to cover is a rectangle measuring some 15 ft (4.6 m) north-south by 20 ft (6.1 m) east-west (p 163), which are appropriate dimensions for an oblong mortuary chamber, standing up inside the pit and containing the whole of the funeral deposit. Over quite a wide expanse of the floor, there were lying cremated human remains of which, contrary to what was alleged in the published report, the museum has preserved a quantity, weighing 346 grams (Foster 1986, 133-9, with the aid of Mr Don Brothwell). The four samples examined as most significant, together making nearly half that total weight, give a balance of evidence pointing to cremation of a man who had died in maturity (its sharing by a female or a youth being possible but doubtful).

### **The grave-goods: their character and condition**

That they are unique for a burial in Britain was recognised at once, and the published report showed much of their variety and richness. Too little indeed was then known to let the brothers date the grave precisely; it was seen to be late pre-Roman, and the fame of the Camulodunum king Cunobelin, who died very soon before the Roman conquest began in AD 43, rather naturally prompted the guess that the burial was his. But even in the 1970s and early 1980s, when signals



were first being given for a date less late, mainstream archaeology still took the matter no further. The ground for advance to be made on was laid by individuals, expert in specially-relevant studies, but few. And now Dr Foster, acknowledging what they did, has given us all the grave-goods, catalogued and drawn (with 16 plates of photographs): bronzes (British and imported classical pieces); a possible casket; chain mail; silver- and gold-work; iron objects such as ferrules, pins and broad-headed nails; plates (some curved but more often flat, straight-edged or scalloped), bronze-coated studs and curved bars (not tyres of wheels), all convincingly proposed (Foster 1986, 94-105) as from a perished wooden chest rather than a 'palanquin or litter' as some had fancied; and flat bars including some likeliest as parts of a folding stool (*ibid*, 1986, 108-9) — the Roman ceremonial prerogative of persons of authority, such as emperor, consuls, and generals (iron when in the field) (*ibid*, 188). With the aid of sections on organic remains, soil samples (by Dr Martin Bell), amphoras (in full by Dr David Williams), and the scanty other pottery (by Valery Rigby), Foster assesses the whole assemblage on page 178, having stressed as expected the small silver 'medallion' (more properly, medalet) with a portrait of Augustus, cast from one of the *denarii* minted in Spain 19-16/15 BC (Dr R A G Carson attesting; *ibid*, 90-92, 175).

Nearly every sizeable artefact has been broken, or purposely damaged and often left lying incomplete. Small ones only have survived intact, some in scatters from the textile or leather on which they were mounted. Reasons for the breakage or damage can only be two (*ibid*, 164-70). The first, credited by Laver as affecting a northerly segment of the grave, and bounded on the published plan by a curved broken line (*ibid*, 26, fig 9) is the forcing of an entry into it by subsequent robbers, who would ransack it as far as they were able to reach. That they never could have ransacked all of it, leaving the mound as intact as it is, may further be judged from what Foster presents in her pages 166-8: distributions of finds in the pit not only on plan but in vertical depth. Her figure 42 (*ibid*, 166) shows, from what Laver recorded, that most were found on or in or very near the grave-floor within the extent of the chamber proposed, as aforesaid (*ibid*, 168-9, fig 43). Only in the north and west of it were any found higher in the filling. Apart from a George III halfpenny in the Wellingtonia tree-hole, there were seven groups of these, all at various depths. In Foster's tables, arranged by the excavator's 'site' numbers (*ibid*, 50-51), they are as follows. 'Sites' 25-30: iron fragments and nails, with amphora (pieces) from sites 25 and 30 (but at site 30 not with Pot 3 which is really from site 37); site 24: sheet-bronze piece no 48 (wrongly stored as from site 34; *ibid*, 78); and site 33: bronze palmette-escutcheon (from a Roman bowl of Augustan date; *ibid*, 63-5, 176-7) of which the twin lay more than 12 ft (3.7 m) away at site 5, with a great deal else in the sandy loam of the floor at its farthest east end (*ibid*, 49). All these little things picked from the filling are explained as droppings by the robbers here presumed, to be buried by the filling when the chamber-roof finally collapsed. But the 'line of ancient

opening' on the published plan, giving a robbed northern segment of the grave an exact limitation, was never really proved, so that Foster has rightly discounted it. The best entry for robbers of a mortuary chamber — and chamber there must have been for such an entry to be possible — would have been through a side of it, and this would seem indeed to have been northern to judge by the broken-off pieces and pot-fragments scattered there, and the pair of pits (containing some fragments) dug down there into the grave-floor. That, though one cannot say more, is clearly acceptable by itself.

Yet the grave-goods' breakages or damage have extended everywhere — beyond the powers of robbers to inflict them all. This brings us to the second of the only two reasons there can be for them (Foster 1986, 170). It is, that the majority of all will have deliberately been broken, to conclude the funeral ceremonies: ritual breakage, 'killing' them to make them in the after-world mystically whole again, for the dead man, himself restored, in his after-life. That rite, known in various times and places in the world (*bris rituel* in French), is not thus attested for the earlier British Iron Age. For the middle and later 1st century BC, as at Aylesford in Kent and the Welwyn region north of the Thames, and any rare furnished graves of the period elsewhere, the burial of goods unbroken contrasts with it sharply. There are non-funerary cases, however, of broken goods in deposits such as offerings in temples: in the Hayling Island example cited by Foster (*ibid*, 170), the objects are partly of Iron Age date, although Romano-British otherwise. And in burials of Roman age, as in some examples in Belgium, Britain has certain cases of ritual breakage, at any rate of pottery. But in spite of such occasional later continuance, in the tumulus this is unique in its range and destructiveness.

## Archaeological dating

Just as the ritual breakage must seem an innovation in the British Late Iron Age, so, it appears, was the building of a tumulus at all. Foster (*ibid*, 75-186), considering and listing the Roman-age barrows in Britain, compares them with the numerous Roman-age barrows in Belgium. But the Roman age there, having begun from the conquest by Caesar, had nearly a century's start over ours, beginning under Claudius in AD 43. Thus while there need be no doubt that the practice in Roman Britain could be influenced from Belgium, its first showing there could have prompted the adoption of the like in the British Late Iron Age. And the group of four excavated barrows there (actually in Luxembourg) with cremations showing at present the practice first, are those called Goebange-Nospelt (because they lie between those two villages, dated to 20 BC and in the next two decades) (Werner 1977, 370-74, 397; Haffner 1969, 243 for date; G Thill 1966, 1967a & 1967b for excavation and finds). Thus while the

Wessex Late Iron Age barrows, few and small, such as Hurstbourne Tarrant (Hawkes & Dunning 1931, 304-9), may reflect the presence of several in north-west Gaul, our tumulus, near the coast facing north-east Gaul (whence the Rhine-mouth sea-route recorded this way by Strabo, iv. 5, 2), might already reflect it there, soon after the Goebblange-Nospelt date. Its size, and the wealth of its grave-goods in Roman imports, go together in proclaiming it the tomb of a man both great and with Roman connections.

His archaeologically-datable goods, for any tighter chronology, are first the Augustus medalet, later (by little or by more) than the years close around 17 BC; next, among the more loosely-datable classical bronzework, the twin palmette-escutcheons from a bowl, such as have dates in Roman Germany of 15-9 BC and c 10 BC to perhaps AD 16 (Foster 1986, 175-6; one at Fingringhoe had no associations). Some among the British metal pieces afford comparisons, but they take us little further (*ibid*, 177-8). The amphoras (though all of course in fragments) help us most (*ibid*, 131-2, using Williams on pp 124-30). There are six of Dressel Form 1B (Cam 181) which is an Italian wine amphora, the production of which ceased just after c 10 BC, but begun considerably earlier (Sealey 1985; Fitzpatrick 1985), and there are thirteen (or possibly eleven) of Dressel Form 2-4 (Cam, 182-3). These were also of Italian origin (west Mediterranean sorts seem absent) and were mainly for wine production. They first occur c 30 or even c 50, but probably in no great quantity until c 15 BC, when production of Dressel Form 1B was dying away (German evidence: Fitzpatrick 1985). As for the one piece of 'Rhodian' (Cam 184) claimed by Peacock in 1971 (p 183) but no longer in his survey of 1977, nor recognised by Williams (Foster 1986, 125), discounting it leaves the burial's date to be inferred, first of all, from the Dressel Form 1B in conjunction with the Dressel Forms 2-4 (to which belong probably most of the featureless bits (*ibid*)), and secondly from the absence of any that are Spanish. These, whether for wine or oil — or most distinctively, the Dressel Forms 7-11 group (Cam 185-6) from south Spain, for fish-sauce (*garum*) — were reaching Britain in the final years of the century, and on into the next (*ibid*, 131-2).

As for the very few pieces of smaller-sized pottery, the account by Valery Rigby (*ibid*, 110-23) gives the butt-beaker figure 37,1 (p 112), which is a local copy of a Gaulish import of the last quarter of the 1st century BC, a date-range starting 15 BC (so before the 'final decade', *ibid*, 115), though continuing to AD 70, and the same for figure 37,2 which is a piece of a black-faced cordoned bowl of form Cam 218. She discounts Isobel Thompson's classification, and consequent wish to have the bowl post-AD 43 (*ibid*, 118). (Thus its fine thin-walled fabric will not be 'Romanising' as at Sheepen (Hull in *Camulodunum*, 259), any more than 'Gallo-Belgic' (Hull 1958, 253), but could very well suit its being specially made for the grave.) The date-range is again the same for her figure 37,3 which is parts of a small, very fragmented, mica-coated drinking-pot of form Cam 102, convincingly taken as an import. (Hull illustrates a vessel from Lebach in Saarland, Germany

as his type-form (*Camulodunum*, pl 56).) The seven or eight other pottery items, none of them attested as grave-goods, are either in the collection by mistake (the last three are much too late for it) or will have come from the mound above, sunk from its surface or left within it, by its builders or else by its disturbers (rather than the robbers) who planted the Wellingtonia trees in 1860.

Foster's summing-up ends with a likeliest date for the assemblage c 15-10 BC (Foster 1986, 177-8). Her caveat, that objects may be buried after long retention as heirlooms, plainly cannot apply to it all; and her start for Spanish amphoras and Gallo-Belgic tableware in Britain, c AD 5, can be too late by about ten years or so, though the absence of both from the burial will even then let her date for it stand. Elsewhere indeed (*ibid*, 132), the amphoras here present have a date of deposition around 15 BC; Peacock, whom she quotes for this, put it actually 'about the last 15 years BC (Peacock 1971, 178, 183), as she recognizes on page 175 ('c 15-0 BC'). At any rate he was first in proposing a BC dating at all, moving thus to supersede the previous notion, stemming from Laver 1927 (p 252), that the occupant, a noble, died probably in the reign of Cunobelin, so at a date AD within years that would run to c40. (For Webster (1980, 70), he was king Cunobelin himself.) Peacock's lead has been followed already by others (eg Rodwell 1976, 357) but Jennifer Foster alone has done so from the whole of the burial's evidence. Thus far, the grave's archaeology. Its occupant, approached by her (pp 187-8) from several angles, will here have his place below, in the relevant history.

## History (in texts and from coins)

[Figs 4.1-2]

Onward from autumn 54 BC when Caesar quitted Britain, never to return, down to the conquest begun in AD 43 under Claudius, events and situations in Britain have often been studied, by many, mainly from coins and especially from rulers' inscribed coins: as by Evans (1864), Rice Holmes (1907), Derek Allen (1944, 1961), Warwick Rodwell (1976), and now by Dr Colin Haselgrove (1987 and after), whose aid to me in this essay I gratefully acknowledge. The first modern study drawing on the coins and on the extant total of the relevant classical texts (not just on parts of them), was by the late C E Stevens, entitled 'Britain between the Invasions', in the volume presented to O G S Crawford (Grimes 1951). And with regard to the passages on Rome and Britain, in the Greek of the *Geography* of Strabo, Stevens observed that on Roman affairs there are almost none between 7 BC, when Strabo seems to have left Rome (his own city was East Greek), and about AD 16 when he added just a few. This had first been observed (Stevens 1951, 341, note 66) by Ettore Pais in his classic *Italia antica* (1, 303).

The passages on Rome and Britain are both well known:

Strabo, ii. 5, 8 and iv. 5, 3. The former declares that if Rome were to conquer Britain, its tribute would bring in less than in fact do the tolls, levied at Gallic ports on the British trade; a conquest would itself be expensive; and a garrison for the island would require 'at least a legion and some cavalry'. The island', for a garrison so small, can only mean the parts that Romans were aware of — Caesar's report of its size (*Bell Gall*, v. 13, 1-2) being either dismissed as false or conveniently forgotten. This applies no less to the other passage (Strabo, iv. 5, 3), where 'the whole island' has rendered 'home ground, almost' to the Romans. What Strabo has long been seen to be reflecting, in both, are official explanations — in this one, exaggerated purposely — of the policy, determined already, of the emperor Augustus: he had declined any notion of a conquest. That he had more than once intended one, before 26 BC, was believed, eg by Stevens (1951, 335-7), from some allusions by Roman poets and then from the later historian Dio, but disbelieved by Syme (1978, 48-51, 186). (See Momigliano 1950 for the texts. These references I owe to Dr Barbara Levick, St Hilda's College, Oxford.)

How then could Britain (that is, the parts of it known at the time) be in official propaganda presented as 'almost home ground' to the Romans? The same passage tells us, 'Now indeed some of the dynasts there, by embassies and courtesies gaining the friendship of Caesar Augustus, have set up offerings in the Capitol' — a ceremonial act under Public International Law (Stevens 1951, 341). That Strabo would call that 'now' in a late addition, of about AD 18 when Augustus had been dead four years (which was Frere's belief in his *Britannia* of 1967, p 44), cannot stand: the friendship had begun with Augustus alive, and the offerings were made by rulers who themselves, it is stated, had begun it. To see the 'almost home ground' situation as resulting only later, which is not what Strabo is saying, is an evasion. The idea will have sprung from doubt that such a situation, earlier than 7 BC, could be historically credible. (Even Stevens (1951, 341, note 66) felt some doubt, but only through his thinking it would have to correspond with the dates, shortly before and shortly after AD 10, of the coins of southern British kings that bear the Roman title REX: see below.) That this is not needed at all, follows from his own historical picture; and moreover he was ignoring our Lexden Tumulus because he was presumably crediting a date for it under Cunobelin (Laver 1927, 252) which is too late to be relevant. It is true that no embassies nor offerings from Britain are recorded by Augustus in his *Res Gestae*, surviving inscribed on the *Monumentum Ancyranum*. Yet that account of his life gives only what he personally wished to be remembered for. Would he wish to be remembered for a friendship with British kings if it had not endured for long, and was replaced by relations not all so secure and dependable?

The only British kings in the *Res Gestae* were the two whom he records as having come to him as suppliants (at 32, 1 = vi. 2 in the older chaptering that Stevens

used: 1951, 339, note 55). Their names in the text are Dubnovellaunus and Tin...; the obliterated space fits the rest of the name Tincommius, the king well known from his coins as ruling in the southern British kingdom, and as son of its previous ruler, the long-lived Commius (Allen 1944, 6, with notes 4 and 5 and repeated regularly in subsequent publications, and fortified by Stevens 1951, 339, note 52). On why they came as suppliants, Augustus is silent, but the reason can be only that each had been expelled. If his policy had kept them as friendly kings till then, would he wish to reveal what had thwarted it in driving them out? All that pride would allow him to record would be that it was as suppliants to himself that they both had come.

This explanation of his terseness fits the dating of the relevant coins. Tincommius's father Commius, who had figured in the *Gallic War* of Caesar, cannot have lived much after the years about 25 BC; his coins are of 'Celtic' style, and the latest were the model for the earliest coins of his sons. But next, Tincommius abruptly took to types derived from Roman ones. And Allen (1944, 7-8), after showing this first (his pl 1,5-6) on the gold, perceived that on the silver (*ibid*, 12-14), one (ie no 14) was copied directly from a dated Roman model, a Lugdunum (Lyon) issue of Augustus of c 15-12 BC. Stevens (1951, 338) then took the date 15 BC as a fixture, since the conquest of the Alpine forelands, made that year by Augustus's stepsons, was praised by the court poet Horace in two of the odes in his Book iv, published only two years later, ie Ode 4, and then Ode 14 which goes on to name Britons (line 48) among the peoples who 'admire' or who 'listen to' Augustus. Stevens inferred that while his Rhine and Danube policy was one of warfare (with a reverse in 17 BC to avenge, and pacification in northern Spain completed), he dealt with Britain by diplomacy — inducing Tincommius to seek and obtain an official Roman friendship, and letting his precious-metal coins be given models, or even some designs, that were Roman. Thus as Allen accepted a date for his death within the early years AD, he would be one of the kings whom Strabo, already before about 7 BC, recorded for just such a friendship, with the 'almost home ground' situation resulting. Even after his flight to Augustus, within those early years AD, the emperor's concern for keeping friendship with the kingdom let him stretch it to the next two kings: Eppillus, who briefly reigned at Silchester (Calleva), before departing into Kent, and Verica who then took all the kingdom. To each he granted the status of 'client king': this alone can explain, as Stevens (1951, 346) was the first to see, their coins inscribed in Latin with the title REX. Both were Tincommius's brothers; and whatever may have turned them against him, that grant—though Verica was soon to lose the title — meant approval of their profiting from his flight (Allen & Haselgrove 1979).

About his fellow-fugitive, Dubnovellaunus', all that needs to be emphasised here is what his coins will be telling us below, that he fled from a kingship in the land of the Trinovantes — won by him (at latest) not very long

1 There is the problem of whether or not there were two rulers by the same name — see footnote 2 on page 7.

after archaeology's date for the tumulus, given here above as c 15-10 BC. From his fleeing to Augustus as a suppliant, like Tincommius, we can judge that he himself had seen the value of Roman favour; the 'almost home ground' situation in Strabo thus need not have ended, in this Trinovantian kingdom, till his flight. But it was back before 7 BC that Strabo wrote of it. Stevens, for the southern kingdom, had it start in 15 BC; for the Trinovantian one, he had nothing to say before Dubnovellaunus — little guessing that twenty years from the printing of his article, Peacock would be proffering that same date for the tumulus.

This date may just be put later, though by barely five years at the most, as we have seen. So were all its classical bronzes not imported any earlier than that, nor earlier, then, than the Augustus silver medalet? May not the 'almost home ground to the Romans' situation, here at Colchester, have first been approached rather sooner? At all events those bronzes (and the iron folding stool like that of a Roman general) must together stand as indicators, stronger than any of the kind, of just such a situation. The Augustus medalet, certainly anyhow of the owner's last years, might be seen as a memento of an embassy to Rome, such as Strabo also wrote of; might this have been the climax of a relationship begun rather earlier? That between Rome and Tincommius, down south, was presented by Stevens as created at a single stroke in 15 BC. For that, he combined his texts with Allen's dating of the Romanising coins. Yet might not Augustus, using means not touching any coinage, have let a quiet preparation of the ground begin already rather sooner. The traders of Italian wine, in its amphoras of Dressel Form 1B and even some still of Form 1A, had been plying on the southern kingdom's coast quite certainly already (Peacock 1961, 172-3, 180-81; Fitzpatrick 1985, 309-310, with maps figs 3 and 4 that augment both Peacock's and also Galliou's of 1982, reproduced in fig 2). The situation anyhow concerning us now is Trinovantian; and in this regard there is more to be said about Stevens.

He showed in his first two pages (as already in Stevens 1947, 8), that the British south-east, through the surrender to Caesar by the leader of its resistance, Cassivellaunus, was regarded at Rome as conquered, although never yet officially annexed. He had also shown (Stevens 1947, 7-8) that Cassivellaunus's surrender left the new Trinovantian king entitled to a 'client-king's status (see my 1978, 160-61, 177). The new king, Mandubracius, was accepted for protection by Caesar (after his father had been killed by Cassivellaunus), and was instated as ruler by him when his tribe made friendly submission. What he could not know was the modern extent of the archaeological evidence, that of the 'Welwyn-type' cremation-graves — fully presented by Foster (1986, 178-87) following Smith 1912 and chiefly Stead 1967 — which show that from c 50-10 BC, within counties to north of the Thames, there is a cluster, mainly in Hertfordshire but away to the north-east from Verulamium, which

includes some that had Italian-made amphoras for wine, three with precious foreign vessels for serving or drinking it — the famous two at Welwyn and that at Welwyn Garden City, and one with a foreign glass bowl at Hertford Heath. All must stand as a sequel to Caesar's favours for Mandubracius; there and on to a frontier through Hertfordshire, to which his power will have now been extended (Peacock 1971, 175-6, map fig 36: some of his amphoras east towards Colchester have contexts later than the tumulus, no matter that he classed it wrongly along with the 'Welwyn'). For Partridge (1981, 353-4), the power would be held by one or more other tribes, some of them Mandubracius' neighbours, who had also submitted to Caesar (and my note in Hawkes 1980a would have readjusted his reasoning only slightly). Yet the example for their submission had been set by the Trinovantes; the lords of a Welwyn-type burial would be sharing in the fruits of their submission, and now were the forward front against the Verulamium Catuvellauni.

That being assured, we next may revert to British coins. Not now those that introduced 'Romanisation' (discussed above from Stevens), but farther back: not so far as to be prior to Caesar, so here irrelevant, but after him, ones from c 50 through the next thirty years or so<sup>2</sup>, and accordingly, north of the Thames, covering the time and the area considered in the foregoing paragraph. In the standard classification, Allen's (here as deployed in Allen 1961, 132-3), the first such group is his L, named Whaddon Chase because of its preponderance in the massive hoard from there (on the border of Buckinghamshire; *ibid*, 294, no 16); and the [north-of-Thames](#) group standing closest to that is his LX. While Rodwell (1976, 243, noting also Harding 1974) thought that L could have started somewhat earlier than had Allen, and assigned both it and LX to Trinovantes (*ibid*, 247-8), his maps show both of them tending in two directions: westward in the foregoing paragraph's area, and eastward in that around Colchester, each most firmly apparent in the map of LX (*ibid*, fig 20). And next, we come to inscribed coins. Most of the western concentration disappears, and the LX tradition is merged in the wider extent of a named king's coins, those of the Verulamium ruler Tasciovanus (Rodwell 1976, 251-61, maps figs 22-7) who was Catuvellaunian, and is generally reckoned to have reigned from 20/15 BC to c AD 10. But in what had been the northerly side of that concentration, there are find-spots for L in its later-tending form LB, and for LX in its form LX2 — a couple of each — near that of one of the inscribed gold staters Mack 266 (Mack 1953, 82). It is a type with some resemblance to LX (Allen 1961, 133), but also with enough to LB (Mack 138) to stand among the L-tradition types developed from this (Allen 1961, 109). LB occurs also in north-east Essex, the region of Colchester, and adding to Rodwell's gold map (Rodwell 1976, fig 19) that of the L-tradition uninscribed silver and bronze, north of the Thames (*ibid*, fig 20), brings the affinity between the two regions clearly out. We come now to the name inscribed on the Mack 266,

2 Current opinion places the start of British L in the decade 60-50 BC (C Haselgrove).

and on the ensuing series Mack 267-9, all with north-east Essex among their occurrences. It is Addedomaros. And it is he, reckoned from the coins to have died about 15 BC or little after, whom first Peacock (1971, 179) and then Rodwell (1976, 357) have suggested, or accepted, as the cremated personage buried in the Lexden Tumulus. Jennifer Foster (1986, 187) was unwilling to identify the occupant from funerary evidences only, although the coins are consistent with such an identification and other factors too.

The sequence of Addedomaros's coins, of L-LX-LB derivation in the inland quarter just described, and their leap in distribution, still early, to the Colchester region (Rodwell 1976, map fig 21 upper) invites acceptance. (Rodwell puts LX2 among his own early coins.) My map [never drawn] contrasts these with the later extension up the Thames, west from the Lea, with a scatter out northwards (Rodwell 1976, map fig 21 lower); it can be thought to reflect an addition to his power, along the southerly margin of the Catuvellaunian area. In that area itself, and spreading variously around, are the coins of his Catuvellaunian part-contemporary Tasciovanus (derivation again from the L-LX uninscribed). Hostility between them, inferred already and by Allen (1944, 16-17, with map) will be implicit in the two distributions — now mapped most handily in Cunliffe 1981. Over Addedomaros's westward stretch, Tasciovanus spreads, next, wiping it out: still early in his reign, which ended only cAD 10. For serial maps of all his coins, see Rodwell 1976, figures 22-7; the mint-signed are almost wholly from Verulamium. We shall be coming to those with another mint-signature soon; meanwhile, our concern must be still with Addedomaros, for his early descent to the Colchester region and the sea-coast. There are some uninscribed coins of the L family there, as we have noticed, but his are the first inscribed ones, gold staters of the three successive types we now can see on Figure 4.1: Mack 266 (Mack 1953, 81-2), with horse on reverse above a wheel; Mack 267, with horse on reverse above what is taken as a cornucopia (the three pellets on its top standing possibly for fruit in it); and Mack 268, with horse on reverse above a palm-branch, the latter being a symbol of victory for the Romans. Though modelled crudely here, the cornucopia (horn of abundance) is a classical concept too. The quarter-stater Mack 269, also Mack 270-271 which like the silver Mack 272-272a and the bronze Mack 273-274<sup>3</sup> bear no name but resemble the staters variously otherwise, can bring the total of issues up to ten, so that the reign can have been anyhow moderately long (some 12 or even 15 years?), most of it over a region running east to the tidal Colne and the coast. Crossings from Belgic Gaul could there be direct, whereas to Welwyn they would have to go round by the Thames and up the Lea. That route will have brought the Italian wine-amphoras there, but the crossing to the Colne could bring them here, starting with the same form Dressel 1B, later but at least by

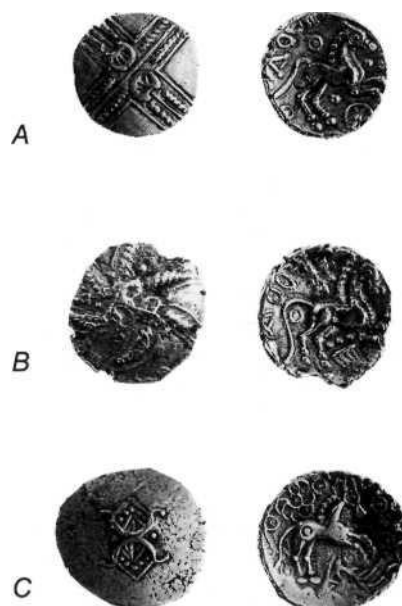


Fig 4.1 Inscribed gold staters of Addedomaros: A... Mack 266; B... Mack 267; C... Mack 268. 1:1. The photographs were provided by the British Museum.

20 BC, even possibly 25. (See page 76 above, for Colchester sites, including Sheepen; for its fragments, see Fitzpatrick 1986, 36, further to his 1985a, 332, and to Sealey 1985a, 98-9, and 1985b, 125 ff; for the tumulus also, see page 88 below.)

This trade can then be added to the hints of Roman influence, just noticed, on the second- and third-type staters of Addedomaros. His descent to these parts will have opened relations with the empire of Augustus, political as well as the commercial attested in the second of the Strabo passages. The Trinovantes, long in Roman favour inland, were more accessible for it here; and Augustus himself was in Gaul in 27, with Britain also, then and still in 26, on his mind. The account of this by Stevens (1951, 335-7), noticed briefly on page 88 of my foregoing section, presented Augustus as really intending, several times, expeditions into Britain, not merely letting poets or others believe it on rumours. In Sir Ronald Syme's belief, quoted also there, Augustus did not intend to invade Britain, and the credulity was later mistaken, by the time of the historian Dio (active c AD 190-230), for credence of fact. Yet Stevens himself distinguished, from expeditions for conquest in arms, what he called just 'walking in' to get peaceful obedience. And Dio, for 27 BC having Britons appearing likely to come to terms (liii. 22, 5), yet for 26 having them refusing to (liii. 25, 2), lets one wonder whether his Britons were the same both times. Whether or not, he states that for Augustus to punish the refusal was made impossible by a revolt of the Salassi (on the Great St Bernard Pass through the Alps), and by

<sup>3</sup> There is no conclusive evidence that Mack 273-274 belong with Addedomaros' gold. The focus of their distribution is really Bedfordshire and NW Hertfordshire (C Haselgrove).

hostilities calling for the conquest of north-west Spain. In 27, it was Gaul's being 'still unsettled' that kept him off Britain, but might he otherwise have tried a 'walk in' there?

If the Britons seeming likely to be peaceful were Trinovantes, their ruler then might have already been Addedomaros. The little one can credit from Dio as fact leaves any such notions only guesses; but in less than twenty years has come Strabo's 'almost home ground' situation, the phrase which is exaggerated but which carries the 'embassies and courtesies' and the offerings at Rome, on the Capitol, made by 'certain British dynasts'. The case of Addedomaros as one of them, and even as the earliest involved, brings those coin-designs of his, and those Italian wine-amphoras, into the far more striking context of the riches in the tumulus, where the foreign pieces have among them the medalet cast with Augustus's head. Its being Addedomaros's tomb, as having amphoras to suit but none of the South Spanish that one would expect if it were later, may thus have circumstantial support from the historical side. As for guessing it not to be a king's, though we have no comparable likelier tomb for any king, that guess has never faced the evidence of Strabo and the date that requires it to be early, nor combined with it both the uniquely-rich personal gear bestowed in the tomb, the imported classical bronzes and the iron folding stool evoked by Foster, of distinctive Roman type and implying high rank. Given the date, 15 BC or barely after, and adding the medalet, can one fancy anyone lower in rank, or later in date, than one of the 'dynasts' in Strabo? So here is my case for the tomb as that of one of them; it should strengthen, I think, the belief that he was Addedomaros.

For our not having other such tombs of the period, there is another reason besides our failure to find any: two kings ended their reigns still alive, by fleeing from Britain to seek the protection of Augustus. One was Tincommius, the king in the south whose enjoying Roman favour we have seen from those of his coins which Stevens could date 15 BC (p 89); to the other, Dubnovellaunus, I shall soon be returning. But if any king dying in Britain had his tomb ever found, the one of most interest here would be Tasciovanus — noted at the start of the foregoing section as contemporary in part with Addedomaros and with coins of derivation like his, but a Catuvellaunian. For sons of his own son (p 173) are recorded as being that (Dio, lx. 20, 2); and his coins, abundant in all three metals (Mack 149-196), have frequently the mint-signature of Verulamium (VER, occasionally VERL, VERO, or (on the bronze Mack 172) VERLAMIO), in Catuvellaunian territory as noted already, and his Hertfordshire capital. From there (adjoining St Albans) the coins spread widely, in one direction seemingly annulling the westward stretch of the middle and later issues of Addedomaros. Hostility between them, quoted on other grounds already (p 91), will have

renewed that of Caesar's time, between his enemy Cassivellaunus and his friends the Trinovantes. But Tasciovanus reigned far longer than Addedomaros, and died at an estimated date c AD 10. And his inferred over-running of his rival's stretch westward came certainly early in his reign. Early again in his reign came also what concerns us here: he had another mint-signature, on gold coins of his, far rarer. It has long been known, and it stands for Camulodunum. So this Catuvellaunian ruler, for a time seeming not very long, had over-run Trinovantian territory too: Colchester country — reacting, surely, to Addedomaros's death. Yet the tumulus shows us a death that was mourned by elaborate funeral rites, completed then by the building of its mound, unhindered. Tasciovanus's Camulodunum coins, nonetheless, are of types so early as to show him here very soon after.

The types are the stater Mack 186, and the quarter-stater 187. Of Mack 186, the coins with locations attested are at present only two.<sup>4</sup> One is from Leyton (Fig 4.2.A), where south-west Essex is now in Greater London (Mack 1953, 66; Allen 1961, 221; British Museum). Its obverse design is like that of Mack 149, which is seen to be early while the reverse is inscribed with TASCIOVAN and the mint-signature CAMV. The other (Fig 4.2.B), on both faces similar to the first coin but with the CAMV off the flan (*British Museum Cat*, 239), is one of the eleven staters found in 1827 in a hollow flint at High Wycombe, Buckinghamshire (*Arch*, 22, 297; Evans 1864, 239, pl 5,7; Mack 1951, 66; Allen 1961, 220). The other ten coins are mostly Mack 149-150, with two or three of Mack 154-7, which are very similar and are again early. The stater taken by Allen as Mack 186 (1961, 223) from the Cleeve hoard (Oxfordshire, Goring-on-Thames), found c 1903 (*VCH Oxon*, 1 (1939), 264; Ashmolean Museum), is here discounted on account of Dr Haselgrove's doubts.<sup>5</sup> To be added with the certainly-attested two coins is the quarter-stater Mack 187 (Fig 4.2.C; Mack 1953, 66; Allen 1961, 223; *British Museum Cat*, 268), given in 1931 by a Mrs Tupper, from the Roman and older site (with temple, etc and other British coins), near Albury in Surrey, on Farley Heath which was explored c 1848 by Martin Tupper. Its obverse is very like Mack 186, and its reverse has the CAMV, though with king's name missing.

These three located coins have been supplemented lately by a new type, of Tasciovanus at Camulodunum, having the signature CAMLV/TASCI, with design of a standing goat. One example (Fig 4.2,D) was found in 1980 at Great Canfield (Eddy & Priddy 1982, 35 & 37), and another (Fig 4.2,E) was recognised lately by Haselgrove amongst the coins from Baldock, Hertfordshire (no 18 in Goodburn 1986, 93; not identified there). Thus Tasciovanus was at Camulodunum for long enough to issue two types of stater, one with a quarter-stater. The distribution of these five coins is shown in Figure 4.2, and the question

4 An example has since been found at Hemel Hemstead (*Brit Numis Jour*, 58, 44) and a possible variant was discovered in Reading in 1994 (Celtic coin index, Institute of Archaeology, Oxford). Information from C Haselgrove and Philip de Jersey resp.

5 Mack 149 (C Haselgrove).

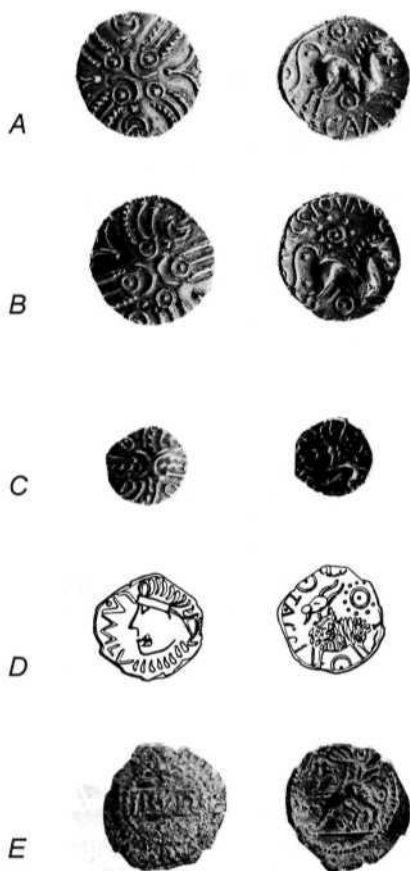
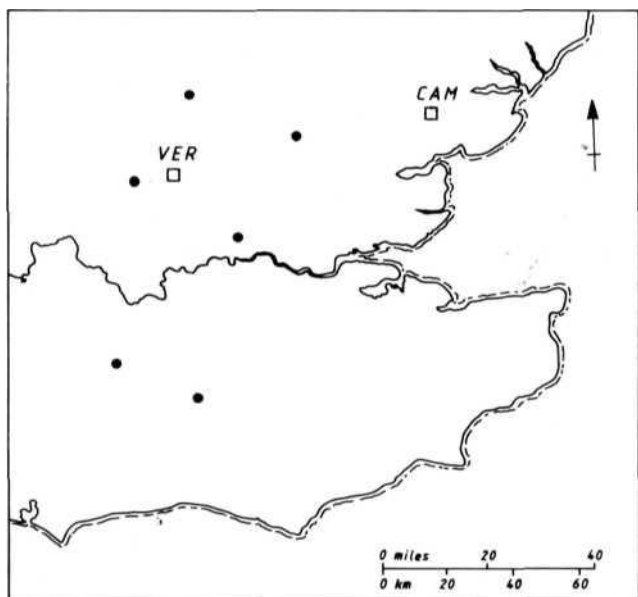


Fig 4.2 Coins of Tasciovanus at Camulodunum:  
 A... Leyton, Greater London (Mack 186); B... High Wycombe, Bucks (Mack 186); C... Farley Heath (near Albury, Surrey) (Mack 187); D... Great Canfield, Essex E... Baldock, Herts. 1:1.  
 The distribution map includes a coin from Hemel Hempstead, Herts (Mack 186) — see footnote 4 on page 92. The illustration of coin D is reproduced by courtesy of the Essex County Council and the Essex Society for Archaeology and History. The photographs were provided by the British Museum.

remains why none were any nearer to Colchester. I believe the explanation to be this: that the next king there 'called in', for melting down and re-minting, all of those Camulodunum coins that he could get at. The Trinovantes will have welcomed this treatment of the coins of an intruder.

Allen already in his 1944 (p 15), from the two of them then known to him, inferred that Colchester had fallen into Tasciovanus' hands early in the reign of Verulamium, and in 1962 (Allen 1962, table facing page 25) he gave 15 and 10 BC as the dates between which it had started there. Wheeler (1936, 7, 9) had earlier given the starting estimate of c 20 BC or 15, which was perhaps on the liberal side. However, for 15 as a maximum, Allen's reasons demand acceptance. So too may his 10 BC, and no later, for the Camulodunum issues — letting thus his intrusion here last for some three or four years. Was it he, then, who ordered the robbing of the Lexden Tumulus?

Accepting it as Addedomaros' tomb will make this a most likely possibility, and the robbing would then be further revenge for that thrust along the Thames, which Tasciovanus countered and over-ran, as guessed above (p 91). There was anyhow the older hostility, going back to Cassivellaunus's killing the Trinovantian king, father of the Mandubracius instated by Caesar. And a further point here, which Stevens (1951, 337) pressed, is that Caesar, in his dealing with Cassivellaunus, charged him never to molest the Trinovantes again (*Be// Gall*, v. 22, 5); and the charge, in the eyes of Augustus, would have been binding on any successors. Tasciovanus at Camulodunum was openly flouting it; Stevens dated his intrusion there a little too early, and his fancied reason for its briefness, an imperial protest (Stevens 1951, 338), was clearly inadequate anyway; the most realistic one, however, and surely the most satisfactory to Augustus, is his expulsion from the place through its capture by a British successor. Who then could this be but the Dubnovellaunus here noticed already (p 89): first at the end of his reign, when he fled, together (or at least in the same year) with the southern king Tincommius; second for his coins, datable from soon after c 10 BC among the Trinovantes; and third as calling in Tasciovanus Camulodunum coins (or all he could get of them), if it was he who was the next king here — which his own coins allow — and thus with the strongest claim to have expelled that Catuvellaunian intruder. For no other name but his can be advanced, as king among the Trinovantes, thence on till he in turn was expelled (no other cause will do) to flee to Augustus. His coins moreover, staters and quarters (Mack 275, 276), and one of the bronze (Mack 277), have features taken from some on Addedomaros' later ones, such as double-crescent obverse and reverse with horse and palm-branch, as in his Mack 268 (p 91). In this way he respected the memory of the man whose tomb the intruder may have had robbed, and he resumed Trinovantian tradition. The relations with Rome, inferred already in Addedomaros' reign, and recorded by Strabo about this time, have additional pottery,



Roman-provincial now, for reflecting them commercially (p 72, at Sheepen, and elsewhere).

But first it must be noticed that Dubnovellaunus, when he entered into Essex, had different coins (Mack 282-291), starting earlier in eastern Kent, where he thus had a prior kingdom. But if thus he entered Essex not as a native, there are coins in western Kent showing that Tasciovanus had intruded there as well as at Camulodunum, where Dubnovellaunus (invited perhaps, and then aided no doubt, by patriot Trinovantes) can have arrived with Kentish grounds already for his enmity. Anyhow his flight put an end to his kingdoms in both; and, briefly, we ought to consider the date of this event.

For three years directly after give us possible occasions, less likely indeed that the Tasciovanus intrusion, for a robbing of the tumulus. The record of the flight in Augustus' *Res Gestae* (p 89) is of course what the dating depends on. In the standard edition by Brunt and Moore (1967, 6, with note 1), the *Res Gestae* had its principal draft in 2 BC, and had been completed by April AD 13 (Suetonius' *Life of Augustus*, 101) — apart from two additions by Tiberius, his successor, made closely after Augustus' death in AD 14. Disappointingly it allows no dates for events between 2 BC and 14 AD, though some can be fixed from other sources. This indeed improves on the older view (as still in Allen 1944, 7, 23) that none could be fixed more closely, but negates the subsequent belief in a recension of the work, and in a date for this of AD 7 (as in Stevens 1951, 339), which would thus be the latest for the flight of both Tincommius and Dubnovellaunus (so eg Webster 1978, 42; Todd 1981, 47, 52). Nonetheless, among the events fixed otherwise are three of AD 4, and two, perhaps three again, of AD 6, so that Augustus most probably in each of these cases, would have made their several entries all at one time — an inference I owe to the same Dr Barbara Levick as acknowledged for advice to me already on page 89. And so, as she has further proposed to me by letter, he could at the same time have entered any other event of the year, though happening not to be amongst our definitely fixed ones. Thus a year that has two or more fixed could be thought more likely than a year that has none, for such an event as the arrival of the suppliant kings. The coins put it anyhow close to those years, to Allen's AD 5 or directly after; and if conjecture from the *Res Gestae* be allowed on the lines suggested, it may best suit a preference (seeming possible to Dr Levick) for assigning the flight of these kings to AD 6.

Their coming as suppliants of course was not an 'embassy' (rightly remarked by Brunt and Moore 1967, 74) such as Strabo's British 'dynasts' made to Augustus. But they must have been two of those 'dynasts' while they were reigning. Collingwood (1936, 73), although scolded for it by Stevens (1951, 341), declared this as 'evidently' so, and he was certainly right. Strabo's 'almost home ground' situation thus had lasted through

both their reigns — through Tincommius' (or at least from early in it) and on into his youngest brother's (Verica, p 89). Then among the Trinovantes, what happened when Dubnovellaunus, expelled as he must have been, had fled? And who expelled him? Is the hand of Tasciovanus to be seen in these events, in an act of revenge on the man who had driven him from Camulodunum? And rather than venture back there, did he had have the deed done for him by someone else?

We are very near Cunobelin, the greatest of Camulodunum's rulers. He was the only one apart from Tasciovanus, whose rule was so brief, to make its name the mint signature of coins. He declares himself a son of Tasciovanus on some which are distributed mainly in his father's old country, where his succession followed closely his accession in Essex. Of the long series Mack 201 to 261, these are the silver Mack 235-241 and the bronze Mack 242-249, all with Tasciovanus' name as well as his, with the definitive F for *Filius*, 'son', on Mack 235, 242 and 245. (For the gold, Mack 201-234, see Allen 1975.)

Dates for the Essex accession have been c AD 5 or c AD 10, either before, or at, the death of Tasciovanus. If before, the son avenges the father's expulsion from Camulodunum by Dubnovellaunus, the vengeance being now the latter's own expulsion, which the suggestion of AD 6 for it would perfectly suit. But if Cunobelin acceded c AD 10, his father's death would leave Verulamium to the Segos...(?Segonax), whose gold staters are inscribed with his name as another son, and Camulodunum to be captured by Cunobelin at that same time. (On these alternatives as Allen originally posed them, see Allen 1944, 18 and 22.) If the first alternative is right, Cunobelin's enmity to Dubnovellaunus could, as one might suppose, have served to keep alive what we here have ascribed (p 91) to Tasciovanus: enmity to Addedomaros. A final kick of it would then be a robbing of his tomb. Yet Cunobelin's success must imply conciliation of his Trinovantian subjects; and some details on his early coins reflect his doing so, ie Addedomaros' and Dubnovellaunus' palm-branch used again, and placed higher up (Mack 202-7, 209-211), while only on Mack 202 does the horse resemble that of his father's on the Camulodunum Mack 187, rather than theirs. So we may turn to the second alternative, with Dubnovellaunus' flight being still before AD 10. Then either whoever expelled him could order the robbery, or else there was a state of confusion in the years between, advantage of which could be taken for robbing the tomb by unauthorised hands. But its robbing by Tasciovanus' order, while he occupied Camulodunum, still seems to me the likeliest guess.

This has been an essay on the Lexden Tumulus, starting from its new interpretation by Jennifer Foster. And here I must end my endeavour to see its archaeology in terms of history.



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## 5: GOSBECKS

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### Introduction

[Fig 5.1]

The principal man-made features at Gosbecks (Fig 5.1) can be summarised as follows:

- i) a large multi-period enclosure, originally of trapezoidal shape, with faint traces of occupation in the west corner
- ii) a complex system of trackways and other enclosures
- iii) a series of defensive dykes to the west
- iv) a Roman fort against the rear of Heath Farm Dyke
- v) a portico, temple, and enclosure ditch
- vi) a theatre within a large enclosure, and
- vii) a road leading from Gosbecks to Colchester with subsidiary enclosures on either side at its southern end.

The oldest part of the Gosbecks complex was a large native farmstead, the main buildings of which were enclosed within the large trapezoidal enclosure. Leading from this and developing over half a century or more was a complex multi-period system of trackways, fields and dykes, all focused on the main enclosure. To judge by the areas detectable as cropmarks (Fig 5.1), the field system extended westwards for about 1500 acres (600 hectares) or more of the fertile land bordering the northern edge of the valley of the Roman River.

The various features of the Gosbecks site and the surrounding area will be considered separately below, and a general discussion of the site in the context of Camulodunum as a whole is to be found in Chapter 7.

### Previous excavations and surveys

[Fig 5.1]

The first known excavation at Gosbecks took place in 1842 when the Reverend Henry Jenkins uncovered parts of the portico and temple which he mistakenly took to be the remains of a Roman villa (Jenkins 1846; Roach Smith 1852, 41-2; *Rom Col*, 260-63). In 1933, at the instigation of Mr M R Hull and through the efforts of Mr O G S Crawford, the Royal Air Force flew over the Gosbecks area and took the first aerial photographs for archaeological purposes. These enabled M R Hull to perceive the true nature of Jenkins's 'villa'. This consisted of a Romano-Celtic temple in the south-east

corner of a square enclosure demarcated by a deep ditch enclosed on all four sides by a double portico. The latter lay within a pair of large walled enclosures. Just after the Second World War, following the discovery of the magnificent copper-alloy statuette of Mercury (*Rom Col*, 264), a spate of small excavations was carried out at the site, the most outstanding result of which was the identification of the theatre (*Rom Col*, 259,261,264-70; *CMR* 1949-50, 16-17). In 1958, the first plan appeared showing the temple complex, theatre and cropmarks as revealed in 1932 by the Royal Air Force and subsequently by Professor J K St Joseph (*Rom Col*, 260, fig 113). In 1967, the theatre was partially excavated by Miss B R K Dunnett (1971), and in 1977 small areas of the theatre and portico were examined to assess plough damage (Crummy & Smith 1977).

The dry summers of the mid 1970s and a much increased level of aerial survey by a number of fliers made it possible to plot the cropmarks in considerably greater detail and over a much wider area than M R Hull had been able to manage. Four successive plans have appeared in print, each being an improved version of the previous one (Crummy 1974, 4; *Britannia*, 6 (1975), fig 11, facing p 259 and repeated in Crummy 1975, 13; Crummy 1977, 88). The latest plan (Fig 5.1; Crummy 1980a) includes revisions up to 1994.

The cropmarks at Gosbecks have been plotted from a collection of over 700 photographs. The collection has been assembled by the Colchester Archaeological Trust using photographs of the following: John Hampton of the National Monuments Record, Professor J K St Joseph and Dr D R Wilson of the Committee for Aerial Photography of the University of Cambridge, Mrs I McMaster, Mr R H Farrands, Mr F Erith, and the Trust. The latest plan of the cropmarks is based mainly on photographs by the National Monuments Record.

The most striking aerial discovery of the mid 1970s was a Roman fort built up against the innermost dyke at Gosbecks. This was found independently by Dr Wilson and Mr Hampton (Wilson 1977; St Joseph 1977, fig 2; Crummy 1977, 87-9).

### Earliest activities

[Fig 5.1]

For reasons given below, most of the cropmarks in the Gosbecks area are assumed to indicate broadly



Fig 5.1 Plan of Gosbecks, based mainly on cropmarks. [Pages 95-105]

contemporary features which are Late Iron Age to Roman in date. There are however some ring-ditches to the south-east of which two are double (Fig 5.1). These probably represent Bronze Age barrows, although there is the possibility that one or two of the single ring-ditches belonged to round-houses in which case a Bronze Age or Iron Age date would apply. No doubt some of the other cropmarks are also of earlier origin but these are not sufficiently distinctive in plan to be identifiable as such.

Finds from Gosbecks earlier than the Late Iron Age are very limited. The most significant are sherds of late Neolithic grooved ware which were in a pit excavated in 1994 on a redevelopment site at the north end of the site (Benfield & Brooks 1994). Other finds consist of a dozen sherds of ?Middle Iron Age date found during the excavation of the theatre in 1967 (Dunnett 1971b, 29 and 44-5), one sherd of similar period discovered during the excavation of part of the portico in 1977 (Crummy & Smith 1977), and one sherd of Late Bronze Age or Early Iron Age ?flint-gritted pottery found in 1949 in the ditch of the main trapezoidal enclosure ditch (*Rom Col*, 271 and below). There is considerable evidence from the Colchester area of a general scatter of activity dating from Mesolithic times onwards (see Chapter 6), and it is against this background that the earliest Gosbecks material should be viewed.

## The farmstead

[Figs 5.1-5.3]

The heart and earliest part of the Gosbecks complex was a large native farmstead which had been remodelled several times (Figs 5.1-5.2). In its original form it consisted of a large trapezoidal enclosure protected by a substantial defensive ditch. Later (presumably Roman) periods of alterations are represented by straight stretches of narrow ditch forming a series of secondary enclosures and internal subdivisions. In the later periods the perimeter ditch was of much smaller proportions than before, indicating that it was no longer defensive. It enclosed a bigger area. Traces of occupation in the form of pits and a possible hut are visible as cropmarks in the western corner of the trapezoidal enclosure. The system of trackways which surrounded the enclosure focused on what was presumably an entrance at the west corner.

The identification of the enclosures as a multi-period, native farmstead rests on the similarities of the complex with excavated farmsteads elsewhere, the presence of the adjacent field and trackway systems, and the indications of occupation within the enclosure.

Farmsteads represented by sub-rectangular or sub-square enclosures are a feature of the latter part of the Iron Age and the Roman period. Enclosed farmsteads

originating in the Late Iron Age are often distinguishable from their Romano-British counterparts by a less regular appearance, and sometimes by a more substantial enclosure ditch up to 2.5 m (8.2 ft) deep. This was first noted in respect of Late Iron Age and Roman enclosures in the Thames valley by Riley (1944, 80-81) and subsequently in *A Matter of Time*, 12-15 (RCHM 1960). The same distinction has been made of enclosures in the Trent and Tame area although increased complexity rather than greater regularity is regarded as a diagnostic feature (Smith 1977, 58).

The trapezoidal enclosure at Gosbecks was clearly a defensive work and was an exceptionally large example of its type, being for example about four times the area of the Orsett farmstead (Carter forthcoming) and twice as large as the one at Stansted (Brooks forthcoming) (Fig 5.2). Both are Essex sites.

The key dating evidence for the farmstead at Gosbecks, such as it is, comes from two small trenches dug by Lt Col R J Appleby under the supervision of Mr M R Hull in 1949. Although at the time the nature of the earth-works was not understood, the results of the work are nevertheless very valuable. One trench (21 ft long) was dug across the northern ditch of the trapezoidal enclosure and the other (15 ft long) across a smaller ditch about 130 yds to the north. (The exact positions of the trenches are indicated by the letters 'E' and 'F' respectively in fig 113 in *Rom Col*, p 260. They are repeated here on Fig 5.2.) An account of the work appears in *Roman Colchester* (pp 270-71) and the sections, hitherto unpublished, are included here in a redrawn form but as originally annotated (Fig 5.3). In his report, Hull concluded that 'these ditches are pre-Roman in origin, but remained open late enough for Roman wares to get into them. The large ditch was the earlier and was, it seems, filled in before the smaller' (*Rom Col*, 271).

The ditch of the trapezoidal enclosure (Fig 5.2) was found to be 5.5 m (18 ft) wide and 2.5 m (8 ft) deep measured from the modern ground level. At the bottom of the ditch was 0.4 m (1.5 ft) of 'rapid silt' which contained no finds. The fill above, separated from the rapid silt by a spit of yellow sand, was of 'darkish earth' and contained mainly Late Iron Age pottery of Sheepen types. Near the bottom of the main fill, a few sherds were discovered which were described as possibly Roman. Above these, at a depth of five feet, was a Langton Down brooch (CM 151.49, Hull's Corpus no 323) datable from perhaps as early as the mid 1st century BC to 60-70 AD (M R Hull's unpublished corpus of Roman brooches). Higher up in the fill, about half-way up, were some Gallo-Belgic sherds and a few fragments of South Gaulish *terra sigillata*. From this, there is little doubt that the ditch was silting up in the Late Iron Age and Roman periods and, assuming the 'uniform darkish earth' to be backfill, was levelled some time after the conquest. However, what is not clear is the date when the trapezoidal enclosure, and therefore presumably the farmstead, was set out.

The smaller ditch to the north was found to be 3.6 m (12 ft) across and 1.4 m (4.5 ft) deep below the modern surface (Fig 5.3). It appears to form the north boundary

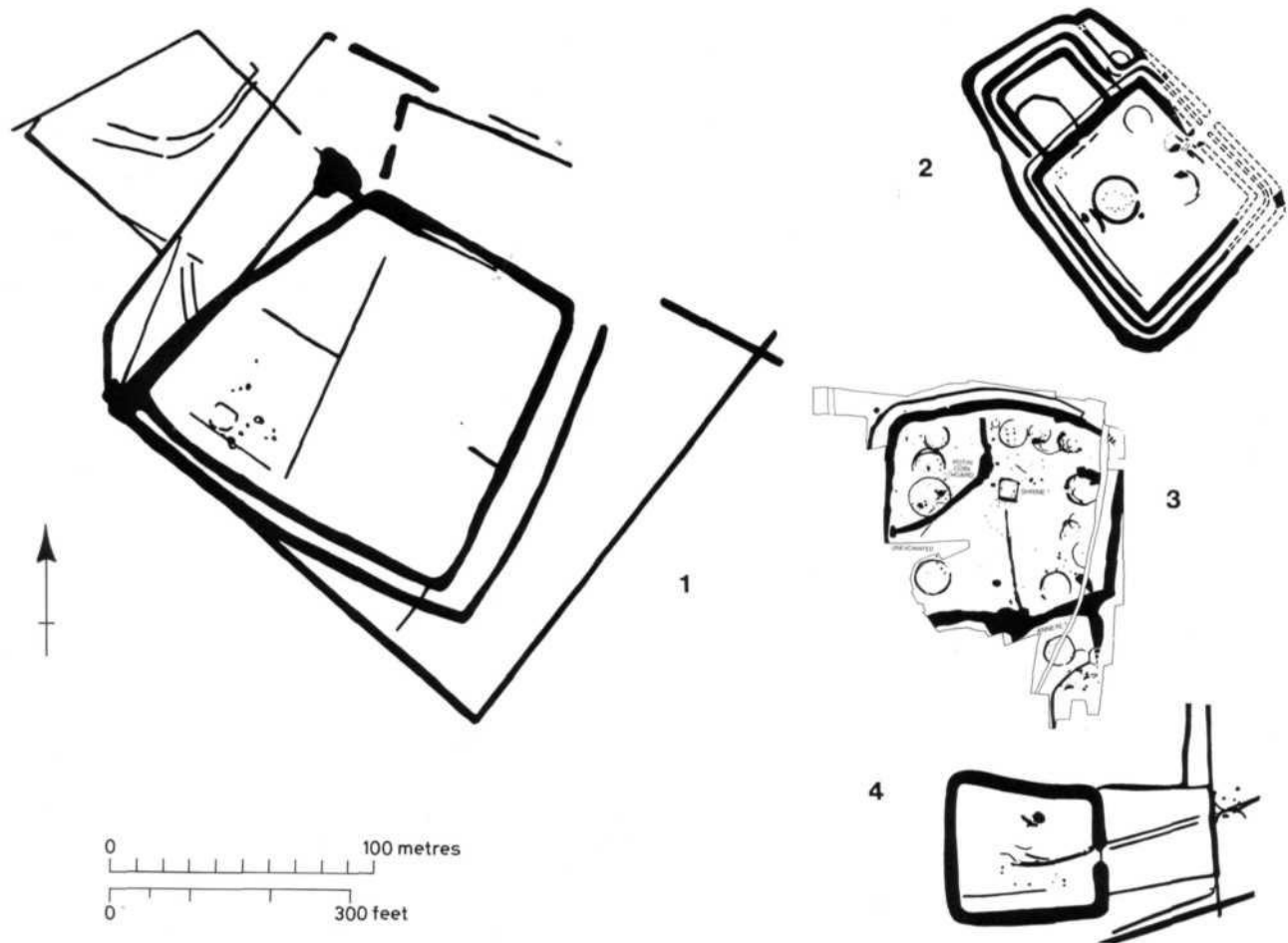


Fig 5.2 Examples of Late Iron Age/Roman farmsteads in Essex: 1... Gosbecks; 2... Orsett (Carter forthcoming); 3... Stansted (Havis, Brooks, & Walls forthcoming); 4... Gun Hill (Drury & Rodwell 1973). The Stansted plan is reproduced by courtesy of the Essex County Council and the Essex Society for Archaeology and History. [Pages 97-8]

of a small enclosure abutting the north ditch of the trapezoidal enclosure. It contained no 'rapid silt' and the fill was 'dark sandy loam' which, according to Hull, changed gradually to 'brown earth' higher up the fill. There was a distinct old land surface, above which until recent times the ditch had been visible as a slight earthwork. Compared with the ditch of the trapezoidal enclosure, the lower fill contained proportionally more Roman than native pottery and included pieces of mortaria and flagon indicating a post-conquest date. Like the other sectioned ditch, there is no clear evidence to show its date of construction, but the fact that the earthwork was not levelled in antiquity coupled with its relatively high proportion of Roman pottery suggests that the feature was late in the sequence of ditches.

## Dykes

The farmstead and the adjacent trackway and field system at Gosbecks were protected on the west side

by a series of dykes. These, described in Chapter 2 above, are Gosbecks Dyke South and North (p 26), Heath Farm Dyke South and Middle (pp 29-32), Kidman's Dyke South and North (p 33), and Gryme's Dyke South (p 29).

## The system of trackways and fields

This was not only extensive but also had a complex development. The trackways near the farmstead were secondary and were laid across a pre-existing field system since several long east-west boundaries are cut by the trackways.

The relationships between the innermost dykes and the adjacent east-west field boundaries to the east require comment. North of the stream they stop 10 m or so short of the Heath Farm Dyke Middle — more or less along the inner edge of the site of the rampart. Two explanations are possible: either the fields could have

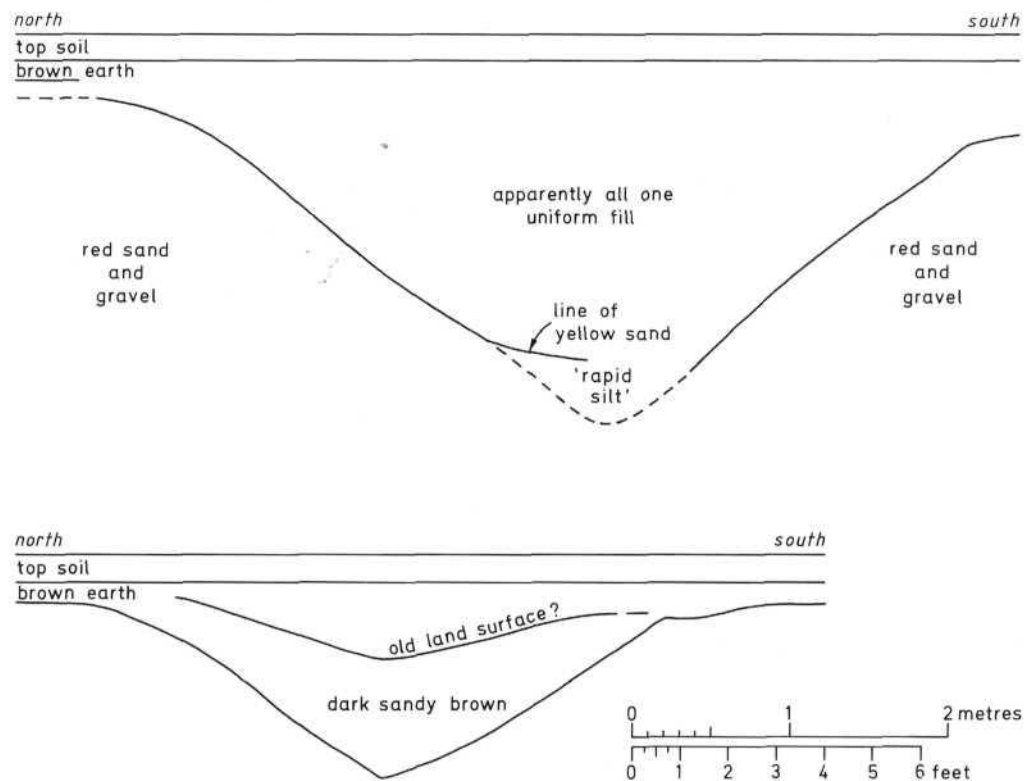


Fig 5.3 Sections dug by M Ft Hull in 1946. Above: the ditch of the trapezoidal enclosure. Below: the smaller ditch to the north. [Pages 97-8]

been laid out after Heath Farm Dyke Middle was constructed, so that the rampart provided the west boundary, or the fields could have predated the dyke, and enough of the base of the rampart still survives to render the underlying ditches undetectable as cropmarks.

South of the stream the situation is different since the ends of the ditches appear to be cut by Heath Farm Dyke South and stop against the inner edge of the rampart of Kidman's Dyke South. (The rampart of the latter seems much smaller, presumably reflecting a smaller ditch.) This appears to contradict the evidence from elsewhere, away from Gosbecks, namely that Kidman's Dyke is later than Heath Farm Dyke (p 33). At least two explanations are possible. That the field system south of the stream predated all the dykes is one possibility. Another is that what we call Kidman's Dyke South was in fact Heath Farm Dyke South, and that what we call Heath Farm Dyke South was a minor secondary enhancement which may have been intended to match Kidman's Dyke Middle north of the stream.

However in the main, the trackways give the impression of being later than the dykes, since the latter seem to dictate the positions of the former rather than *vice versa*. In places, the trackways tend to squeeze past the lines of the dykes where these stop near the stream; certainly the trackway along the western side of Gosbecks Dyke North was laid out in a manner indicative of a pre-existing dyke there. Where the

trackways join, they form very distinctive wide and gently-curved junctions and at the stream funnel together to cross at a ford or bridge.

If the line of the east-west Roman road through Easthorpe to the west is projected eastwards, it would meet the trackway system at its junction on the south side of the ford (see Fig 5.1). No convincing cropmarks have yet been observed to confirm that this was the case.

An outer part of the field system was excavated in 1973-6 just south of Dugard Avenue (pp 116-119).

## Roman fort for an auxiliary cohort

[Fig 5.4]

The plan of the fort (Fig 5.4) is based on the near-vertical photographs taken for the National Monuments Record in 1976; the quality of these is such that even extremely faint and fine cropmarks can be detected. Oblique photographs taken around the same time by Dr D R Wilson of Cambridge University (Wilson 1977, pl 13) have allowed clarification of some of the detail. As many as possible of the cropmarks within the fort have been illustrated here although inevitably the level of error and uncertainty rises markedly in proportion to their faintness.

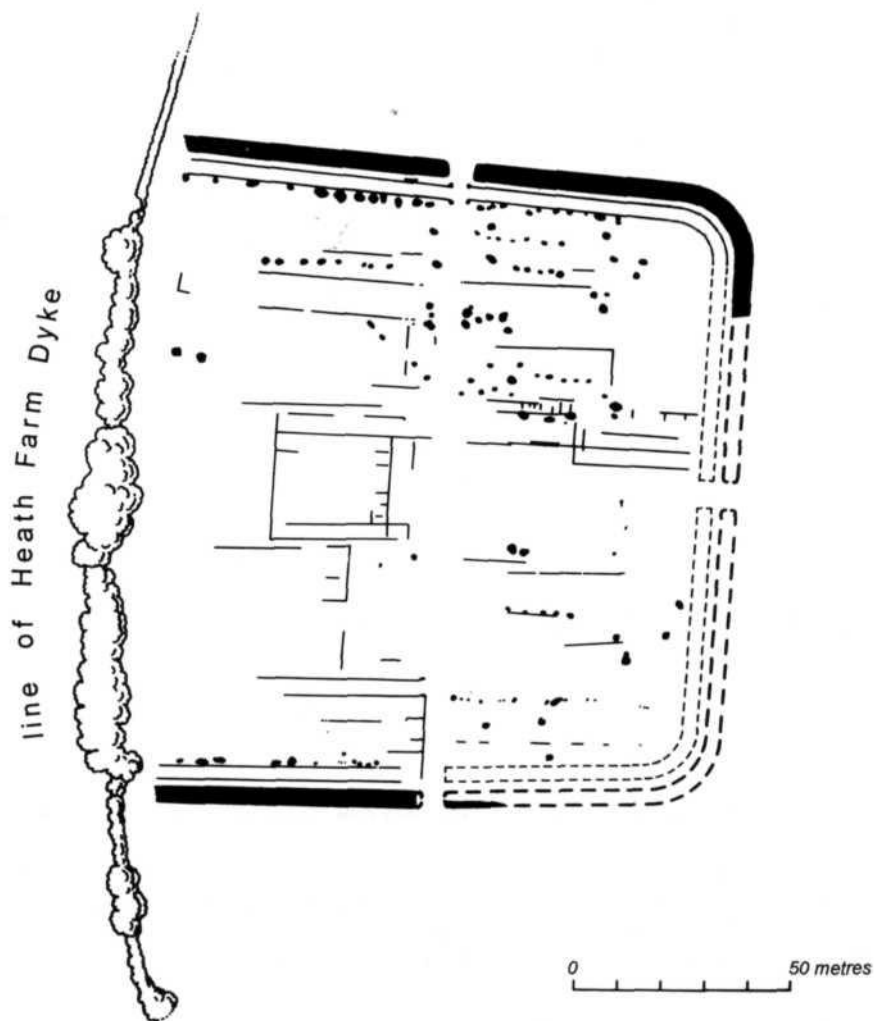


Fig 5.4 The Roman fort at Gosbecks. 1:1750. [Pages 99-100]

On the north, south and east sides, the defences of the fort consist of a single ditch and a rampart. The latter had a double timber revetment, the construction trenches of which are clearly visible. On the west side, the fort utilised the Iron Age defences (as in the well-known case of Hod Hill: Richmond 1968, 66 and plan fig 62) which consisted here of two parallel dykes, ie Heath Farm Dyke Middle and Kidman's Dyke Middle. That the fort's own bank and ditch did not exist along the west side may be inferred from the absence of cropmarks in this part of the fort.

In Dr Wilson's publication of the discovery, he stated that although he felt that the existing earthwork had been used to provide the western defences of the fort, the northern and southern military defences were nevertheless curved as if they had continued in an orthodox manner to form a west side. He shows just such a curve on the north-west angle in his plan (Wilson 1977, fig 2). Under the circumstances, such an

arrangement would be surprising (cf Hod Hill) and no such angles are indicated on our plotting of the cropmarks. This is done, not because we feel that these do not necessarily exist, but because in our view the cropmarks are not quite clear enough in this part of the fort to support such an interpretation.

Of the entrances, only the northern one shows clearly. Here the butt-ends of the ditch are distinct, and so too are four large post holes or post-pits which form the ends of the double revetment of the rampart and were part of the north gate. Of the opposite gate, only its western side is distinguishable with any degree of clarity. Nothing can be seen of the east gate (*porta praetoria*) because of the poor quality of the cropmarks in this part of the fort.

The buildings inside were aligned east-west and were built in the common military fashion, with the use of construction trenches of which some are visible in the photographs as cropmarks. The *principia* stands out the

most clearly and seems to measure about 29 by 42 m (128 by 138 ft) or more. Many of the pits visible in the interior of the fort lie in east-west lines and seem to have been dug along the minor streets. Particularly clear are the lines of pits dug hard up against the inner faces of the northern and southern ramparts. These must have been placed along the outer edge of the *intervallum* road. The lines of pits help to distinguish buildings. Apart from the *principia*, the most distinctive are two long buildings in the NW corner separated by a line of pits. Of the major streets, only the *via principalis* is detectable with confidence. The *via praetoria* is not so clear and there is unlikely to have been a *via decumana*.

Assuming the fort to have been enclosed on only three sides by its own bank and ditch, it measured within the ramparts about 4 acres and was thus the size for a cavalry *ala* or an infantry cohort. The unit concerned may conceivably have been the *ala / Thracum* since the tombstone of Longinus, a *duplicarius* with that unit, was found many years ago in Colchester (*Rom Col*, 6).

The fort was sited to cause minimal disruption to the layout of the settlement; essentially the function of the fort was to control not to destroy. The historical context for the fort is obscure but it seems unlikely to have been contemporary with the fortress a few miles to the north. Thus three possibilities present themselves. Firstly, the fort predated the fortress and/or overlapped with its period of construction. It would therefore have been built during in or shortly after AD 43 as part of the early military occupation of Camulodunum. Secondly, the fort accommodated the small force which Tacitus stated was on hand at the time of the Boudican revolt (*inert modica militum manus* (*Annales*, xiv. 32, 3)), in which case it could have been built as early as c 48 when the fortress was evacuated by the army. Thirdly, it was constructed shortly after AD 60/1 as part of the renewed military campaign in the aftermath of the revolt.

Short of excavation, only the plan of the fort can offer any help with closer dating. The absence of a *porta decumana* is a distinctive feature shared by the three best known forts of this period, namely Valkenburg I (van Giffen 1948), Hod Hill (Richmond 1968), and Great Casterton (Todd 1968), all of which date from about the AD 40s. Since only one other British fort is known to have had this form of layout, namely the practice fort D at Cawthorn (Richmond 1933; Jones 1975, 140-41), the fort at Gosbecks seems more likely to have been built in the AD 40s. In other words, its plan favours a construction date of c 43 or c 48 but not as late as C60/1.

The relationship of the Gosbecks fort to the native settlement and its defences is similar to that at Hod Hill, where the fort was built inside the hillfort and against its defences (Richmond 1968). The difference between the two, however, is that whereas the native settlement at Hod Hill apparently came to an end with the arrival of the Roman army (Richmond 1968, 33), there is no reason to suppose this was the case at Gosbecks.

## The portico, temple and enclosure ditch

[Figs 5.5-5.6]

The Romano-Celtic temple stood in the SE corner of a square enclosure which was surrounded by a deep ditch and a double portico. The group as a whole lay in the western part of a large walled enclosure which was subdivided into two by an internal north-south wall (Fig 5.5).

M R Hull trenched the square enclosure from its NW to its SE corners so as to cross diagonally the Romano-Celtic temple and, in two places, the double portico and the deep ditch. Although the Romano-Celtic temple (Temple 8; Lewis 1966 and Crummy 1980a) shows clearly in aerial photographs as a temple of the standard square-within-a-square plan, measuring externally about 15 m (50 ft) across, Hull failed in his excavation to find any traces of the walls apart from vague patches of mortar. As Hull comments, the walls were probably robbed out and missed during his excavation (*Rom Col*, 261-3).

In his two sections, Hull found the ditch to be V-shaped in profile, 3.4 m (11 ft) deep and 10.2-11.0 m (33.5-36 ft) wide. At the bottom was fine silt 400 mm (15 in) deep in one section and 800 mm (30 in) in the other. Directly on top of the latter, the fill was apparently of one period and contained much gravel, tile, septaria, pottery and *tesserae*. Directly on top of the silt was much mortar, 3 inches in one place, which Hull seems to have equated with the construction of the double portico. In the silt itself was a coin of Cunobelin and a little native pottery (*Rom Col*, 263-4). The portico consisted of three parallel walls round all four sides of the enclosure, with, as indicated on aerial photographs, two cross walls in the middle of its eastern side. This corresponds to a gap in the centre of the eastern side of the ditch and formed the entrance into the western wall of the western enclosure in which it lay. The central foundation, where examined in 1977 (pp 122-3 & Fig 6.12; Crummy & Smith 1977), was 1.0 m (3.3 ft) deep and 1.1 m (3.6 ft) wide, the inner foundation 0.6 m (2.0 ft) deep and 1.1 m (3.6 ft) wide and the outer foundation 0.6 m (2.0 ft) deep and 1.0 m (3.3 ft) wide. The only layer of any consequence to survive within the walls (at least in the area examined in 1977) was a thin patchy spread of gravel which may have been the remains *in situ* of the base of an external bank round the deep ditch.

The walls of the enclosure in which the portico lay have only been partly traced, and thus its plan is incompletely known (*Rom Col*, 264-7). The north wall of the eastern half of the enclosure contained a series of rectangular recesses which were probably *exedrae*, recesses for benches or something similar. The magnificent and well-known statuette of Mercury is thought to have been found just outside this enclosure (*Rom Col*, 264), although there is considerable uncertainty about its find-spot.

The presence of the Romano-Celtic temple and the surrounding portico both point to the area enclosed by

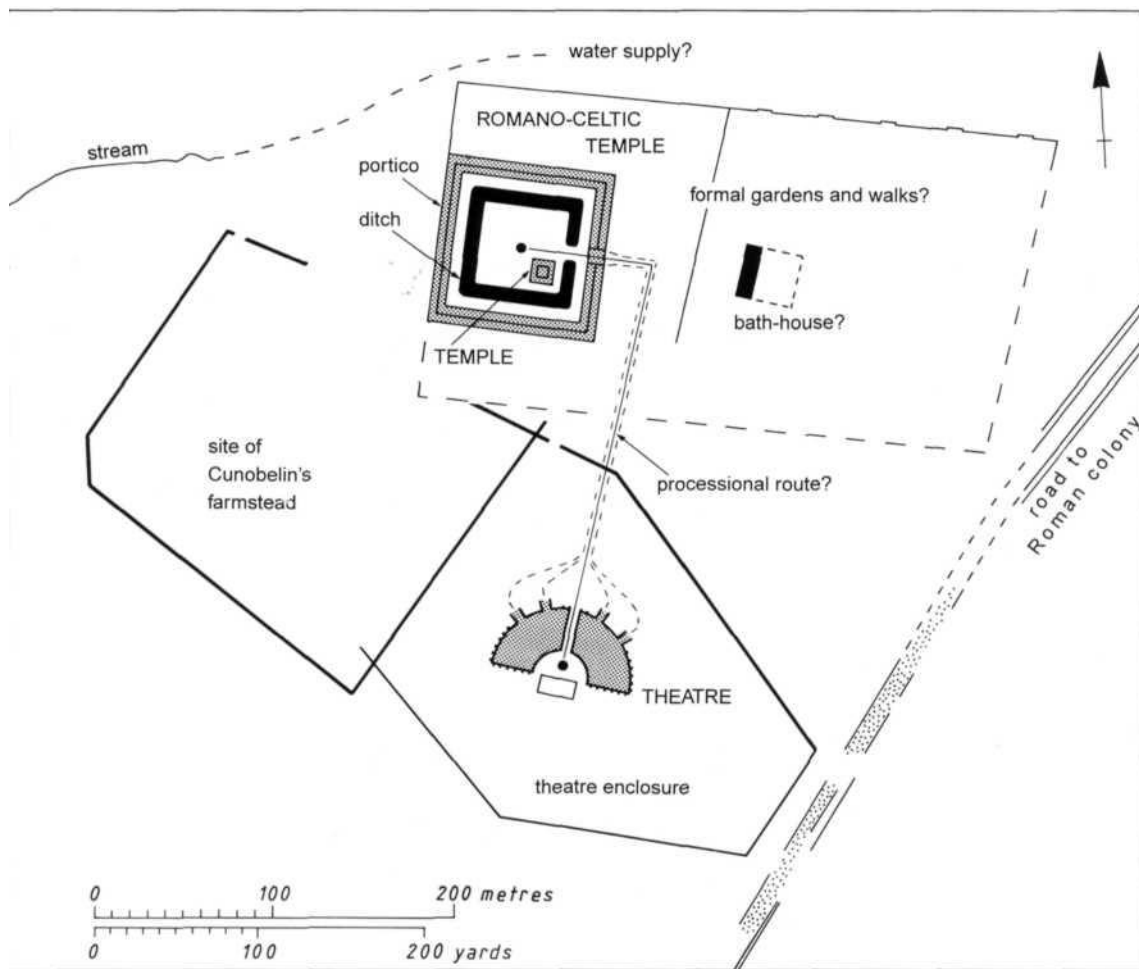


Fig 5.5 Gosbecks in the Roman period. [Pages 101-4]

the great ditch being sacred and reserved for ritual. The building materials found by Hull well down in the ditch fill should be regarded as demolition debris from the temple and portico and thus as indicative of the ditch being open when these structures were standing (*Camulodunum*, 10-11). Presumably the ditch preceded the temple and portico by some time but was retained to be incorporated in a unified layout.

There would appear to have been no room for a bank on either side of the ditch, at least not after the construction of the temple and portico. Either the bank was removed to allow one or other of these structures to be built, or (surely more likely) the ditch had never had a bank at all. The purpose of a deep, bankless ditch may have been to allow people to look into the enclosed area while denying them access. In the Roman period, this would have applied to those in the portico.

While it must be almost certain that the ditch is pre-conquest in origin, the possibility of a Roman date for it should not be dismissed. The enclosure formed by the ditch seems to be a perfect square. It is very regular in plan, more so than any of the surrounding enclosures of presumed Iron Age date. It is also neatly integrated with the layout of the double portico and the

Romano-Celtic temple, so much so that all three have the appearance of being contemporary and of one design. Especially significant in this respect is the correspondence between the positions of the entrances of the double portico and ditch, and the relationship of these to the temple, which was placed neatly in the south-east corner of the ditched area.

Vitruvius described the approved method of building porticoes and explained that in general these should be double (*De Architectura* V, 9, 2). From this information it is possible to reconstruct the Gosbecks portico closely along the lines as shown here (Fig 5.6, adapted to suit Gosbecks from Morgan 1960, 152). Vitruvius wrote that since the principal purpose of such structures was the provision of both pleasant walks and shelter from the weather, the areas enclosed by them should be embellished with vegetation to make the environment more pleasant and healthy. In a similar vein, he went on to describe the virtues of uncovered walks set out as part of ornamental gardens. Thus at Gosbecks it would not be too fanciful to imagine the area within the large double enclosure as having been laid out as formal gardens and walks. The *exedrae* of the eastern enclosure are neatly compatible with such an explanation.



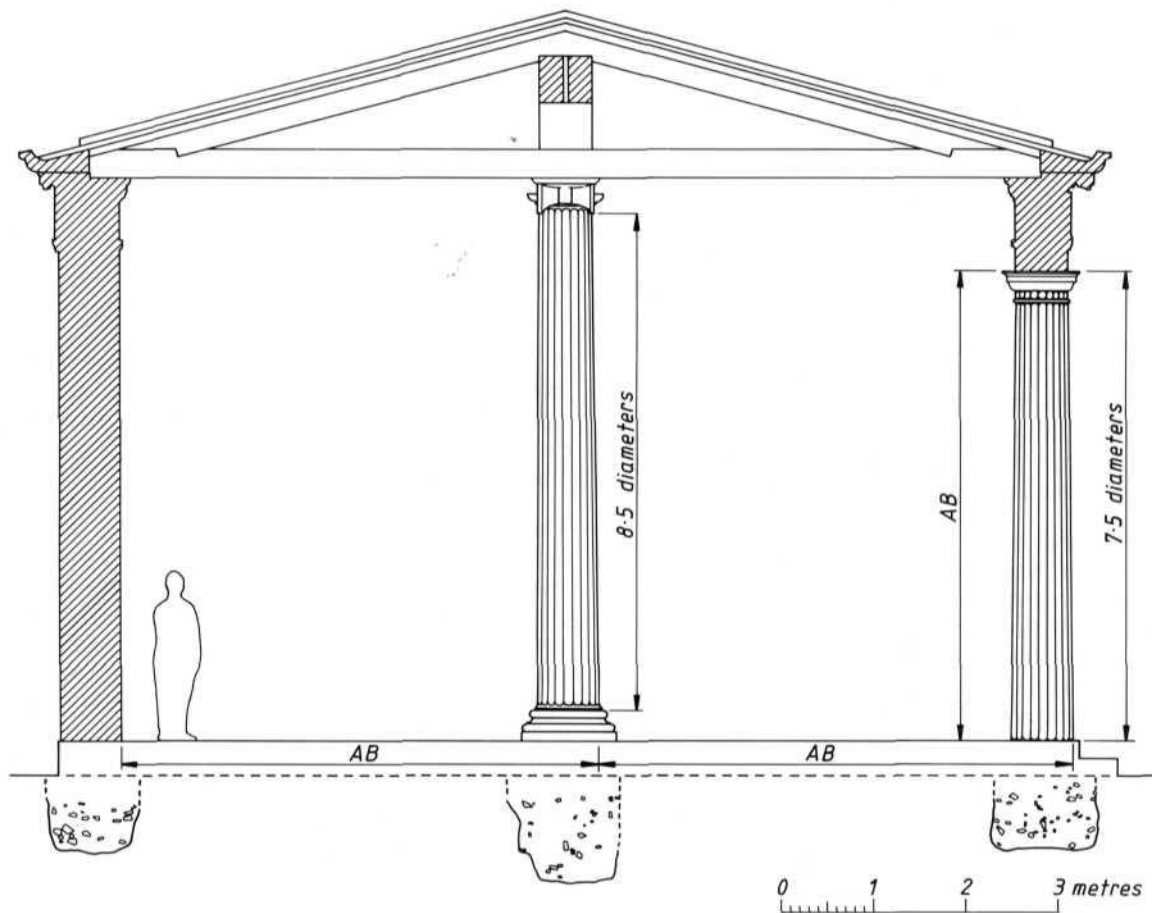


Fig 5.6 A reconstruction of the Gosbecks portico. Based on Vitruvius and the excavation of 1977 (pp 122-3). [Pages 101-2]

## The theatre

The remains of the theatre at Gosbecks stand as a low mound only 1.5 m (5 ft) high. Although dug into by Jenkins as early as 1842 (Jenkins 1846, 48), the theatre was not recognised as such until the limited exploratory excavations of Hull in 1948 (*Rom Col*, 267-9). Present knowledge of the theatre stems mainly from the more detailed excavations of 1967 by B R K Dunnett who, by partial excavation of the western half, interprets the theatre in terms of two phases. At first the theatre was of timber which was built and demolished in the 2nd century. It had a semicircular *cavea* pierced by an axial passage at ground level leading to the centre of the orchestra. There was a rectangular timber stage but apparently no wings and no *scaena* building behind. Later the timber *cavea* was replaced by one in turf and sandy clay with a stone revetment wall. The latter was decorated with pilasters and had four external staircases. The end of the later theatre is dated to the mid 3rd century, if not much earlier.

The excavation of 1977, carried out primarily to assess plough damage, enabled a re-examination of the 'turf' of the Phase 2 *cavea*. This, it would seem, was blocks

of cover loam (cf Fig 6.12 on p 123) on average 0.10-0.15 m (4-6 ins) thick, most of which had in places a thin, dark turf-line still *in situ* (Crummy & Smith 1977).

The plan of the theatre is discussed further below (p105).

## Possible bath-house

[Fig 5.1 & 5.5]

Gosbecks may yet prove to have boasted a bath-house. The most likely site is shown by a rectangular cropmark measuring 35 x 10 m (very approximately) which could indicate the remains of hypocausted rooms. If not, it could be the site of a *piscina*. The site lies east of the portico entrance and inside the eastern half of the double enclosure (Fig 5.5). The straight cropmark extending NW from one corner could be a drain or trench for a pressurised water-main. A trench containing the remains of at least one water-main was found about 400 m to the north in 1994 (Benfield 1994). They may share a common source at the head of the spring immediately north of the portico.

## The Colchester-Gosbecks road and its secondary enclosures at Gosbecks

The road from Gosbecks to Colchester has been sectioned only once. This was in 1936 at Rayner's Farm, when Hull was puzzled to find three ditches rather than the two he had expected (*Rom Col*, 10). However it is clear from aerial photographs that the road had four ditches in all, the outermost pair forming the limits of flanking footways; of these Hull had found only three. Where visible, the road is perfectly straight except at Gosbecks where there is a slight change of direction southwards (6°). South of this the road shows as a light cropmark indicating the survival of metalling (Figs 5.1 & 5.5). Although not certain, it seems from aerial photographs that the road did not continue beyond its southern limit as known at present.

At its southern end there was a series of enclosures which must have been laid out after the conquest. Some of these presumably contained buildings which fronted on to the Roman road. Together such buildings are likely to have provided a substantial proportion of the inhabited dwellings on the site and thus they suggest that the number of people living at Gosbecks, at least in the Roman period, must have been quite small.

### Other discoveries

In 1943, as a result of deep ploughing, several burials were found not far west of the large farmstead (Fig 5.1). Information about the discovery is minimal but the circumstances and date of the finds suggest that they were cremations. It appears that the burials were inferred from the exposure of an unrecorded number of pots, the tops of which had been removed by ploughing. No bones are recorded as having been found. Hull described the pots as 'all Roman coarse ware .... scarcely sufficient to give more than a broad 1st to 2nd century date' (*Rom Col*, 259).

Between 1948 and 1950, the Roman Essex Society and Hull carried out a series of small excavations (*Rom Col*, 259-71). This work included the sectioning of ditches of the large farmstead, an examination of the walls of the double enclosure containing the double portico and the discovery of the theatre, all of which have been mentioned above. Other discoveries consisted of, at point N on Figure 5.1, a tile-built kiln of unknown function and probably of 2nd-century date. Near the latter was found a well-preserved timber structure, the purpose of which was also unknown (*Rom Col*, 269-70).

On the surface of the area south of the theatre, Hull records the exposure after ploughing of 'many patches of burnt earth' in or near which were 'dense patches of a peculiar, thin and heavy slag' indicating an 'extensive metal-working industry' (*Rom Col*, 260). From the

position of these remains, it is probable that they were associated with the secondary enclosures at the south end of the Gosbecks-Colchester Roman road and are therefore of post-conquest origin.

### Conclusions

[Fig 5.1 & 5.5]

It is important to bear in mind how limited the excavations have been at Gosbecks and consequently how speculative are some of our conclusions about the nature of the site. Nevertheless, some broad conclusions can be drawn.

The scale and central position of the farmstead at Gosbecks, and the convergence on it of the trackways and field systems suggest that within the enclosure in pre-Roman times was the occupation site of the highest social status — in other words Cunobelin and his predecessors. That there was a substantial population in the Gosbecks area at the time of the conquest is implicit in the construction on the site of a Roman fort and the secondary enclosures at the southern end of the Colchester-Gosbecks road which, as suggested above, must have been set out after the conquest. The arrangement of fields seems compatible with mixed farming carried out on a large scale on the in-field out-field system. Close by the large farmstead, especially to the west and south of it, the fields tend to be compact and approximately rectangular. These were probably used primarily for cultivation and contrast with the large and more irregular parcels of land around the fringes of the cropmark complex which were more likely associated with pasture. The latter is exemplified by the enclosures examined at the Dugard Avenue site (pp 116-19). Ditch 3 is long, straggling and curved towards Kidman's Dyke North to form a small gap between it and the ditch of the dyke as if intended for the control of livestock. The southern end of the Dugard Dyke has a similar appearance and the northern end of Ditch 2 forms an enclosure so small (ie Ditches 2a and 2c) that any function other than some kind of animal pen is hard to imagine.

The trackways would have been most effective as droveways for moving animals from area to area. The fact that these appear to have been linked to the large trapezoidal enclosure suggests that the latter was associated with livestock. The substantial dimensions of the enclosure ditch show that its main purpose was to protect the animals within from theft and predators, and the exceptional size of the area of the farmstead enclosure itself can be seen as a reflection of the large numbers of animals involved.

The trapezoidal enclosure has a double ditch on its southern and eastern sides. Either the ditch here was of two phases or the trackway system continued around two sides of the main enclosure as shown on Figure 5.1. The latter explanation is perhaps less likely

because the distance between the two ditches on the south side is much less than that between the two ditches on the east. This is an unlikely discrepancy if there had been a trackway in this position. However, trackway systems continuing around the sides of farmstead enclosures have been postulated elsewhere, ie at Barton Court Period III (Miles 1978, 84-5) and Fisherwick (Smith 1977, 59-60). Had this been the case at Gosbecks, then perhaps this part of the trackway had a secondary use as a corral just as has been suggested for a comparable arrangement at the Danebury hillfort (Cunliffe 1974, 177).

The extent to which agricultural operations continued at Gosbecks after the Roman conquest is uncertain. There was certainly industrial activity of some kind on the site at Gosbecks during the post-Boudican period. The scale of this is unclear, but its existence at least in about the 2nd century is attested by the kiln-like structure found in 1948 and the discovery at about the same time of scatters of slag (p 104; *Rom Col*, 269-70).

Gosbecks has been compared with the great sanctuaries of Gaul (*Rom Col*, xxx; Dunnett 1971b, 27) such as Sanxay (Formige 1944), Vieil-Evreux (Esperandieu 1913 & 1921; Baudot 1943), Champlieu (Grenier 1958, 407-414 & 1960, 814-18), Drevant (Grenier 1958, 359-62), Berthouville (*Antiq J*, 8 (1928), 322; *Trier, Jahreas* 3, 60), Le Mont Castillon (*Gallia*, 7 (1949), 114) and Ribemont-sur-Ancre (Agache 1978, 404-410). But with the probable exception of Champlieu, these sites were on a much larger scale. A closer parallel is provided by the sanctuary of Lenus-Mars at Trier (Wightman 1970, 211-13). This lay on the opposite bank of the Mosel to the colony on a site probably sacred in pre-Roman times. It contained a large hybrid classical Celtic temple, a theatre, *exedrae* and a large enclosure but apparently no baths.

It would be a mistake to see Gosbecks in the Roman period as being only a native sanctuary, since the site may have fulfilled commercial and administrative roles which are likely to have been bound up with its religious function and probably more important than that.

The plan of the theatre differs in many respects from the Vitruvian model for Roman theatres, and the key to understanding the function of Gosbecks in the Roman period may lie in understanding these differences. The apparent absence of a *scaena* building is very significant since this suggests that the theatre was primarily intended not for performance but for ritual, ceremony and debate. The position of the portico reinforces this conclusion. It was north of the theatre rather than behind the stage as described by Vitruvius. He said that it should be in this position partly so that there would be somewhere to prepare

the props. He also tells us that the orchestra was to contain seats reserved for senators (*De Architectura*, v. 6, 2 and 9, 1). Thus the axial ground-level passage at Gosbecks could have provided access into the orchestra for prominent people of various kinds coming from the portico perhaps via a 'processional route' (Fig 5.5).

The arrangement at Gosbecks is reminiscent of Lugdunum where representatives of the Gallic tribes met in the amphitheatre at the outskirts of the town. Near the amphitheatre was a massive sanctuary containing the famous altar erected in honour of Rome and Augustus (Drinkwater 1983, 11-14). An important difference appears to be that the site at Lugdunum was primarily for the promotion of the imperial cult which at Colchester was presumably fulfilled by the Temple of Claudius in the Roman colony.

However, at the very least, we could see the portico, its outer double enclosure, and the theatre at Gosbecks as being the administrative focus of the *civitates* and a Romanised version of whatever equivalent arrangements existed for the Trinovantes in the immediate pre-Roman period. A more extreme view is that, in the Roman period, Gosbecks was where the provincial council met, and before that it had been a place of assembly of the tribes which either were subject to the Catuvellauni or formed a broader-based British confederation (if such existed). Both possibilities suit Suetonius' description of Cunobelin as the King of the Britons (*Life of Caligula*, 44, 2).

In addition to all this, there would have been Gosbecks' commercial role. The statuette of Mercury, patron of commerce, shows that trade is likely to have been very important here, not only in the Roman period, but before the conquest too. Indeed, as will be suggested below (pp 172-5), Camulodunum is likely to have been founded as a coastal trading centre, so Gosbecks would be the natural place to hold the largest of its periodic markets.

The theatre and the scale of the portico show that the site must have been visited by large numbers of people in the Roman period. The theatre was significantly bigger than the theatre in the walled Roman town (*CAR* 6, 385-6; Crummy 1982). It was large enough to accommodate up to 5,000 people seated and it is probably no coincidence that the portico, having a circuit of 340 m (approx), could have held them all. Such large numbers of people were never resident on the site: they must have come from the Roman town close by and the surrounding area. Although there is no direct evidence to prove it, the site was probably equally popular for much the same reasons in Late Iron Age times.

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## 6: Catalogue of excavations and observations (1932-1993) relating to the pre-Roman period

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This chapter consists of a catalogue of the excavations and observations concerned with the pre-Roman period within the area enclosed by the dykes. The locations of the sites are shown on Figure 6.1.

The sub-headings in the entries are of the same format which is as follows. The first line gives the National Grid reference, the second line shows whether the entry relates to an excavation or an observation, the third line gives the name of the compiler of the original record, and the last line shows the source of the information used in the entry. The abbreviations are as follows:

BRKD      B R K Dunnett (now Niblett)  
CFCH      C F C Hawkes  
MRH      M R Hull  
PC        Philip Crummy

### *Kilns on Figure 6.1*

Kilns 1 to 33 are listed and described in Hull's *The Roman Potters' Kilns of Colchester*. Kilns 34 and 35 were discovered during the excavation of Temples 4 and 5 at Sheepen by Mr B Blake in 1959-60 (*Jour Rom Stud*, 51 (1961), 184-5). Kilns 36 and 37 were for making tiles and bricks and were excavated in 1969-70 by the Colchester Archaeological Group (Holbert 1971). Kilns 38 to 40 were excavated in 1973 at Oaks Drive (*CAR* 6, 338-40). Kiln 41 was discovered in 1979 at Middleborough (*CAR* 3, 182-4).

### *The temples on Figure 6.1*

Temples 1, 2, 3, 6, 7 and 8 are described in *Roman Colchester*, 160-77. The excavation of Temples 4 and 5 is unpublished but notes on the work are to be found in *Jour Rom Stud*, 50 (1900), 229 and *Jour Rom Stud*, 51 (1961), 184-5. The presumed Roman church (Temple 9) was partly excavated in 1938 (*Rom Col*, 245-8) and again in 1965 (Dunnett 1971a, 78-84). Fuller excavations were carried out in the late 1970s and 1980s (*CAR* 9, 164-91). Temple 10 was excavated in 1977 (*CAR* 3, 123-6).

Temples 1 to 7 are described in Lewis 1966 and Temples 1 to 10 in Crummy 1979b.

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### 1

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Bronze Age cemetery, Chitts Hill

TL 95802653  
Excavation  
PC  
1973  
Trans Essex Arch Soc, 9 (1977), 1-16  
Five ring-ditches (shown on plan), twenty-four Deverel-

Rimbury urned cremations; also two ?Late Bronze Age or ?Iron Age field-ditches.

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### 2

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Ditch of uncertain date, Chitts Hill

TL 95702636  
Excavation  
M Petchy  
1973  
Trans Essex Arch Soc, 9 (1977), 17-20  
Late Bronze Age or Iron Age?

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### 3

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Gryme's Dyke North

TL 95692625  
Observation  
Recorder unknown  
c 1930  
Note on CM OS 6" map  
Gryme's Dyke cut by water-main?

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### 4

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Gryme's Dyke North

TL 95832587  
Observation  
Recorder unknown  
c 1930  
Note on CM OS 6" map  
Dyke 'clearly marked' although position compared with cropmark further north slightly off line.

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Gryme's Dyke

TL 95882560337  
Observation  
P Laver  
1923  
Laver's Diary, 27/3/1923 (MS in CM)  
The ditch, seen in the side of a pit, 'was 23 ft west of the west hedge of the road, and filled with black earth only'.  
Position along dyke approximate.

<p>6</p> <hr/> <p>Roman road, Iron Latch Lane</p> <p>TL 95492549 Excavation MRH 1936 <i>Rom Col</i>, 12 Section across road.</p>	<p>11</p> <hr/> <p>Triple Dyke</p> <p>TL 95372539 Excavation CFCH (presumably) 1961 Note on CM OS 6" map Section in Hunter's Rough (227-230 yds north of Lexden Road). See pages 55-9.</p>
<p>7</p> <hr/> <p>?Roman road, Lucy Lane</p> <p>TL 952252 (approx) Observation P Laver 1923 <i>Rom Col</i>, 12-13 Metalling observed in a bank.</p>	<p>12</p> <hr/> <p>North-south ditch, Straight Road</p> <p>TL 96542432 Observation P Laver 1923 Laver's Diary 27/3/1923 with small sketch section A deep ditch was apparently observed in east-west trenches dug about 10 m apart to lay sewer pipes. Laver judged the ditch to be 9 ft deep and about as wide as the road.</p>
<p>8</p> <hr/> <p>Roman road, Halstead Road</p> <p>TL 95822519 Observation MRH Positions noted on CM OS 6" map; <i>Rom Col</i>, 12 Section across part of a Roman road seen under the modern Halstead Road. The exact position is uncertain.  A location is shown on a CM OS 6" map. The latter shows the location of another section across the road; presumably this is a mistaken plotting of the previous entry.</p>	<p>13</p> <hr/> <p>Roman road, no 149 Straight Road</p> <p>TL 96622425 Excavation PC 1977 Mr White, the owner of the property, carried out an excavation 6.7 x 2.0 m at the far end of his back garden where he discovered Roman road metalling of two phases. The earlier had a well-preserved surface 0.45 m below present ground level and make-up 0.10 m thick of sandy loam. The surface of the later phase had been destroyed but 0.10 m of its gravel make-up still survived. Mr White believed there to be indications of a possible ditch aligned roughly east-west along the southern edge of the road metalling. The site was visited by PC.</p>
<p>9</p> <hr/> <p>?Roman road ditch, London Road</p> <p>TL 95982500 Observation P Laver 1923 <i>Rom Col</i>, 13 Observation trench. Ditch was 12 ft across and 5 ft deep below modern footpath. No finds to date.</p>	<p>14</p> <hr/> <p>Ditches north of Dugard Avenue ('Oaklands Avenue excavation')</p> <p>TL 96442417 Excavation PC 1975 Fig 6.2 Two trenches were dug by machine across the ditch of the so-called 'palisaded earthwork' (pp 59-6-; Ditch 10; Fig 6.2). Cropmark photographs were used to site the trenches. The excavation was carried out in advance of imminent residential redevelopment. In the side of the machine-cut trenches, not only was the ditch observed but so too was another ditch (Ditch 12) which was hitherto unknown. Small sections of both were excavated by hand but no finds were recovered. The ditch of the 'palisaded earthwork' was of different dimensions in each trench, being from the modern</p>
<p>10</p> <hr/> <p>Line of the Triple Dyke near the river Colne</p> <p>TL 96322577 Observation BRKD &amp; D G Davies 1965 Note on CM OS 6" map Note records, 'no trace of dyke in valley bottom'.</p>	

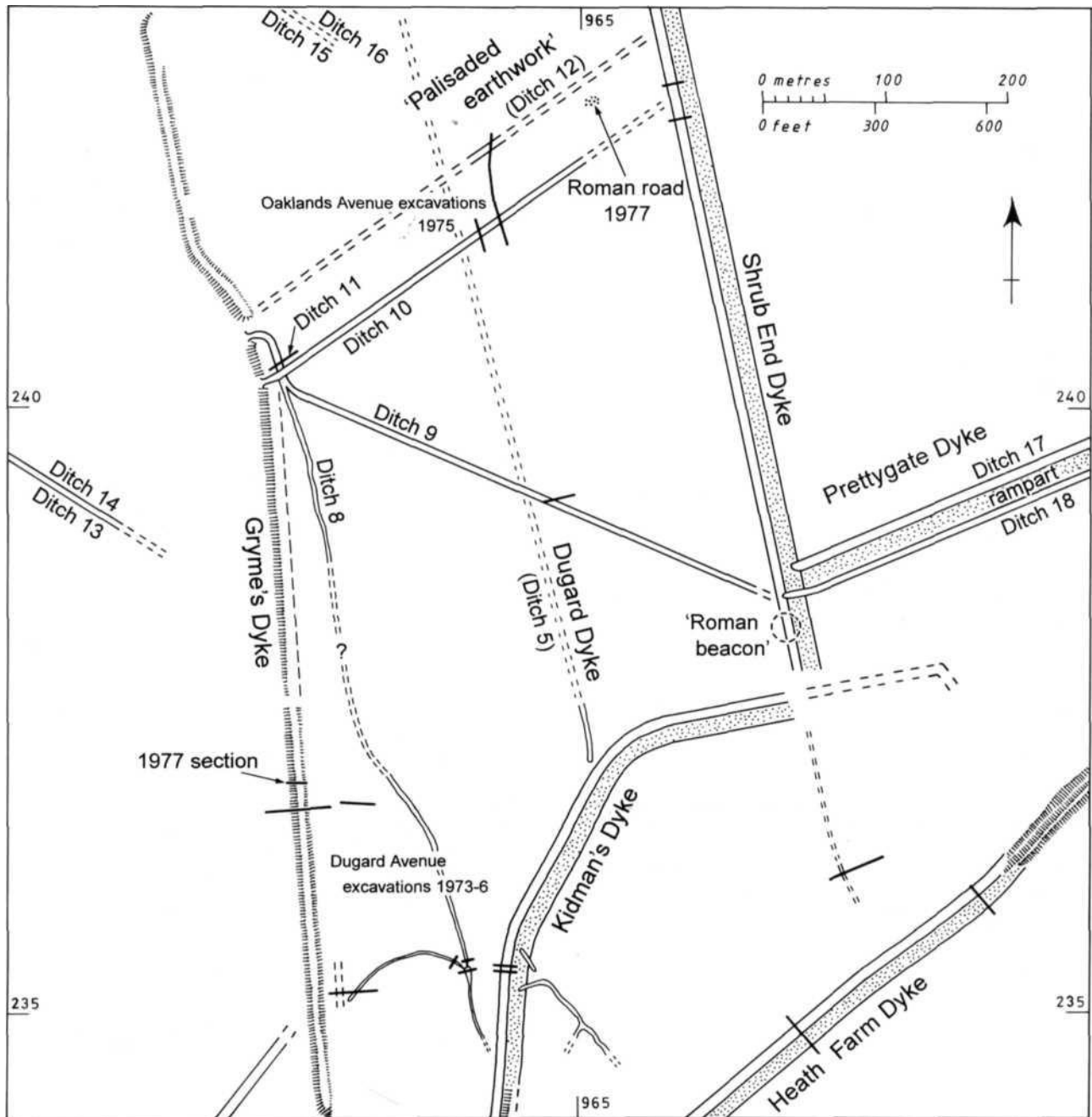


Fig 6.2 Plan of the area east of the entrance through Gryme's Dyke. [Pages 107-8]

ground-surface 1.8 m (5.9 ft) deep and 3.6 m (11.6 ft) wide in one and 1.7 m (5.6 ft) deep and 5.0 m (16.4 ft) wide in the other. Ditch 12 was 1.8 m (5.9 ft) wide and from the modern ground level 3.3 m (19.8 ft) deep. The exact direction of Ditch 12 was not established but it seemed to be roughly parallel to the ditch of the 'palisaded earthwork'.

The gravel metalling in the rear garden of 142 Straight Road (no 13 above) lies just north of the line of Ditch 12 projected north-eastwards. No metalling was noted in the 1975 trenches discussed here. Thus Ditch 12 is best interpreted as the south ditch of a Roman road which was parallel to the 'palisaded earthwork' and passed through the entrance in Gryme's Dyke to the south-west (Fig 6.2).

No sign was detected of the 'palisade trench' found to the west (p 60). Its absence suggests that another explanation needs to be found.

15

Dugard Dyke

TL 96412412  
Excavation  
CFCH (possibly by A F Hall)  
1957

Section showing north end of dyke. See page 26.

16

## Dugard Dyke

TL 96512375  
Excavation  
CFCH (possibly with A F Hall)  
1956-57

Section very near the south end of dyke. See page 26.

17

## The 'Laver ditch' (Ditch 9)

TL 96482392  
Excavation  
A F Hall  
1936

The following information is taken from Hull's MS *Colchester outside the walls* in the Colchester Museum.

An 18 m (60 ft) long east-west trench was dug by Mr A F Hall at the northern end of field 1302 and was positioned to section our Ditch 9 (Fig 6.2). The ditch was cut obliquely and found to be 1.2 m (4 ft) deep below modern ground level and (after allowing for the angle of the cut) approximately 3.0 m (10 ft) wide. Mr Hall wrote, 'At the bottom there is an almost stoneless silt. Above the silt the soil filling grows progressively more stony. There was no pottery here or anywhere in the dig.' The trench extended eastwards and another ditch was found about 6 m (20 ft) east of the first. It was 1.1 m (3.5 ft) wide and about 0.5 m (1.5 ft) deep below the modern ground level. Hall thought that the latter may have been cut into an earlier much bigger ditch which was difficult to differentiate from the natural. Further trenches, the exact positions of which are not known, were dug to the north and north-west in fields 1299 and 1300 respectively. Ditch 9 was followed for some distance towards the entrance in Gryme's Dyke but no further traces could be found of the small ditch or the putative ditch into which it is supposedly cut.

18

## The 'Laver ditch' (Ditch 9)

TL 96402396  
Observation  
1953  
Note on CM OS 6" map

Ditch sectioned obliquely giving a depth of 4 feet and an oblique width of 22-24 feet.

19

## Gryme's Dyke North — entrance and convergence of ditches (Ditches 8-11) to the south

TL 96262404  
Excavation  
AFH/CFCH/R J Appleby  
1946/1947-8, 1952, 1956-7/1958  
CMR 1950-54, 18-19

See pages 27-9 & 59-61.

20

## Gryme's Dyke excavation, 1977

TL 96272370  
Excavation  
PC  
1977  
Figs 6.3-6.6

*Introduction*

Excavation of a section across Gryme's Dyke took place in September 1976. The site is 64 m south of Dugard Avenue where planning permission was granted to remove part of the rampart to construct a footpath across the line of the earthwork. Unfortunately, most of the bank was bulldozed before the archaeological work could begin and without fulfilling the statutory requirements under the Ancient Monuments Acts. Archaeological excavations still took place however and the ground level at the site when this began is indicated in the section drawing on Figure 6.3.

*The bank*

By the time the archaeological work began, only the very base of the rampart at its eastern end survived (Fig 6.4). This consisted of sand and gravel overlying a layer 0.15 m thick of redeposited cover loam (Layer 5) which no doubt had derived from the area of the ditch.

In section, tips of sand, gravel and cover loam were visible in the body of the bank. These sloped upwards in a westward direction. The material forming the bank stopped along an indistinct vertical line, delineated by its junction with Layer 9 which consisted of brown sandy loam with stones and small gravel.

At the western end of the section was also a small deposit consisting of redeposited cover loam (Layer 8) within which no traces of the vertical line dividing Layer 9 from the bank could be detected.

The upper parts of the bank had been badly damaged. In section, two deposits of brown sandy loam (Layers 6 and 10) did not seem to be part of the original bank since they do not follow the general pattern of tip-lines within the bank but sealed them.

Unfortunately, the southern section of the trench had been badly mutilated by the developers whilst removing a tree so that it was not possible to confirm if the features in the northern section were repeated there.

*The buried soil*

The buried soil was well preserved (Fig 6.4). At places in section a very thin dark grey turf-line (Layer 2) no more than 0.2-0.3 m thick was visible. The cover loam (Layer 3) varied in thickness between 0.18 and 0.4 m and was the usual sandy silt loam (discussed on p 129). It contained a distinct worm-sorted horizon 0.05-0.08 m thick. At the base of the cover loam, overlying the glacially-deposited sand and gravel, was a layer of very stony cover loam (Layer 4).

*The pre-bank features*

Sealed by the bank and cutting into the cover loam was a series of small shallow features (Fig 6.3) filled with

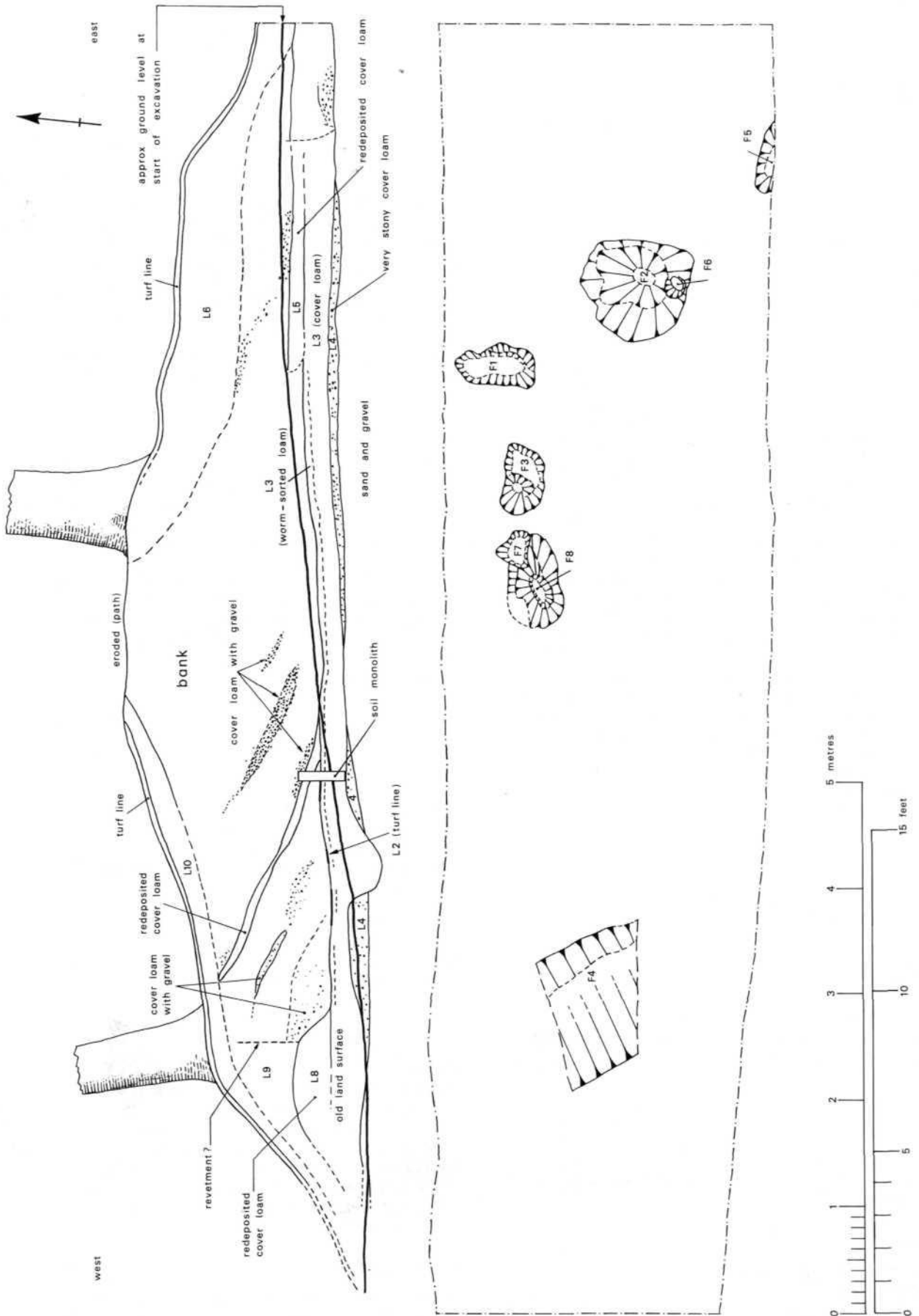


Fig 6.3 Gryme's Dyke 1977 (no 20). Section and plan. [Pages 109-115]





Fig 6.4 Gryme's Dyke 1977 (no 20). Above: the north section. Below: the buried soil profile beneath the rampart. The old turf-line appears as a dark line across the centre of the photograph. [Pages 109-115]

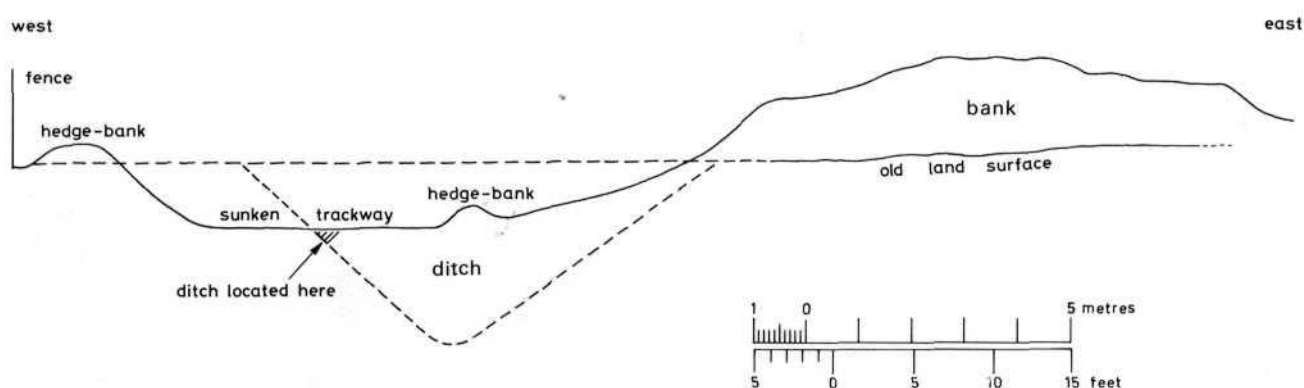


Fig 6.5 Gryme's Dyke 1977 (no 20). Section/profile across the bank, ditch, and later trackway.

yellowish-brown or brownish-yellow sandy silt loam mixed in some cases with gravel.

Feature 7 was 0.35 m deep and was possibly a post hole. Features 1, 2 and 8 were shallow depressions. Feature 6 may have been a small feature in its own right or simply part of F2. Feature 4 contained no pottery and was filled with material identical to the cover loam so that it was possibly natural. The same is true of Features 3 and 5 except that the fill of these was slightly browner than the surrounding cover loam. Features 1, 2, 5 and 6 were clearly sealed by Layer 5.

The most easterly area of the old ground surface was slightly browner than the rest and may therefore have been the fill of a feature except that it appeared to cut Layer 5 (see section). However, the difference in colour may be due to this area being under the tail of the rampart and therefore where the cover loam has been subjected to slightly different chemical conditions and a greater degree of root penetration than elsewhere.

#### The ditch

The ditch, not sectioned in 1977, lies to the west of the rampart under a sunken trackway (Fig 6.5). In 1978, following various proposals to lay pipes along the side of the earthwork, a very narrow exploratory trench was dug in the trackway to establish the western limit of the ditch so that future services laid along the trackway could avoid it. The western edge of the ditch was found about halfway across the trackway indicating that the width of the ditch was originally about 8.0 m (26 ft).

#### The finds

On the old land surface under the bank were found rim sherds from a large vessel also of uncertain form (Fig 6.6, no 2). The paste is light grey with dark grey surfaces. The vessel may have been a bowl of form Cam 230 or less likely of Cam 221.

In Feature 2, a substantial amount of pottery was discovered. This includes parts of three storage jars, two of form Cam 270A (Fig 6.6, nos 7 & 9) and one of form

Cam 271 (Fig 6.6, no 4), two bowls, one of form Cam 211 (Fig 6.6, no 5) and the other of form Cam 218 (Fig 6.6, no 6), two butt-beakers, one of form Cam 116 (Fig 6.6, no 8) and the other of form Cam 118 (Fig 6.6, no 10), a pot of form Cam 254 (Fig 6.6, no 12) and a vessel unparalleled at Sheepen (Fig 6.6, no 11). In Feature 1, the principal find was part of a storage jar of form Cam 270A (Fig 6.6, no 3).

All the storage vessels are in soft grey ware usually with oxidised surfaces; the Cam 211 bowl is in light grey ware identical to the pot from the old land surface (Fig 6.6, no 2) and has two false cordons. The beaker Cam 116 is unusual in Colchester in that its fabric is grey not TR4; the Cam 254 pot is in the standard 'corky' fabric, and the Cam 218 and Cam 118 are in 'native' soapy ware. The vessel (Fig 6.6, no 11) appears to be the forerunner of Roman 'poppy-head' beakers and is in soft soapy 'native' ware.

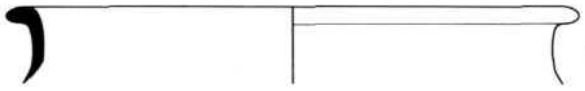
From the redeposited cover loam (Layer 5) at the base of the rampart and forming part of it were a handful of abraded sherds and a badly corroded Claudian copy. Dr R Reece's report about the latter is given below. The most significant coarse ware sherds are light grey and sandy and belong to a wheel-thrown bowl of uncertain form and date (Fig 6.6, no 1). Its fabric seems indicative of a post-conquest date. Of critical importance to the dating of the dyke was a small sherd of *terra sigillata*. This has been examined by three specialists with conflicting results. Dr G Simpson favours a date-range of the sherd of AD 40-55, Mr G B Dannell c AD 55-75 and Dr B Hartley a Neronian or Flavian date and certainly no earlier than AD 50. Dr Simpson and Mr Dannell have kindly provided reports and these are given below. About the sherd Dr Hartley writes (by letter 14.4.78), 'The piece is quite certainly South Gaulish. It is from the footring of a decorated bowl, but there is not enough to be sure whether it comes from form 37 or 30. My guess would be that it is more likely to be the latter, and if so, it gives a rather wide date-range. The fabric and glaze are of the kind which are normal under Nero and the Flavians and I would not care to date it at all closer, though I feel a *terminus post quem* of AD 50 is inevitable, and I suspect that one of AD 55 is even more likely.'

base of bank (L5)

old land surface

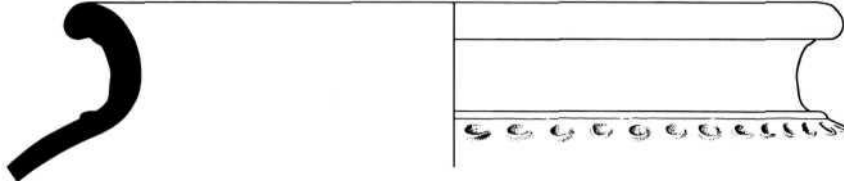


1



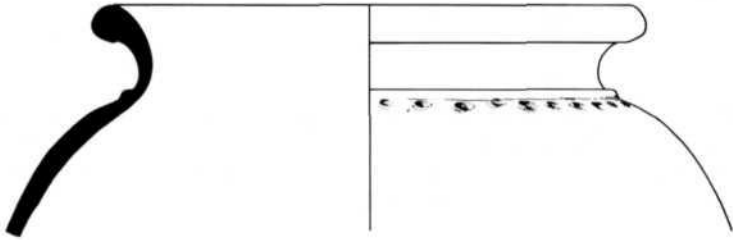
2

F1



3

F2



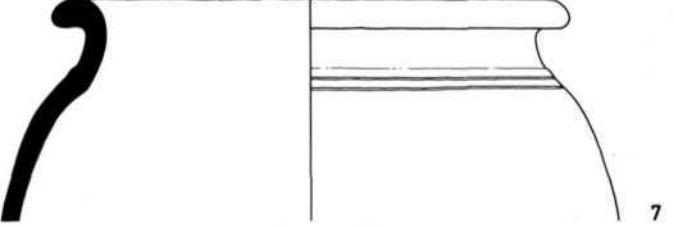
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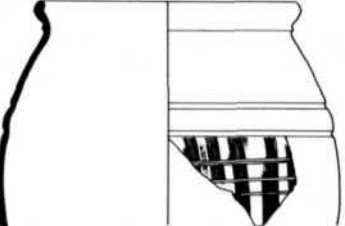
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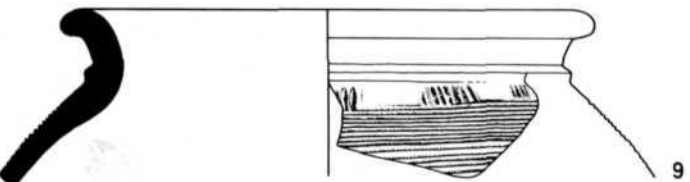
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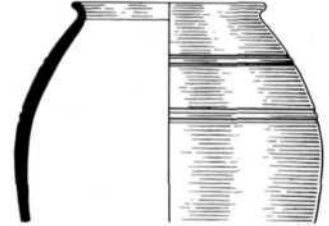
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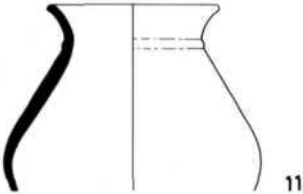
8



9



10



11



12

Fig 6.6 Gryme's Dyke 1977 (no 20). Pottery. 1:4. [Page 112]

*The sherd of terra sigillata from Layer 5*

1) Report by Dr G Simpson

Dragendorff form 30. Small fragment from the footring with the fine exterior mouldings of early South Gaulish examples of this form. The fabric is brownish-pink, with no trace of the creamy-yellow speckles which are typical of late Neronian and Flavian fabrics from La Graufesenque.

The exterior gloss is a very fine quality red: more red than Tiberian, and not so brilliantly glossy red as the Neronian vessels from c AD 55. The interior gloss has been covered by a modern solution which gives the impression that it is the brilliant Neronian gloss.

Period of production: c AD 40-55. •

2) Report by Mr G B Dannell

Dragendorff form 30, South Gaulish, small piece from the footring, including some of the mouldings below the external quarter-round bulge. Soil conditions have not aided superficial appearances, the slip is crevassed, and the outer 'skin' of the fabric has been softened. Slip survives on the inner and outer faces, but is absent from the bottom edge. A film of PVA solution has been applied to cover the marking, and has penetrated the opened surfaces.

The piece was broken for examination; the paste is reddish-pink, with some evidence of calcareous inclusions to the naked eye, the slip is reddish-brown and bright.

Any attempt to date a sherd of this type with precision is fraught with difficulty. Too little is known about the various fabrics used in the South Gaulish workshops (for general comments on the chemical and physical properties, see Picon 1973, 11-26 & 114) to be sure of the chronological significance of one or another. Consequently I decided to examine the sherd with some comparative material under a small microscope (Peak Stand Micro 50x, with 0.02 graticule). Superficial sorting eliminated the two earliest fabrics, the imitation Arretine and the very light-coloured pastes, both of which are quite distinctive. At the same time, the lack of easily-visible yellowish inclusions made a later Flavian date unlikely, as did the structure of both slip and paste.

Five variant fabrics (A-E) were detected:

- A. 29 LIC INVS F Baldock B/BF A6 P38 c AD 45-65
- 29 BASSI Camulodunum 1970 P 139 (6) c AD 50-65
- 24/5 C ELSI Fishbourne 1967 397 (6) (no 23) c AD 45-65
- 24/5 CRESTIO Fishbourne 1962 75 (9) (no 30a) c AD 45-65

This paste is heavily filled with white calcareous particles to about 15 per cent of the revealed surface. The inclusions are clearly defined and c 0.012-0.04 mm in diameter. There are also a number of smaller black inclusions (ferric?), c 0.02 mm diameter at the maximum.

- B. 24/5 CAPITO Camulodunum 1970 P2 36 (1) c AD 50-65
- 18 TERTIVS Camulodunum 1970 II B I c AD 45-65
- 18 OF MVRRA Fishbourne 1962 90 (20) (no 68b) c AD 45-65

Here the aspect is similar, but the inclusions are scarcer, and the size of the calcareous matter c 0.04-0.08 mm in diameter. The black particles are also fewer.

- C. 27 OF ALBINI Fishbourne 1965 262 (9) (no 3) c AD 45-65?

A different paste, with large inclusions of calcareous matter up to 0.44 mm diameter, the black ones being correspondingly large c 0.10-0.23 mm diameter.

- D. 18 GERMANI Fishbourne 1964 203 (18) (no 46) c AD 65-80
- 27 FELIX SEX Fishbourne 1967 351 (31) (no 42) c AD 60-80
- 18 OF JM SV Fishbourne 1966 323 (2) (no 62) c pre-Flavian

While the calcareous particles have a similar distribution to those in fabric A., they are themselves vesicular, and tend to be more regular, c 0.12 mm diameter. The black inclusions are very scarce.

- E. 18 MASCVLVS Fishbourne 1962 92 (9) (no 64) c AD 55-75
- 18 LVPVS Fishbourne 1962 90 (18) (no 59) c AD 50-65?
- 27 CAIVS Fishbourne 1964 201 (7) (no 15) c AD 55-75?

Type sherd

The calcareous material is soft and has not retained its shape, being merged into the matrix, which gives the appearance of 'clouds' rather than 'stones'. The black particles are very scarce, and the few detected 0.01 mm diameter. The colour of the calcareous matter is yellowish-white, but this may be a quirk of refraction.

The weight of the evidence suggests that the piece cannot belong to types A, B, or C, all of which are certainly pre-Flavian, on the grounds that the calcareous matter does not retain its integrity; it is more scarce, and finally the black particles are almost absent. Fabric D. differs in having clearly-defined calcareous matter in the clay matrix and a near absence of black particles. On this basis a date-range of c AD 55-75 is proposed. •

*The coin from Layer 5*  
by Richard Reece

The coin can be described very briefly. It is a worn copy of an as of Claudius showing, on the reverse, the usual figure of Minerva walking right with javelin and shield, without a reverse legend except for the expected letters SC. It is therefore a fair copy of Claudius I, RIC 66. Its date needs a little more consideration.

There must have been three steps at least leading to its deposition. First the regular coinage had to be produced in Rome, and this may have taken place any time after AD 41. The regular coins had then to be copied, probably so far as our knowledge goes at present in Spain, in northern Gaul, or in Britain. The copy, once produced, needed some time at least to become moderately worn, to reach Camulodunum, and to be deposited. An estimate of wear cannot in this case be made because we have no knowledge of the quality to be expected of the copy when it was freshly struck. Even the hesitant description 'moderately worn' may be totally inaccurate because it may have been struck in its present condition from poor or worn dies.

There are two ways of estimating the date of deposition, logically, or sensibly. The logical estimate must start with the year of accession of Claudius, AD 41. The regular coin could have been produced in that year, a copy could have been made as soon as the regular coin was available in one of the provinces, and the copy could have reached Camulodunum in the purse of the first trader to leave Gaul after the first copy was struck. Such a novelty would no doubt be much admired, pass quickly from hand to hand,

become 'moderately worn', lose its novelty, and be lost and not recovered. Logically, all this could have happened before the end of AD 42, and hence before the Claudian advance into Britain. By constructing a totally unreasonable story it is possible to let this coin beat Claudius's army to Britain by about six months. The unreasonableness of the explanation must not be confused with its probability. All possible explanations for distribution of material remains must, when considering human behaviour and its apparent irrationality when judged by the traces it leaves, be given equal probability.

There is, then, a perfectly good, logical probability that the coin was lost and not recovered in AD 42. That particular type of coin was certainly available to be lost in AD 43, 44, and certainly until the end of the reign of Claudius in 54. But then Nero struck no bronze coin at all until AD 62-3, and his first common issues appeared in 64, so such Claudian copies must have formed the bulk of the copper coinage until 65 at the earliest. Site-finds and hoards suggest that Claudian copies died out between 65 and 75 as the main coin in circulation, but there were certainly some still about up to 100. The probabilities of loss therefore start in AD 42 and improve until about AD 65, then decline sharply to 75, and are minimal thereafter.

If we start trying to quantify the probability of loss in any given year then several points quickly become apparent. The probability of loss will be greatest when the greatest numbers of Claudian copies are in circulation and therefore are available to be lost. That probability of loss will be increased towards the end of the period of use, firstly because most of the copies that are going to be made, have been made, and secondly because there is the first trickle of a new supply, and the coin may be going out of use. The year of greatest probability must therefore be 65, the year of greatest improbability must be 42 when there could only have been one or two such coins in Britain. There are good reasons therefore for not choosing the year 42.

Further discussion may be advanced. Not only is there the problem of why our hypothetical trader, hot-foot from the continent, brought such a coin with him, there is the problem of why such a coin was produced.

Claudian copies are at present thought of as a response to a need. This is a view shared by many in Britain and elsewhere, but not by all. Some three specialists concerned strongly favour a late Claudian or early Neronian date at the earliest for the sherd of *terra sigillata* and Dr Reece argues vigorously for the loss of the Claudian copy occurring between 45 and 65. Caution dictates that the eve of the conquest cannot be ruled out as the background to the construction of the Gryme's Dyke but the weight of evidence points compellingly to a later date. If a historical context must be sought to explain its erection, then the Boudican revolt is the most obvious answer. Soon after the rebellion, it appears that the Roman colony which had up to that time been undefended was provided with its wall (CAR 6, 62-4). Gryme's Dyke is explicable as the native equivalent of this, designed to protect the area of native Camulodunum.

I suppose, since we are exploring the lunatic fringes of the subject, we ought to construct a final model by which a

regular coin came to the British court of Camulodunum to be copied by the frenetically forging British moneyers. Copying of coinage was not, at the time, a Roman habit, but it fits well in the immediate pre-conquest period in Britain where a great proportion of British site-finds in gold and silver are either plated or debased. My only problem remaining before I take advantage of this fact is a considerable difficulty in explaining why the British copier copied the original neither on the Roman standard, it is too light, nor on his own bronze standard, it is far too heavy.

The discerning reader may have noted a hint of irony in my attempt to put the case as openly as possible. Let me dispel this unjust suspicion. This is a copy of a coin of Claudius I (reigned 41-54) struck between 45 and 65 and probably lost before AD 70. •

Since the above was written, Robert Kenyon has completed his study of Claudian coins and has contributed the following note:

Though visibly worn, there is no doubt that this imitative Claudian as is not linked to the large group of die-related coins which were almost certainly minted at Colchester during the first years of the Roman occupation (Kenyon 1987 & 1992). Comparison of the weight and diameter of dies used to strike the coin (6.8g, 27 mm) with metrological data taken from imitative Claudian asses excavated at Colchester indicates that it would not have been struck before AD 47. Allowing for its evident wear from circulation, the coin would most likely have been deposited between AD 55 and 75.

### Conclusions

The finds from the features under the rampart can be separated in time from those in the rampart itself. The rarity at Sheepen of some of the forms represented under the rampart (ie Fig 6.6, nos 5, 10 & 11) and the high proportion of pure 'native' fabrics there, suggests a date for the group if not before, then early in Period 1 at Sheepen. Since the Cam 211 bowl is a developed example of its type, a date in the first quarter of the 1st century AD seems most likely.

The deposit Layer 8 at the western side of the bank does not appear to have been cut by the vague traces of a timber revetment visible above it and therefore may have been either a marking-out bank for the dyke or the bank of an earlier, smaller earthwork.

A case can be made that both the coin and the sherd of *terra sigillata* from the main body of the rampart were deposited before the Claudian invasion and that therefore Gryme's Dyke proper (as opposed to our putative 'Phase 1') was constructed in Britain AD 42-45. However, as of three specialists concerned strongly favour a late Claudian or early Neronian date at the earliest for the sherd of *terra sigillata* and Dr Reece argues vigorously for the loss of the Claudian copy occurring between 45 and 65. Caution dictates that the eve of the conquest cannot be ruled out as the background to the construction of the Gryme's Dyke but the weight of evidence points compellingly to a later date. If a historical context must be sought to explain its erection, then the Boudican revolt is the most obvious answer. Soon after the rebellion, it appears that the Roman colony which had up to that time been undefended was provided with its wall (CAR 6, 62-4). Gryme's Dyke is explicable as the native equivalent of this, designed to protect the area of native Camulodunum.

## 21

## Gryme's Dyke North

TL 96272368  
Excavation  
CFCH  
1956

Section: exact location uncertain.

## 22

## Watching brief near Dugard Avenue

TL 96332368  
Observation  
PC  
1977

Sterile trench dug during building works.

## 23

## Iron Age to ?early Roman fields north of Gosbecks ('Dugard Avenue excavations 1973-6')

TL 96502360  
Excavation  
PC  
1973-6  
Figs 6.7-6.10

The Dugard Avenue site lies on the northern fringe of the field system centred on Gosbecks. The excavation was primarily directed towards the examination of an area 100 x 5.5 m (18 by 328 ft) sited to cross Kidman's Dyke and two ditch systems (Ditches 2 and 3) visible as cropmarks (Figs 6.7-6.8). The rest of the work consisted of a series of machine-cut trenches dug to trace these features across the site and to establish the relationship of Kidman's Dyke North to Dugard Dyke, known from the earliest plans of the dyke systems but now not visible on the ground nor from the air. The trenching led to the discovery of Ditch 4 at the rear of Kidman's Dyke, and during the subsequent redevelopment of the site, two more ditches (Ditches 6 and 7) were observed in contractors' trenches (Fig 6.7). Although this combination of excavation, trenching by machine and observation of contractors' trenches has provided new information about the area, the results are piecemeal and consequently in some ways rather unsatisfactory. Nevertheless three principal conclusions have emerged. Firstly, it is clear in plan that all the ditches which were traced for any length, ie Ditches 2 and 3 and Dugard Dyke, were laid out after Kidman's Dyke North. Secondly, to judge by the associated finds, the ditch systems (at least Ditches 2 and 3) were in existence in the Late Iron Age to early Roman period; and thirdly, the ditch systems were probably concerned with the management of livestock. This last conclusion is indicated by the long straggling nature of Ditch 3 and the size and shape of the small enclosure formed by the northern end of Ditch 2. Neither of these is compatible with the square or rectilinear plots associated with cross-ploughing and cultivation generally.

Kidman's Dyke North was sectioned by hand where it crossed the area excavation and located by machine in five

other places. Ditches 2 and 3 had several branches which for ease of reference are described as Ditches 2a to 2d and 3a to 3c.

Ditch 2a was 5.5 m (18 ft) wide, 1.9 m (6.2 ft) deep and was sectioned slightly obliquely (Fig 6.9). Over two dozen sherds of Late Iron Age to Roman date were found in its fill. At the bottom of its silt was discovered part of a butt-beaker apparently of form Cam 115. No finds were found in Ditch 2b, the profile and dimensions of which were the same as Ditch 2a. The two branches, Ditches 2c and 2d, were the same shape in section as Ditches 2a and 2b but were of similar scale varying from two to three metres across. Nothing was found in Ditch 2c but from Ditch 2d was recovered a small quantity of pottery allowable as pre-AD 43. The forms present were Cam 21 (native platter), Cam 218 (native bowl: 2 vessels represented) and the cooking pot Cam 264, of which only a few were proved pre-AD 43 at Sheepen (Fig 6.10, nos 1, 2-3, & 4 resp). Also from here was a small vessel with no close Cam form (Fig 6.10, no 5). The predominance of 'native' ware, ie the thick brown soapy fabric, and the absence of ferra *sigillata* favour a pre-conquest date for the deposit although the Cam 264 pot hints at a later date. The west ends of Ditches 2a and 2c were so close to the eastern lip of the ditch of Kidman's Dyke that they must have been cut at least half way through the rampart of the dyke. The area enclosed by the ditches and the rampart must have been triangular in shape with sides about 25 m long. It would have made an effective animal pen. There was no material in the adjacent ditch of the dyke to indicate that the rampart had been demolished at this time.

Ditch 3 was narrower being at the base of the ploughsoil, 2 m (6.6 ft) wide and 0.75 m (2.5 ft) to 0.90 m (3.0 ft) deep (Fig 6.8). No finds were found in Ditches 3a and 3c, but in Ditch 3b were 28 Late Iron Age to early Roman sherds plus some fragments of a platter and a broken but nearly complete beaker, both of unusual type. Miss V Rigby has kindly prepared a report on these last two vessels (see below). She regards the platter as being the later of the two vessels and dates it to probably c 60-85. In the most northerly of the machine-dug trenches across Ditch 3a, three ditch-like features were observed in section, all of which could be of natural origin and none of which could be identifiable as Ditch 3.

In two of the machine-dug trenches across Kidman's Dyke North, two small ditches were observed to the east and south of the dyke. The eastern trench revealed a ditch 3 m (9.8 ft) wide and 1.4 m (4.6 ft) deep whereas the western trench showed a ditch only 1.8 m (5.9 ft) wide. These have been assumed to be the same feature and have been termed Ditch 4 (Fig 6.7). If this assumption is correct, then Ditch 4 would have run along the inner edge of the rampart of Kidman's Dyke if this existed when the ditch was dug.

Dugard Dyke (called Ditch 5 here) was sectioned across its width in two places and also along the southern part of its length to establish where it ended. At the base of the modern ploughsoil, the ditch was 5 m (16.4 ft) across and 1.8 m (5.9 ft) deep (Fig 6.9). The ditch stopped 5 m (16.4 ft) short of the ditch of Kidman's Dyke.

Ditches 6 and 7 were observed in the section of a trench dug for an electricity cable in 1977 and were 1.7 m (5.6 ft) and 5 m (9.8 ft) wide respectively. Their depths are unknown.

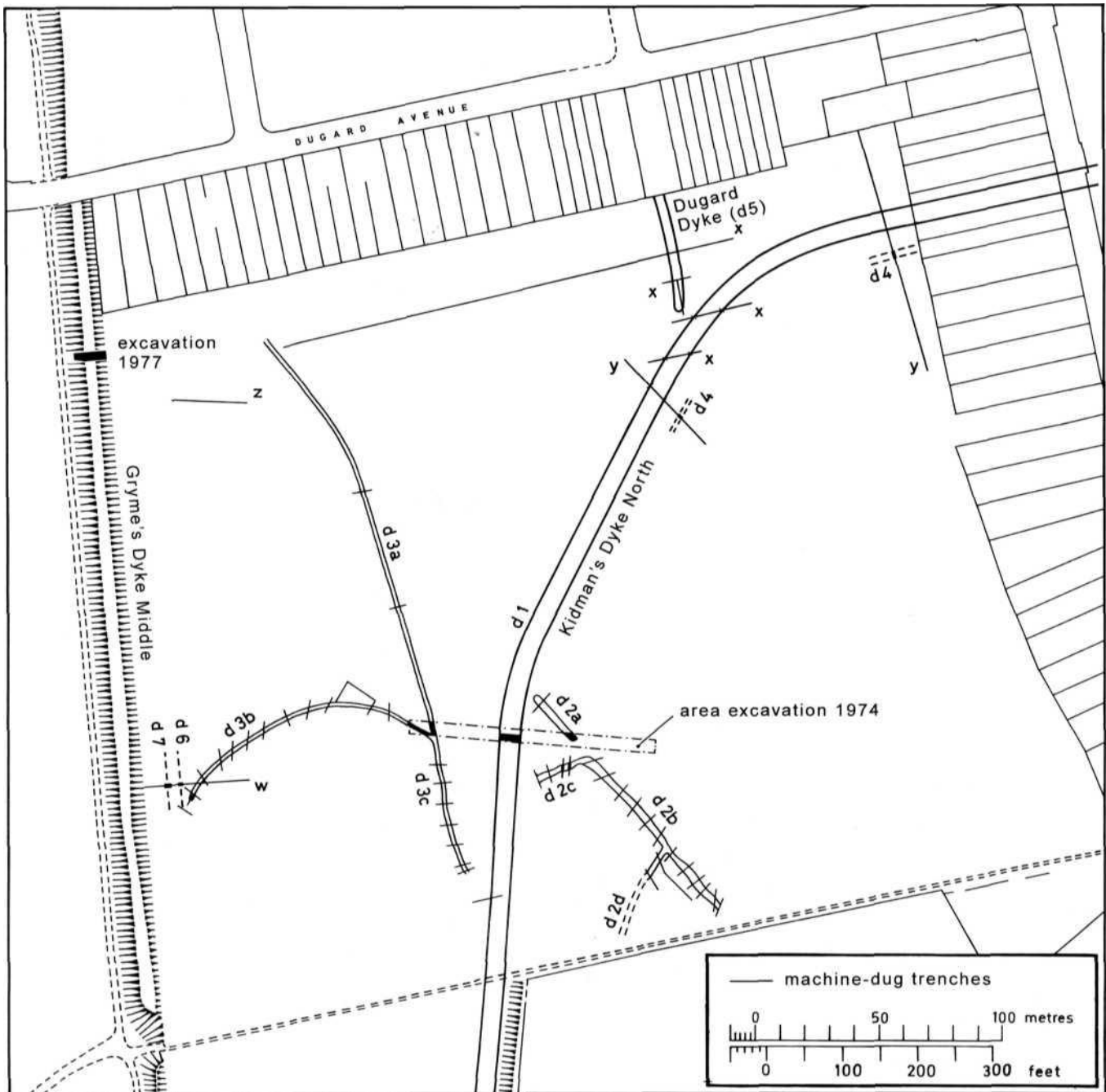


Fig 6.7 Dugard Avenue excavations 1973-6 (no 23). General site plan. 1:2500. [Pages 116-119]

*The embossed beaker and grey-ware bowl from Ditch 3b*  
by Valery Rigby

A barrel-shaped beaker (Fig 6.10, no 6), similar in shape, rim-form and arrangement of cordons to Cam 116a. The fabric is tempered with a mixture of grog and iron grits up to 1 mm in length. It was fired to a comparatively low temperature under oxidising conditions to produce a rich brown core (2.5YR 5/6), and brownish-red surfaces (2.5YR 5/8). Two zones of decoration, separated by burnished cordons, are comprised of bands of rouletting resembling fernleaf stamps, with triangular knobs applied at intervals.

As it stands, this beaker is unique, although the components of the decoration can be separately identified

on a number of British sites. The fernleaf rouletting, on beakers in similar locally-made, grog-tempered wares, has been found at Sheepen, Braughing-Puckeridge and the King Harry Lane cemetery, St Albans, Hertfordshire. Beakers with round, tear-drop or triangular knobs have been identified at Verulamium and in excavations of 1972 at Braughing-Puckeridge, probably from the same 'local' source (Frere 1972, fig 107, 252; Partridge 1977); the King Harry Lane Cemetery, Dorchester-on-Thames, and Silchester, possibly imported from the same continental source (Stead & Rigby 1989, fig 153; Frere 1962, fig 12, g; May 1916, pi 30, 150); Abingdon, Oxfordshire, in coarse-grained sandy wares characteristic of the Oxfordshire Romano-British potteries, so presumably locally made (Parrington 1978, fig 53, 373-4; information



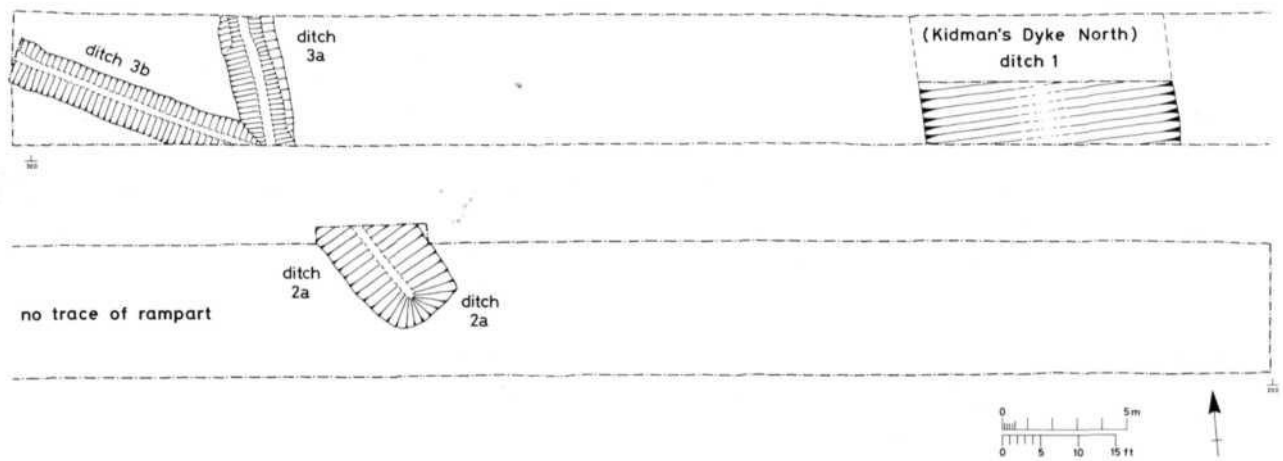


Fig 6.8 Dugard Avenue excavations 1973-6 (no 23). Plan of the area excavation. 1:300 [Pages 116-119]

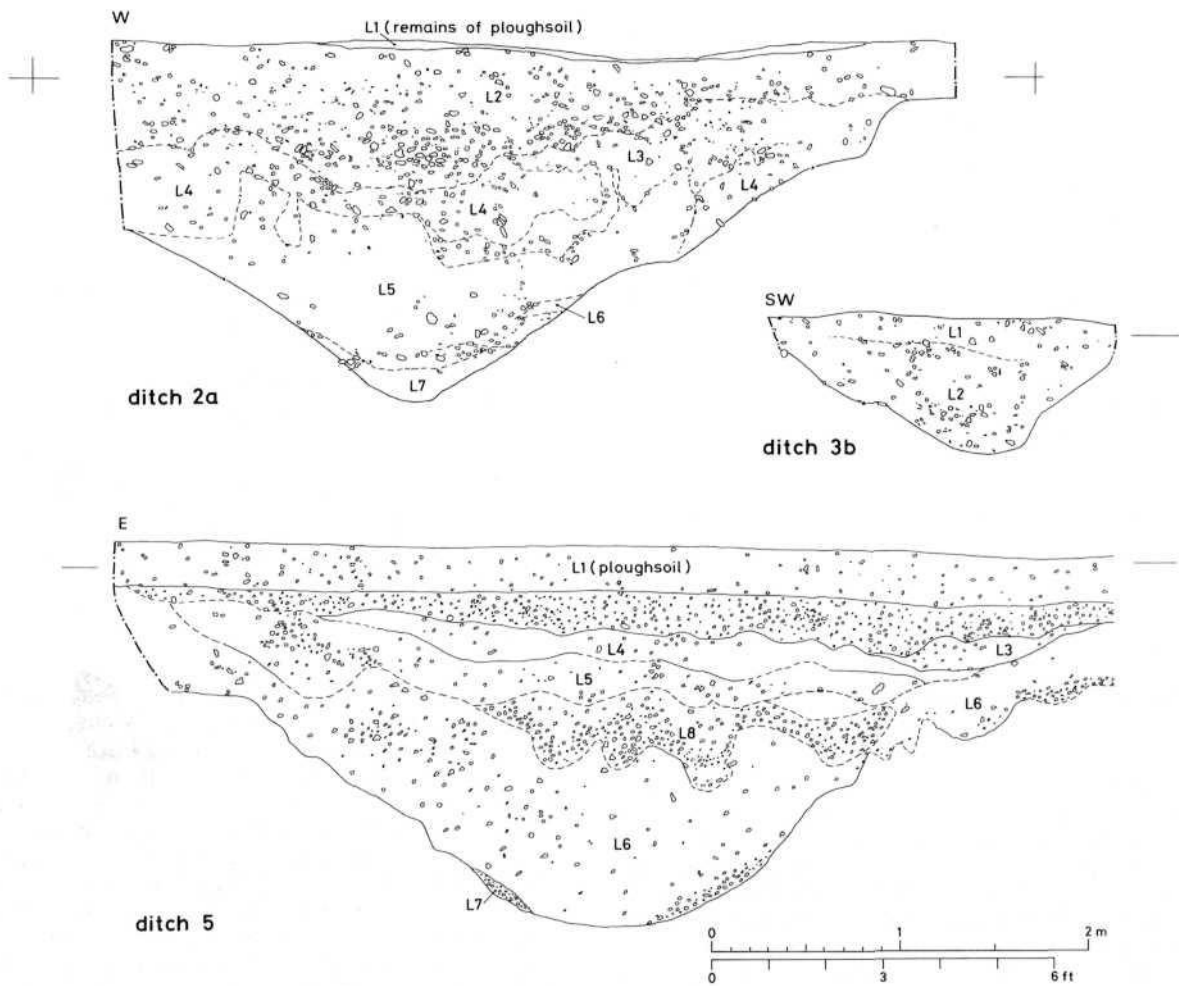


Fig 6.9 Dugard Avenue excavations 1973-6 (no 23). Sections. 1:40. [Pages 116-119]



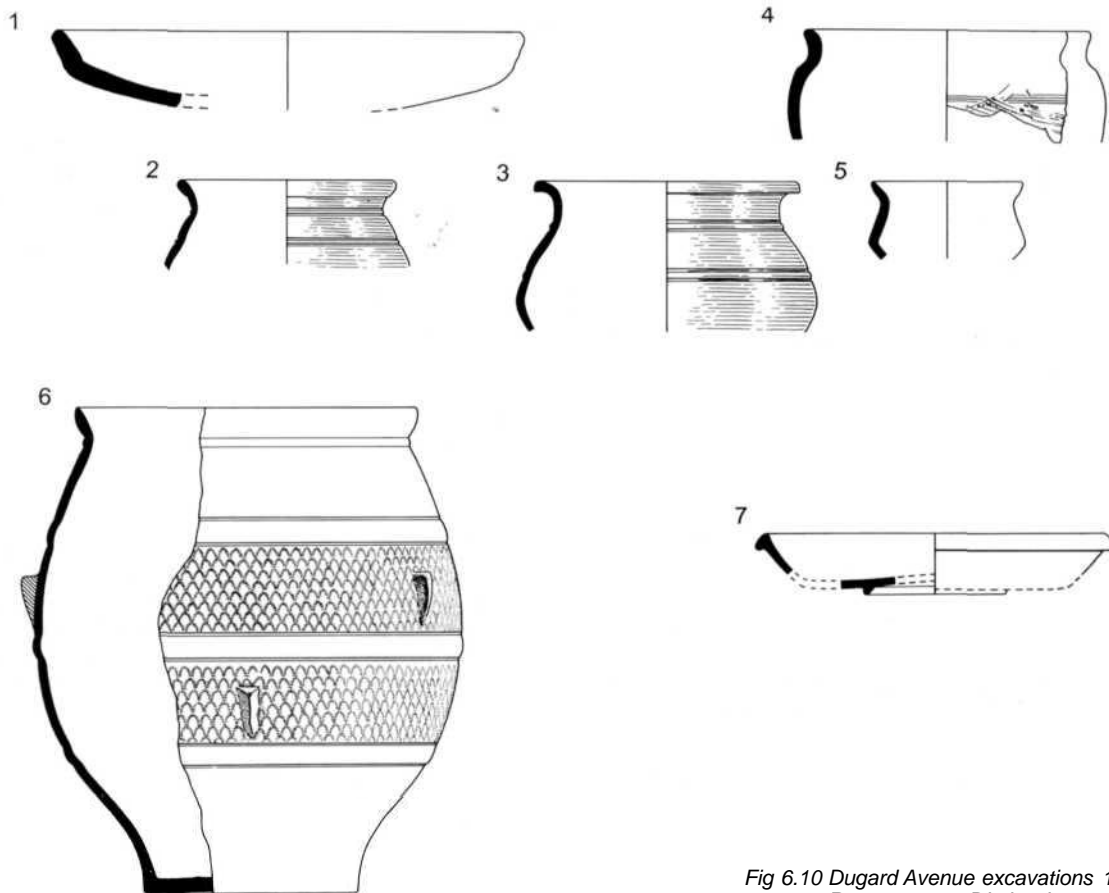


Fig 6.10 Dugard Avenue excavations 1973-6 (no 23).  
Pottery. 1-5... Ditch 2d; 6-7... Ditch 3b. 1:4.  
[Pages 117-119]

from R Thomas); two beakers, in burials, found in the Cheriton cemetery, from a fourth and probably local Kentish source (Tester & Bing 1949, fig 2, 19; 4, 33). The fabric of the Lexden example differs from all these examples, and should fall within the classification of grog-tempered ware and hence be locally made.

The earliest examples of the fernleaf rouletting occur on *oppidum* sites of Central Gaul, like Mont Beuvray (Dechelette 1914, fig 679, 10). It occurs frequently on the earliest versions of the Gallo-Belgic butt-beakers, Cam 112, in TR 3, imported in the late Augustan period, c 10 BC-AD 10. Otherwise typical butt-beakers, with applied knobs on plain rouletting, in TR 3 and TN, are common on sites in northern France, Belgium and the Netherlands, particularly the territory of the Atrebates, where they appear to be in the general repertoire. Such beakers are known to have been made at Arras, Pas de Calais and Hesbaye, Belgium.

A single row of triangular knobs occur on barrel-shaped jars in red ware, found in rich burials at Goeblingen-Nospelt, Luxemburg, so that this motif was clearly well established in the late Augustan period. Its date of manufacture should lie between AD 10 and 50.

The Lexden beaker appears to be a hybrid combining two, usually distinct decorative traits of the Gallo-Belgic potters working in the late Augustan-Tiberian period.

The rim sherds 40, 49 and 50, and the bases 41, 45 and 47, are all from the same platter, which has a narrow wedge-shaped rim and a shallow footring (Fig 6.10, no 7). The form cannot be identified in the standard typology for Camulodunum, so may not have been found there before. In form, it appears to be related to a series of platters from the Flavian occupation of the Bad Nauheim fort, while, in addition, there is another from Cemetery S at Nijmegen, Holwerda type 81 i (Simon 1960, abb 12, 10-15; Holwerda 1941, no 1220). A reappraisal of the Nijmegen Cemeteries O, E, and S suggests that there was much more overlap in their dates than was previously thought, but that Cemetery S has a much higher proportion of Nero-Flavian finds, indicating much greater use in the later period (Stuart 1979, 288-91). The fabric suggests that the platter could have been made locally, rather than being imported, in the period AD 60-85. •

## 24

### Enclosure ditches inside Gryme's Dyke

TL 96302353  
Observation  
H Brooks  
1977

Two ditches (Ditches 6 and 7, Fig 6.7, Trench W) were observed in a shallow trench dug to lay an electric cable.

Ditch 6 was 1.7 m wide and ditch 7 was 3 m wide. Their depths could not be established since the trench was not deep enough.

25

Kidman's Dyke North

TL 96432338  
Excavation  
1950s  
AFH

Two sections in Field 1338 across Kidman's Dyke North: exact locations unknown.

26

Kidman's Dyke North and Heath Farm Dyke Middle

TL 96432325  
Excavation  
AFH  
1950s

As above but section across Kidman's Dyke North and Heath Farm Dyke Middle: exact locations not known.

27

Kidman's Dyke

TL 96442329  
Excavation  
PC  
1978  
Fig 6.11

Section (Fig 6.11) dug by machine across top of ditch to establish its exact western limit so that the dyke could be preserved despite adjacent building works then in progress.

28

Kidman's Dyke and Heath Farm Dyke North

TL 96422324  
Excavation  
CFCH  
1957

Section through upstanding part: no more details.

29

Heath Farm Dyke

TL 96412318  
Observation  
J Sewter  
1974

Although now obscured, the ditch of the dyke had been visible for many years in the side of Shrub End gravel pit. In 1974 the ditch was cleared and photographed as well as the difficult circumstances would allow. It was distinctly V-shaped and was 2.4 m deep and 7.8 m wide. The ditch had been backfilled with reddish brown sand and gravel after having silted to a depth of about 0.4 m with brown sandy loam.

30

Kidman's Dyke and Heath Farm Dyke North

TL 96382318  
Observation  
H Laver  
c 1900  
Laver 1902, 370-71

A profile was drawn across both dykes and subsequently published.

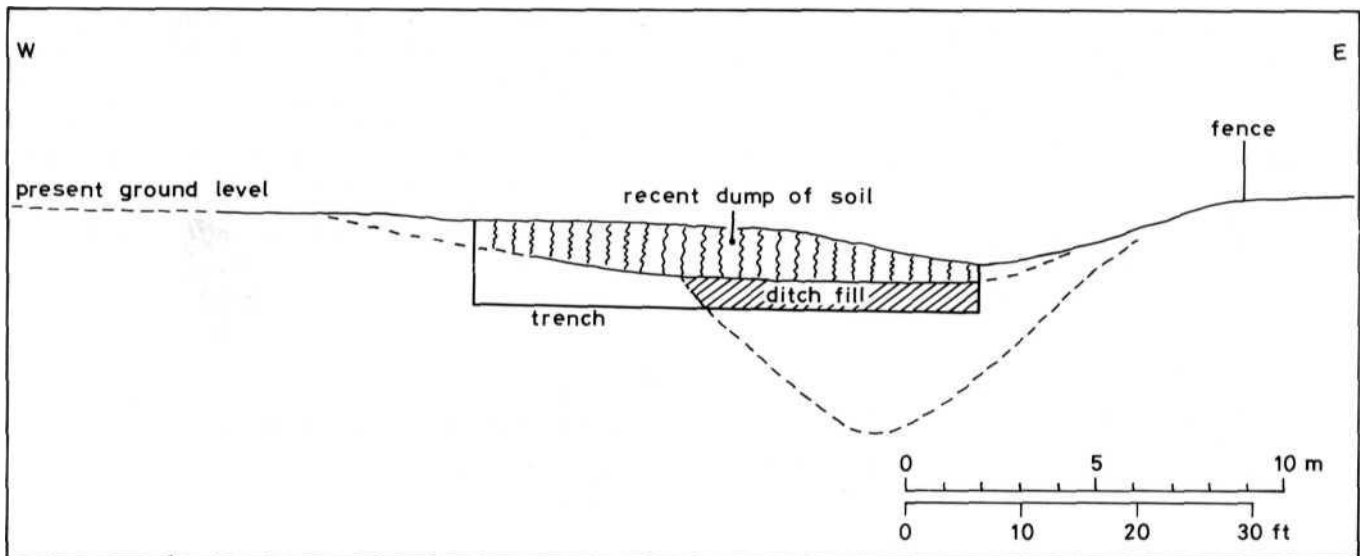


Fig 6.11 Kidman's Dyke (no 27). 1:200.

31

## Heath Farm Dyke and Kidman's Dyke

TL 96412316  
Observation  
J Sewter  
1974

The ditches of both dykes were observed in the section of a trench dug for a pipe in 1974. The ditches were cut obliquely and not sectioned to their full depth. The most useful observation was the possible base of the bank of Heath Farm Dyke. This appeared to survive to a height of 0.5 m and was about 7 m wide.

32

## Kidman's Dyke

TL 96312310  
Observation  
PC & N Smith

The ditch was sectioned by machine by the Colchester Borough Council to establish its exact position and thereby ensure its preservation after the laying of a new sewer system. The ditch here lay at the foot of a natural valley and hill-wash down the slopes made it difficult to estimate the true width of the ditch. However this seemed to be about 8 m. The ditch was not sectioned to its full depth.

33

## Gryme's Dyke North: small southern extension east of Stanway Green

TL 96312330  
Excavation  
CFCH  
1957

No details are available.

34

## Heath Farm Dyke Middle

TL 96692348  
Excavation  
CFCH  
1961

Section in field 1339. See page 31.

35

## Shrub End Dyke

TL 96732362  
Observation  
B P Blake  
1963

Section drawing in CM and also CFCH archive

Dyke sectioned in a service trench dug during the building of the Mormon Church, Straight Road. Section shows ditch

6 ft 6 in (2.0 m) at top but bottom not exposed (p 31).

36

## Heath Farm Dyke Middle

TL 97022376  
Excavation  
CFCH  
1957

Two sections were dug across and entrance. See page 31.

37

## Junction of Prettygate Dyke and Shrub End Dyke

TL 96692386  
Excavation  
MRH  
1936

The so-called 'Peartree Junction' excavation. See pages 46-8 and *Rom Col*, 4-5.

38

## Gosbecks cropmarks

TL 97002290  
Excavation  
S Benfield and H Brooks  
1994  
Benfield 1994; Benfield & Brooks 1994

Mainly machine-based evaluation of cropmarks prior to the redevelopment of the site.

39

## Gosbecks-Colchester Roman road

TL 97312275  
Excavation  
C Crossan  
1989

Small trenches were dug by hand along the line of a new road bordering the south side of a residential development. The largest of the trenches was an unsatisfactory section across the Gosbecks-Colchester Roman road where the ditches and road metalling were hard to detect.

40

## Gosbecks: the large walled enclosure

TL 97002260  
Excavation  
Roman Essex Society  
1948

*Rom Col*, 264-7

Walls traced, 'exedrae' discovered.

---

**41a**

Gosbecks theatre

TL 96822233  
Excavation  
MRH & Roman Essex Society  
1948-9  
*Rom Col*, 267-9

Mound excavated and found to be the remains of a theatre.

---

**41b**

Gosbecks theatre

TL 96822233  
Excavation  
BRKD  
1967  
B R K Dunnett, 'The excavation of the Roman theatre at Gosbecks', in *Britannia*, 2 (1971), 27-47

Partial excavation of the western half of the *cavea* and orchestra.

---

**41c**

Gosbecks theatre

TL 96822233  
Excavation  
N A Smith  
MS *The preservation of the Gosbecks site, Colchester, Essex 1979*, archive report in the CM

Trench across *cavea* mound to examine plough damage. See 43c below.

---

**42a & b**

Gosbecks enclosures

TL 96682249 & TL 96682254  
Excavation  
MRH & Lt Col R J Appleby  
1949  
*Rom Col*, 270-71 & *CMR* 1948-50, 134

The ditches of the principal enclosure and a subsidiary one both sectioned. See pages 97-8.

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**43a**

Gosbecks temple and portico

TL 96802255  
Excavation  
Rev H Jenkins  
1842  
Jenkins 1846; *Rom Col*, 261-2; Roach Smith 1852, 41-2  
Romano-Celtic temple and surrounding portico examined in part and believed at the time to be the remains of a Roman villa. Wall of outer enclosure partly traced.

---

**43b**

Gosbecks: temple, portico, and enclosure ditch

TL 96802255  
Excavation  
MRH  
1936  
*Rom Col*, 261-4

Romano-Celtic temple, portico, and earlier enclosure ditch trenched obliquely.

---

**43c**

Gosbecks: portico (and theatre *cavea*)

TL 96802255  
Excavation  
N A Smith  
1977  
Fig 6.12; see also pages 101-3 and esp Fig 6.12, p 123  
MS *The preservation of the Gosbecks site, Colchester, Essex, 1979*, archive report in the CM

In September 1977, as part of an investigation to assess the condition of the surviving archaeological remains at Gosbecks, the ploughsoil was stripped from an area 30 x 5 m across the south side of the temple portico. This exposed the outer lip of the enclosure ditch, the robber trenches of the foundations of the three portico walls and a gravel layer between and to the south of the portico (Fig 6.12). The foundations had all been robbed but for one small part of the central foundation which consisted of large pieces of septaria in weak gravelly mortar. The distance between the inner and central foundation was 4.2 m and that between the outer and central foundation was 4.0 m. The three robber trenches are of different proportions but the mortar adhering to the sides and bottom show that they accurately reflect the dimensions of the foundations. The inner foundation is 1.2 m wide and 0.6 m deep, the central foundation 1.1m and 1.0 m deep and the outer foundation 0.1 m and 0.7 m. Below 0.6 m the central foundation is stepped in by 0.2 m along its southern side. The differences between the sizes of the foundations may indicate that they had carried different loads, or even that there had been more than one period of construction. South of the outer wall was a layer of gravel at least 0.15 m thick. It continued northwards from the outer foundation but became progressively thinner so that north of the central foundation only one small patch of it survived. Its erosion was due to ploughing, which had cut down into the natural cover loam that underlay the gravel. Nowhere did it have a well-made surface. The gravel may have been the remains of an external bank later levelled for the construction of the portico. The few sherds from the gravel layer probably date from 10/15 BC to c AD 40 and compare closely with material from Sheepen.

As part of the same investigation the site of the theatre was examined. The ploughsoil was stripped from an area 5 x 30 m across the highest part of the western half of the *cavea*. Within this area a trench 1 x 2 m was dug through the *cavea* to the pre-*cavea* ground level to determine the present height of the mound. Beneath the ploughsoil the *cavea* still survived to a height of 1.0 m above the old ground surface. This indicated that as much as 0.2 m of material may have been eroded from the crest of the mound since Hull's excavations of 1948 and 1949 (*Rom Col*, 267-9 and site drawings in Colchester Museum). The

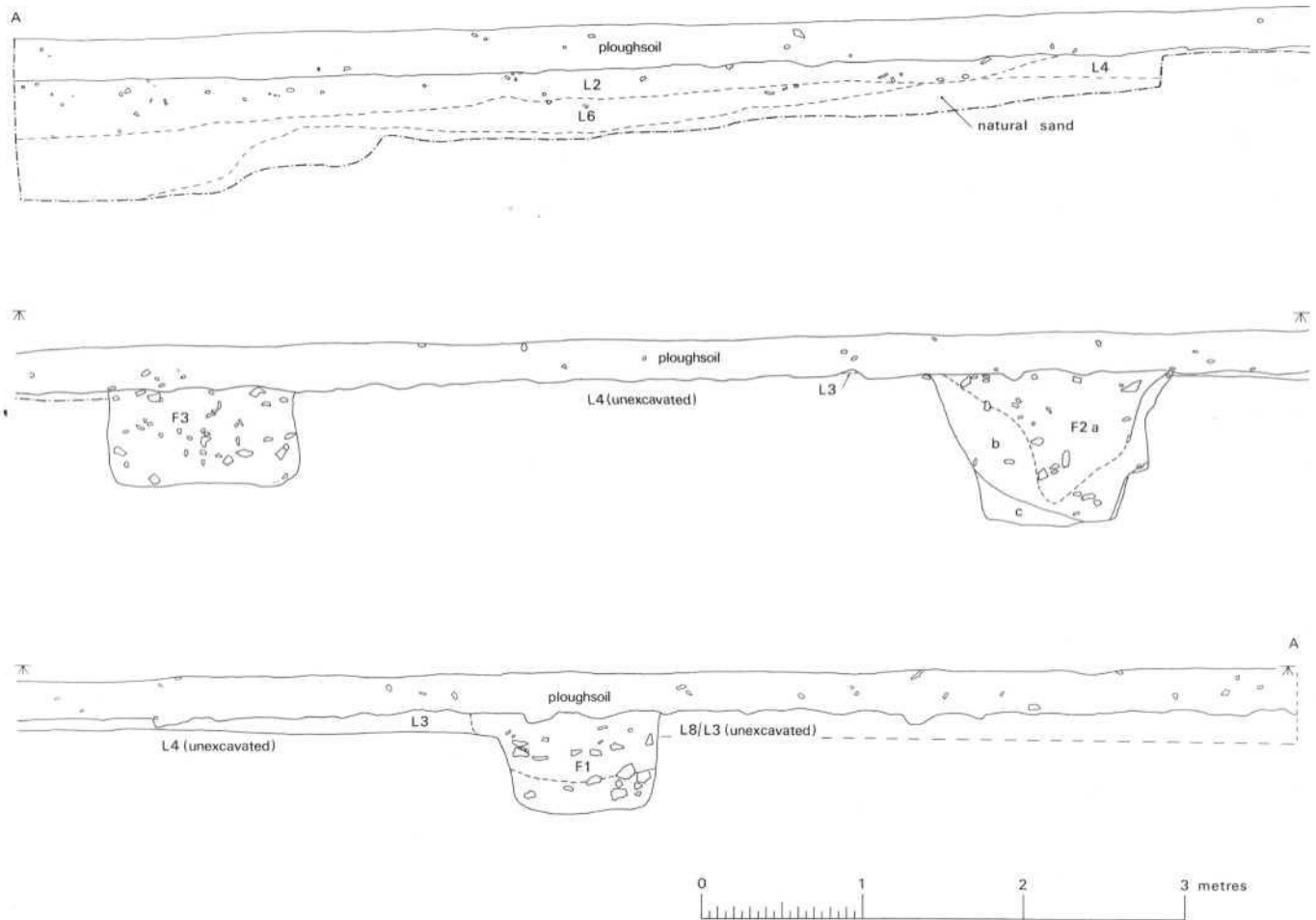


Fig 6.12 Gosbecks portico (no 43c). Section. 1:45. [Page 122]

*cavea* seems to have been built of blocks of cover loam from which most of the surface vegetation had been removed rather than of turf as had been suggested as a result of previous excavations (*Rom Col*, 267-9; Dunnett 1971b).

45

Gosbecks

TL 96482266  
Excavation  
Roman Essex Society  
1948  
*Rom Col*, 261, fig 113, pt 'A'

Small excavation. Nothing found except a few parallel grooves which were thought to be the result of ploughing.

44

Gosbecks

TL 96512257  
Excavation  
Roman Essex Society  
*Rom Col*, 269-70, fig 113, pt 'B'  
1948

Trial trench near the stream where a 'kiln of some sort was found, built of tiles'. Few sherds of pottery found in the area, all probably 2nd century.

46

Gosbecks

TL 96472250  
Excavation  
Roman Essex Society  
1948  
259, fig 113, pt 'C'

Search made in the area where in 1943 1st- to 2nd-century burials had been exposed during ploughing. Nothing found.

**47**

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Gosbecks (hearth)

TL 96452248  
Excavation  
Roman Essex Society  
1948  
*Rom Col*, 259, fig 113, pt 'D'

Hearth found over a small pit of Sheepen Period 1. Small excavation again.

**48**

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Gosbecks (coin hoard)

TL 96462179  
Excavation  
G M R Davies  
1983  
Davies 1983; Bland, R F, & Carradice, I A, 1986

Hoards dating to c 270-275 of over 6,000 *antoniniani* discovered in three pots.

**49**

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Gosbecks Dyke South

TL 96392183  
Observation  
H Laver  
1902  
Laver 1902

**50**

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Funerary enclosures, Stanway

TL 95702260  
Excavation  
1987-92  
PC, D Shimmin, and C Crossan

Excavation of a series of Late Iron Age and early Roman funerary enclosures in advance of gravel quarrying (pp 169-70).

**51a, b & c**

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Layer(-de-la-Haye) Dyke

TL 96452050  
Excavation  
B P Blake  
1961  
CMR 1962-3, 7-8

Three sections cut across dyke. Results not very clear (p 34). Ditch of dyke difficult to distinguish from recent field ditch(es).

**52**

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Lexden Mount

TL 96862486  
Excavation  
H Laver & F W Reader  
1910  
Laver & Reader 1913

A barrow, containing fragments of Roman tile, somewhat crudely excavated. Nothing found *in situ* in the centre but perhaps robbed out. Flattening of the top of the barrow was evidently done later.

**53**

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Lexden Dyke

TL 97322481  
Excavation  
CFCH with T Cruso  
1932

Sections across entrance and elsewhere within the former Lexden Park. See pages 35-45.

**54**

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Junction of Prettygate Dyke, Heath Farm Dyke  
North and Lexden Dyke

TL 97642428  
Excavation  
AFH/CFCH  
1943/1956-7

'Prettygate Junction' excavations. See pages 48-50.

**55**

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?Road or trackway, playing field of the Colchester  
Royal Grammar School

TL 98252448  
Observation  
CM OS 6" map & *Rom Col*, pi 43

Cropmarks including ?road or trackway in playing field of the Royal Grammar School.

**56**

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Excavations near Altnacealgach, 1956

TL 98122463  
[Figs 6.13-6.15]  
*by John Wachter*

During May 1956 a field to the south of Altnacealgach House (Fig 6.13) was investigated by H M Ministry of Works. Time only permitted a limited exploration, which consisted of a line of trenches, 3 ft wide and 18 ft long, with 2 ft baulks in between each trench, running north-south across the field. Further cuttings were then dug to follow up the few features found. The natural undisturbed surface

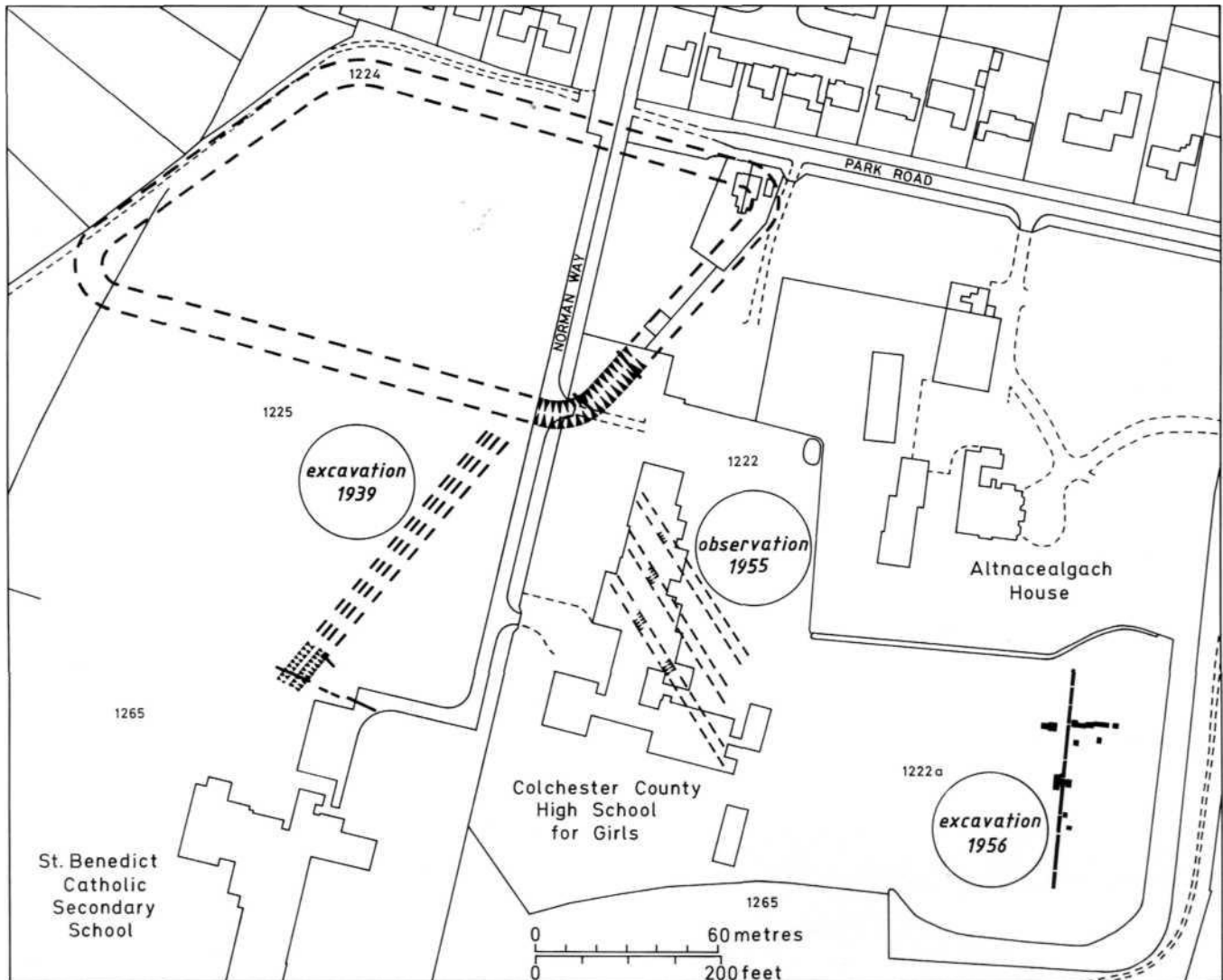


Fig 6.13 Altnacealgach House, the Colchester County High School for Girls, and the surrounding area. 1:2250. [Pages 124-7]

was found at approximately 2 feet below the modern ground level. It rose slightly to the south, and was composed of orange sandy gravel with pockets of clean orange sand. In places it was capped by dirty gravel, seemingly of less natural origin.

The few features found can be briefly summarised as follows:

#### Palisade trench (Figs 6.14-6.15)

Running east-west across the north end of Trench 4 was a palisade trench. It had been cut into the natural gravel and was approximately 2 ft deep and 4 ft wide at the top. The bottom was cut square, 20 ins deep and as many wide. Apart from three related post holes the bottom was uniformly flat, showing that the palisade was carried on a sleeper beam. A length of nearly 80 ft was uncovered,

without sign of a corner or end. In Trench 10 there were two offsets to the south, which were not, however, picked up in Trenches 12 and 13. If they were, as is reasonable to suppose, intended for buttresses, then the palisade was probably facing to the north.

A disturbance to the south in the same trench was at first thought to be a parallel palisade. It was however less well defined. A similar, but even less convincing disturbance, appeared in Trench 13 but was not seen in Trench 12.

#### Wattle-and-daub structure (Fig 6.14)

In Trench 5 the thin line of a rather irregular wattle-and-daub structure was observed. This extended into Trench 14. At one point were a number of blocks of septaria, Roman brick, and shelly limestone, possibly used to support a weak point in the walls. Little evidence for dating was forthcoming, although lying amongst the blocks was a large copper-alloy needle.

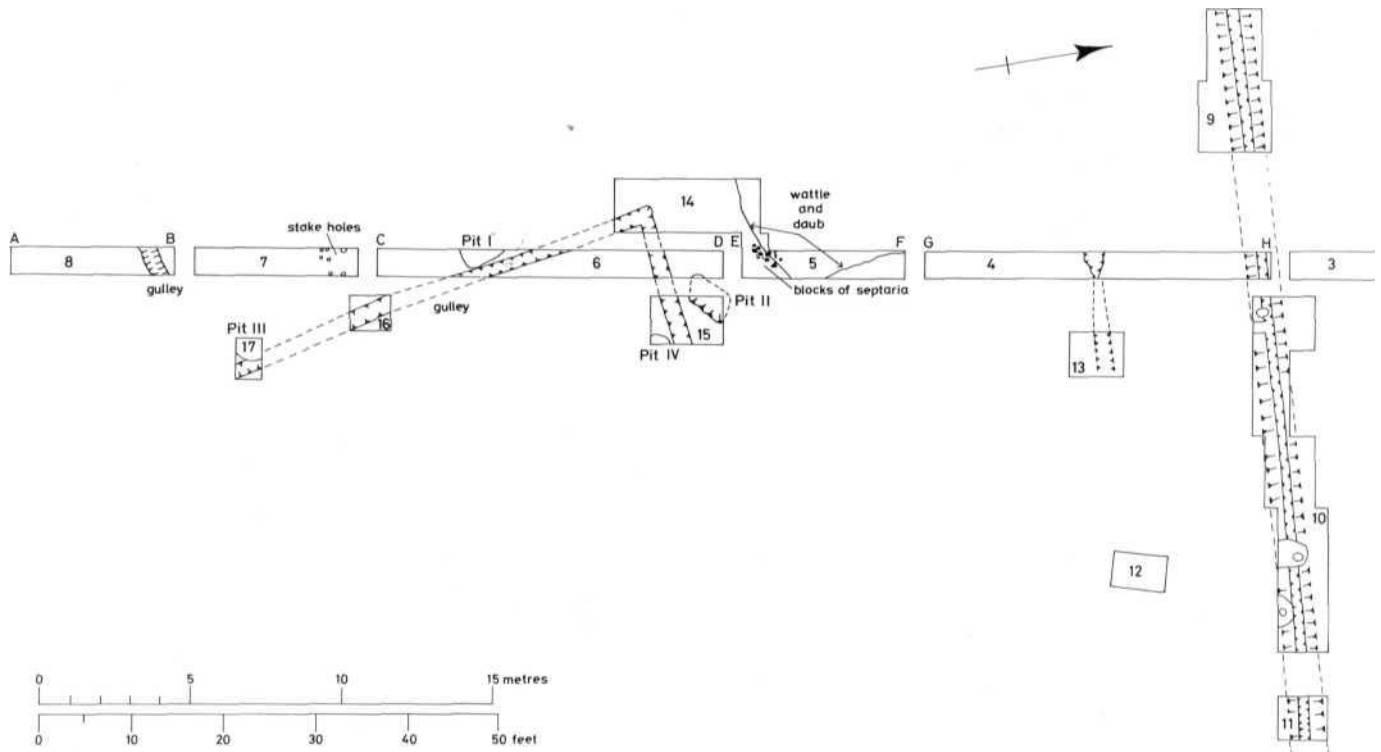


Fig 6.14 Altnacealgach House 1956 (no 56). Plan. 1:250. [Pages 124-6]



Fig 6.15 Altnacealgach House 1956 (no 56). The palisade trench viewed from the east. [Page 125]

The gullies and pits (Fig 6.14)

Two gullies were found during the course of the excavations; one in Trench 6 and the other in Trench 8. That in Trench 6 turned through a right-angled corner in Trench 14, and was also picked up in Trenches 15, 16 and 17. It was roughly semicircular in cross-section and could have been a sleeper beam trench. The filling contained a little pottery and a few nails. In Trench 17 it was found to be cut by Pit III, which is dated to the Antonine period. It must therefore have been earlier than this date. The gully in Trench 8 was V-shaped in section and the filling was archaeologically sterile.

Pit I. Roughly rectangular in shape. Shallow with a small scoop at the bottom. The filling was sterile.

Pit II. Dug in the shape of a rhomboid, it was filled with a light earthy loam, which dried out almost white, and was completely different to anything else on the site. The filling was again sterile.

Pit III. Circular pit in Trench 17. Is later than the gully which it cuts. Contained large amounts of pottery of Antonine date, as well as oyster shells and charcoal.

Conclusion

The site appears to have been occupied only sparsely for comparatively short times during the Roman period, with what structures there were being entirely of wood. It would probably be related to an agricultural economy and might represent the outbuildings of a villa. Certainly the contents of Pit III would seem to suggest a more intensely-occupied site in the near vicinity. •



57

## A triple dyke, Colchester County High School for Girls

TL 97982468  
 Observation  
 R J Martin  
 1955  
 Fig 6.16

A 'triple dyke' was discovered and recorded by Mr R J Martin, the acting clerk of works, during the building in 1955 of the Colchester County High School for Girls in Norman Way. A plan was made (Fig 6.16) but no sections were drawn; notes were written by Hull following his visits to the works. The ditches were not cut through from top to bottom and therefore their depths are unknown. As exposed on the surface they were on average 13 ft (4.0 m) wide and as a group 84 ft (25.5 m) across, 41 ft (12.5 m) less than the Triple Dyke to the west. A small quantity of 2nd-century pottery was found on the site but since this was mainly in the spoil-heaps, none need have come from the ditches. At the south end of the site (Points A and B, Fig 6.16), post holes were observed in section. These were about 6 inches (0.15 m) across, 18 inches (0.45 m) deep and about 18 inches apart centre to centre. Hull wrote 'about 18 inches outside the end of their line there was a small ditch. The appearance presented was that of a building with wooden posts surrounded by a diagonal ditch. The pottery was again 2nd century' (MS *Colchester outside the walls* in CM). At Points C and D (Fig 6.16) was found a 2nd-century pit up to 14 ft (4.3 m) deep. The structural remains may be associated with those subsequently found nearby by John Wachter (pp 124-6).

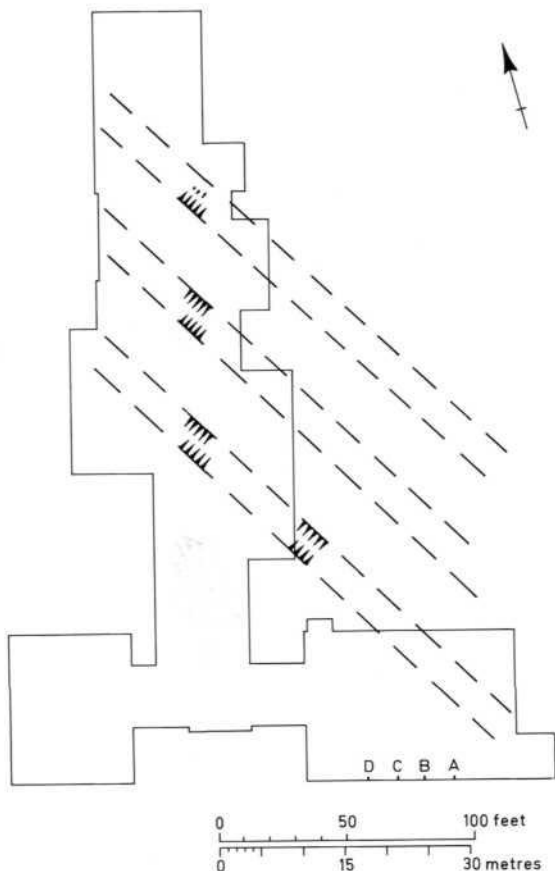


Fig 6.16 Colchester County High School for Girls (no 57). Plan. 1:900.

58a &amp; b

## Possible early Roman enclosure, Altnacealgach

TL 97862465 & TL 97962474  
 Excavation  
 MRH  
 1939  
 Report, plan & sections in *Rom Col*, 271-3

Short trenches dug in the grounds of Altnacealgach. See pages 62-3.

59

## Roman road, Fitzwalter Road

TL 97602479  
 Observation  
 P Laver  
 1923  
*Rom Col*, 13

Observed in sewer trench in Fitzwalter Road. Exact direction unknown.

60a

## Lexden Tumulus

TL 97532473  
 Excavation  
 P & H E Laver  
 1924  
 Laver 1927; Foster 1986

60b

## Lexden Tumulus excavation, 1973

TL 97532473  
 Excavation  
 PC  
 1973  
 Figs 6.17-6.18

*Introduction*

The Lexden Tumulus was partly excavated in 1924 by P G Laver and his brother H E Laver prior to building operations in the vicinity of the monument. Their work consisted of the discovery and excavation of the central grave, with two extensions in different directions to locate the ditches encircling the mound (Laver 1927). When in 1973 a planning application was submitted to the local planning authority for permission to construct a bungalow against the western side of the tumulus, it was discovered that despite the work and careful records of 1924, it was not possible to locate the exact position of the barrow in relation to the modern boundaries. The mound is clearly visible but is low in profile and has no sharp break between the slopes of the sides and the ground level of the surrounding area. Consequently a small trench (Trench 1, Figs 6.17-6.18) was excavated during the spring of 1973, to locate the ditch and thus define the extent of the barrow so that it could be preserved. The archaeological potential of the area immediately around the monument was also

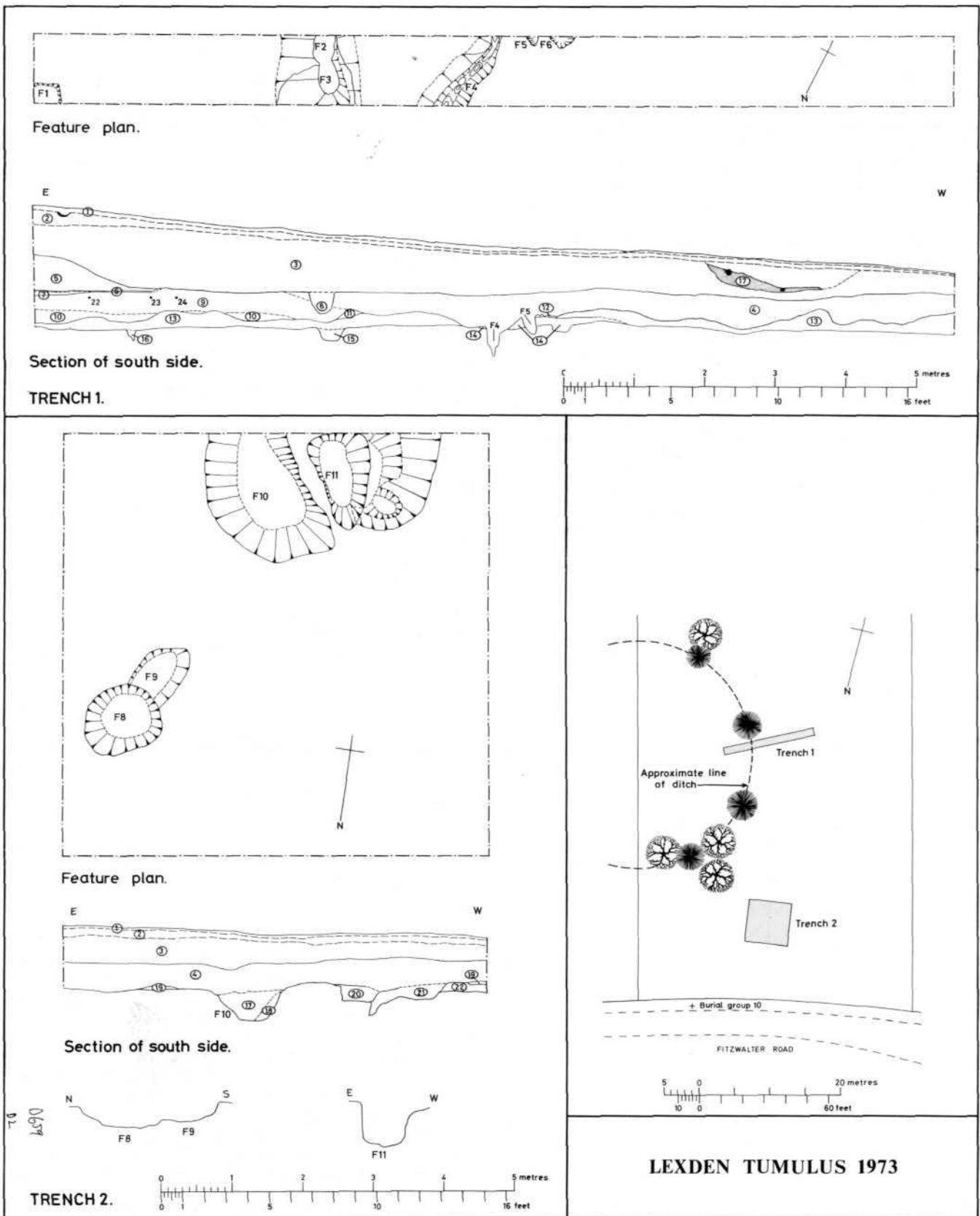


Fig 6.17 The Lexden Tumulus 1973 (no 60b). Plans and sections. [Pages 127-30]



Fig 6.18 The Lexden Tumulus 1973 (no 60b). Trench 1. Left: viewed from the west. Centre: the supposed barrow-ditch F2/F3. Right: irregularly-shaped slot F4.

not known and, to test this, a small area excavation was suggested under the site of the proposed bungalow. Unfortunately, because of the possibility of charges for reinstatement, this could not be carried out and instead a small trench was excavated just south of the threatened area (Trench 2, Fig 6.17).

The natural is sand and gravel and is sealed by a layer up to 50 cm deep of the so-called 'cover loam'. This is a soil which contains a high proportion of wind-borne silt and was formed under periglacial conditions during the Devensian glaciation. It has an irregular junction with the underlying sand and gravel and consequently is of varying depth. (Cover loam is described and discussed by the Soil Survey in Norfolk, in *Soils in Norfolk, No 2* (Soil Survey Records No 21).) Although the subsoil is different here from that at Lexden, the cover is substantially the same.

#### Trench 1

Of the original mound, all that was found *in situ* was some redeposited cover loam (Layer 5) at the eastern end of the trench. This proved, where exposed, to be 0.5 m at the thickest point and to seal a thin compressed turf-line (Layer 6) comprised of yellowish-brown silt loam containing flecks of charcoal. The turf-line was no doubt the same as that detected in 1924 (Laver 1927). The underlying cover loam has several different layers and features detectable in it, all of which were presumably formed at various stages during the Devensian. The upper layers in the cover loam (Layers 4, 7, 9 & 10) are yellowish-brown sandy silt loam. The lower layers (11, 12, 13, 14, 15 & 16) consist mostly of gravel and sand and are a result of a mixing of wind-borne silt with the underlying glacially-deposited sand and gravel.

In some cases the variations and features in the cover

loam were difficult to see in plan or in section and consequently difficulties were experienced during the excavation, especially with the supposed ditch F2/F3 (Fig 6.18). When excavated these two features seemed in plan to be shallow steep-sided pits or post holes set at the bottom of a small depression 0.3 m deep. In section however they appeared to be a composite of two features filled with Layers 8 and 15 respectively, Layer 11, and a slight change in cover loam as shown by the dashed line between Layers 4 and 9 in Figure 6.17. The feature filled by Layer 8 may be the only man-made feature of this complex, Layers 11 and 15 being periglacial formations.

Feature 4 was a long, steep-sided, irregular slot (Figs 6.17-6.18). The feature was undercut in places and contained small tunnels and pockets. It consequently may have been a periglacial feature, or, less likely, an animal burrow. F4 might have been dug to support posts or stakes but the tunnels and pockets make this unlikely. Similarly on excavation F6 appeared to be a possible post- or stake hole but in section seemed to be periglacial. F5 was more positive in section. It was similar in profile and fill to F4 and is likely therefore to share the same origin.

Layer 3 sealed the theoretical position of the barrow-ditch as indicated by the 1924 excavation and extended well beyond the presumed limit of the monument. Five finds were found distributed throughout the upper half of Layer 3 dating it to the 18th or 19th century. These consist of a fragment of clay pipe stem datable to 1680-1700 and four contemporary or later sherds.

Feature 1 was a vertical-sided feature cutting through Layers 3 and 5 and extending westwards and northwards into the baulks. F1 may have been part of a previous excavation. F7 was a shallow rubbish pit containing much debris of early 20th-century date.

### Trench 2

The sequence of layers in Trench 2 was the same as that found in Trench 1. Layer 3 decreased from 0.4 m to 0.2 m in thickness and sealed the cover loam (Fig 6.17; Layers 4, 19 & 22) which occurred up to 0.5 m thick.

Four features were found in Trench 2, ie F8, F9, F10 and F11. Although these could not be detected until after the removal of most of the cover loam, this need not necessarily imply that the features were sealed by it or were part of it. The profiles of F8, F9 and F10 in Figure 6.17 are as they appeared after the removal of the cover loam. F8, F9 and F10 were filled with yellowish-brown sandy silt loam which was very similar to the cover loam itself. F11, on the other hand, contained mostly yellowish-red gravelly sand and, of the four features, is the most likely to have been natural. The difficulty in locating these features in the cover loam was compounded in section (Fig 6.17) where the loam appears to seal F10 and F11. However a sherd of prehistoric pottery from F10 almost certainly indicates a man-made origin for the feature. The position of the sherd in the feature was such that it was probably too far down to be attributable to worm-action.

### The significant Roman and prehistoric finds

The finds of Roman and prehistoric date totalled no more than 30 pieces. The nature and location of the significant sherds for dating purposes can be summarised as follows.

Two sherds were found in Layer 8. These indicate a post-conquest date and consist of part of an abraded mortarium rim probably of form Cam 194 (*Camulodunum*, 254-6) and the base of a coarse ware pot of uncertain form. The mound itself (Layer 5) produced no finds at all but in the underlying cover loam, three small sherds were found. Sherd P22 is flint-gritted and similar to the Late Bronze Age material found at Kiln Road (pp 134-5). Sherd P23 is a fragment of briquetage and is unlikely therefore to predate the 1st century BC (Fawn *et al* 1990, 45). The third piece is a small reddish body sherd which has lost both its surfaces and is probably part of an amphora, in which case it similarly is unlikely to be earlier than the 1st century BC.

The depths of these pieces in the cover loam are shown in section in Figure 6.17 and their occurrence in the loam is explicable by worm-action. F8 contained two small, abraded, flint-gritted sherds of Bronze Age or Iron Age date. F10 contained a similar piece which is heavily gritted and more reminiscent of the Bronze Age material from Chitts Hill (Crummy 1979a). Also in F10 was a body sherd of sandy grey ware belonging to the Roman or the end of the Late Iron Age period.

### The contractor's excavations

The surface was lowered by machine to a depth of 0.5 m or more over some 500 square metres south of Trench 2 and west of the tumulus. No burials or significant features were observed. However a burial group was discovered during the laying of an electricity cable in the pavement to the north of the mound (Figs 6.17 & 7.1). The find-spot is shown in Figure 6.17.

### Conclusions

In the 19th century, the mound was encircled by nine *Wellingtonias* equally spaced almost exactly along the line of the ditch with a tenth in the centre, subsequently pulled down in 1860 when the field in which the tumulus stood was incorporated into Lexden Park (Laver 1927, 241). *Wellingtonias* were introduced into this country in 1853; the tree-planting presumably accounts for the amphoras discovered here in 1860 (p 185). The accuracy with which the *Wellingtonias* follow the ditch suggests that the mound was much more distinct in 1860 than it is today. Layer 3, which is the destruction material from the mound, may be the product of a combination of various possible agencies such as previous excavations, tree-planting, soil-creep, deliberate levelling of the barrow for agricultural purposes, and ploughing prior to 1860.

In Trench 1 no positive barrow-ditch was located corresponding to the 1924 discoveries. If the ditch did cross Trench 1, then it must be represented by F2 and F3 which are in the correct position for the ditch in relation to the mound. The Lavers located the barrow ditch in two places. In one section it was 10 ft (3 m) across and 4 ft (1.2 m) deep whereas in the other it was only 7 ft (2.5 m) across and 3 ft (0.9 m) deep (Laver 1927, 245). The differences in size and the evidence in Trench 1 indicate that the ditch must vary considerably in its dimensions.

The datable finds recovered from the 1973 excavation do not add significantly to our present knowledge of the barrow. The sherds discovered in the cover loam below the mound are not helpful in dating the tumulus and are keeping with Jennifer Foster's date of c 15-10 BC (pp 87-8) for the monument.

### Acknowledgements

I am grateful to D Shimmin for his work as supervisor on the site and to Mr and Mrs J B Baker for granting permission to the Colchester Excavation Committee to carry out the excavations. I am also indebted to Miss S Limbrey for her site visit and her useful comments about the buried soils, and to Mr R G Sturdy for his help with the interpretation of the cover loam. The late Mr L Grant kindly examined the pipe fragments.

61

?Roman road, Fitzwalter Road

TL 97742490  
Observation  
P Laver  
1923

Observed in a trench for a water-main at Fitzwalter Road. Laver believed he could trace the road westwards across Lexden Dyke but his records are not adequate to plot his postulated course for it.

62

?Roman road, St Clare Drive

TL 97492513  
 Observation  
 P Laver  
*Rom Col*, 13

Laver recorded that in his garden part of a Roman road had been exposed, a ditch of which he had also seen just to the east when St Clare Road was being laid out.

63

East-west ditch and a pit at Lindens, Lexden Road

TL 97702517  
 Observation  
 Recorder unknown  
 Date unknown  
 Unpublished report in CM

Pottery and several finds of c AD 30-50 were found in the garden of Lindens, Lexden Road.

64

Enclosure on the north bank of the Colne

TL 98252590  
 Excavation (Field 500)  
 R J M Appleby  
 1952

The enclosure, known as cropmarks, lies on the north bank of the Colne, opposite the Sheepen site. A trench was cut across one of the ditches and 'pottery was found in great abundance — all earlier than 1400' but 'the contents of the ditch below the water-level (ie at 5 ft) have not been sampled'. The date of the enclosure has not been established for certain since the bottom of the ditches have not been excavated. Its eastern boundary reflects the modern layout of the area, a fact which supports a post-Roman origin for the feature.

65

Ditch, St Helena's School

TL 98902583  
 Observation  
 MRH  
 CMR 1937-44, 19

During the building of St Helena's School, 'a ditch of size about large enough to be called a dyke running approximately north and south'. No further information. This is within or close by the precinct of Temple 2.

66a

Sheepen excavations, 1930-39

TL 98602540  
 Excavation  
 CFCH & MRH  
 1930s  
*Camulodunum*

Major series of excavations. See pages 70-84.

66b

Sheepen excavations, 1970

TL 98652573  
 Excavation  
 BRKD  
 1970  
 Niblett 1985

Major excavation on part of the Sheepen site. See p 84.

66c

Sheepen excavations

TL 985252  
 Excavation  
 D Charlesworth  
 c 1971

Excavations by D Charlesworth looking for remains of Roman glass-working. Site was at the south site of Sheepen.

66d

Kiln Road excavation, 1971

TL 98502520  
 Excavation  
 PC  
 1971  
 Figs 6.19-6.24

*Introduction*

In the spring of 1971 trial excavations were carried out on the south side of Sheepen prior to the redevelopment of part of the site occupied at that time by a small estate of prefabs (Fig 6.19). The discoveries can be divided into two groups: i) a scatter of Late Bronze Age post holes, and ii) several features belonging to the Late Iron Age and early Roman occupation at Sheepen. The latter included over 600 fragments of moulds for coin flans.

No archaeological excavations were permitted between the demolition of the houses and the erection of the new buildings and therefore work had to be carried out on the fringes of the site. The topsoil was removed from four trenches in all although only one of these was excavated (Fig 6.20); on inspection the others appeared to be relatively barren and were therefore backfilled. A proton-magnetometer survey was carried out on a substantial part of the site by Dr M Tite to search for kilns or large pits but no magnetic anomalies were detected.

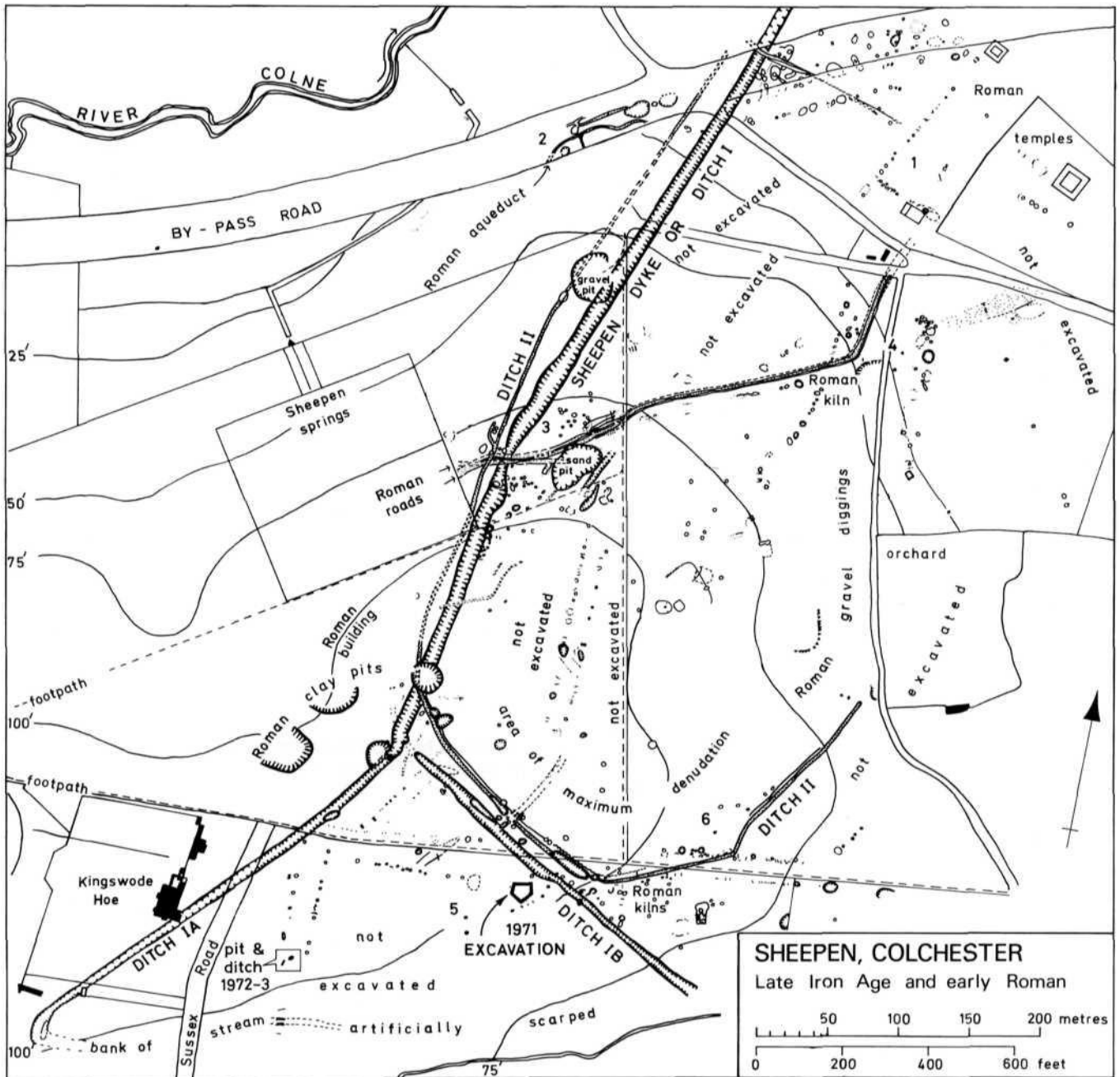


Fig 6.19 Kiln Road, Sheepen 1971 (no 66d). Location of the 1971 excavation in relation to the 1930-39 excavations. [Pages 131-7]

Despite the interesting results of the trial work in one of the trenches, no enlargement of the excavation was undertaken because of pressure of work elsewhere in the town and because the necessary expansion would have meant excavating beyond the redevelopment site into unthreatened areas.

The site excavated lies in Sheepen Region 5, Area Y. This area had been trenched in the 1930s where considerable 'traces of occupation' were found mainly of Sheepen Period 4 which is dated AD 49-60 (*Camulodunum*, 108-121, pl 110). All the features (apart from trenches of the previous excavation) were sealed by a layer of brown sandy loam about 0.3 m thick (Layer 2, Fig 6.21), which in

turn was sealed by a turf-line 0.2 m thick (Layer 1, Fig 6.21). The cover loam was thin and patchy being up to 0.25 m thick. The sand shown in Figure 6.20 is natural and occurs where the cover loam was absent.

In 1972 and 1973, a watching brief was maintained during the building works. The results are described below.

#### The Late Bronze Age features

One short slot and 33 post holes were found. Their profiles are shown in Figure 6.21 and their positions indicated in Figure 6.20. The post holes were filled with brown sandy silt loam and were typically 0.2-0.4 m wide and

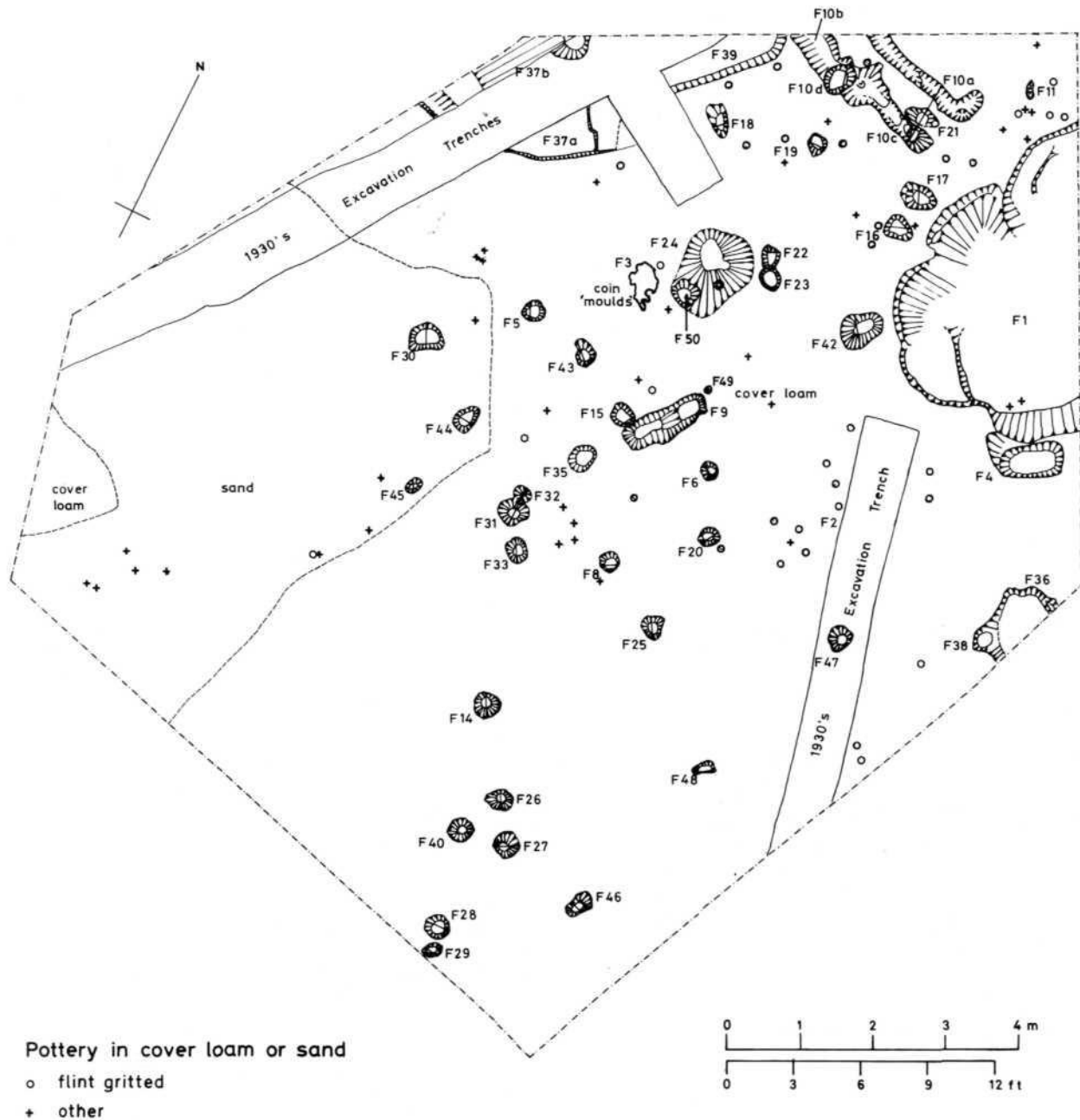


Fig 6.20 Kiln Road, Sheepen 1971 (no 66d). Site plan. 1:90. [Pages 131-7]

0.15-0.25 m deep. Only 19 of these features produced any pottery and this was exclusively flint-gritted. These features were F5, F8, F11, F14, F15, F16, F17, F22, F25, F27, F28, F29, F30, F31, F32, F33, F35, F38 and F40. Feature 14 contained almost half a pot in this fabric (Fig 6.22, no 1). The only pottery from the small pit F36 was a piece of flint-gritted pottery, and therefore the feature is likely to be contemporary with the post holes. Feature 4 contained no finds but because of its similarity to F36 is best seen as belonging to the same period of occupation.

A spread of flint-gritted pottery was also found. The find-spots of the sherds of this type which as a result of worm-action had penetrated the cover loam or natural sand are shown on Figure 6.20.

#### The other features

The features which belong to the Late Iron Age and early Roman occupation consist of two substantial pits (F1 and F37), a small dump of moulds for coin flans, a shallow depression (F24), a nearly complete pot (F29), and a complex of two parallel slots and two small depressions (F10). The features all contained sherds typical of the Late Iron Age or early Roman pottery from Sheepen.

Feature 1 contained several layers of brown or greyish brown sandy loam. These consisted of substantially the same soil and consequently were not distinct in section. The feature consisted of a series of small shallow scoops in the natural sand so that the bottom of the feature was

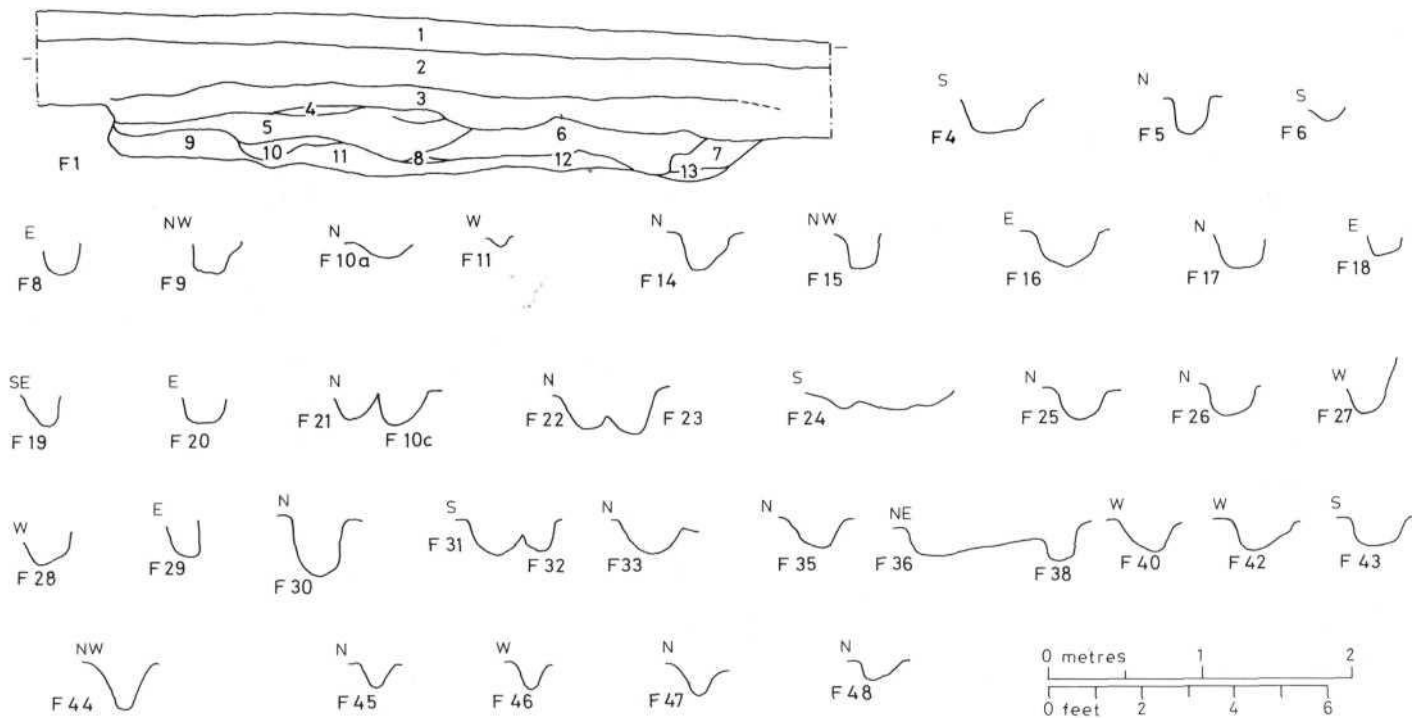


Fig 6.21 Kiln Road, Sheepen 1971 (no 66d). Sections and profiles. 1:50. [Pages 131-7]

flat and its shape irregular in plan.

Feature 10 was filled with brown sandy loam and consisted of two parallel slots (F10a and F10b), the southern one of which contained two depressions at its base (F10c and F10d). The depressions may have been irregularities in the base of F10b or they may reflect posts which had been set in the slot. F21 is probably part of the same complex.

Feature 37 was a large steep-sided, irregularly-shaped pit, which varied in depth between 0.3 m and 1.3 m. The pit was partially dug in the 1930s (*Camulodunum*, pl 110) as one of the many features excavated but unnumbered and therefore not discussed in the text. The tip-lines in F37 ranged from brown sandy loam to greyish brown sandy silt loam at the pit bottom and were interspersed with occasional lenses of sand and gravel.

Feature 3 consisted of a small dump of 438 fragments of coin-flan moulds and several large pieces of partially vitrified clay from a furnace, all apparently resting on the top of the cover loam (Fig 6.22). Fifty-two more fragments of coin-flan moulds were found in F37, 12 in the backfill of the trenches of the 1930s' excavation and 125 fragments either in the cover loam or the brown sandy loam which sealed it.

Feature 24 was a shallow depression about 0.25 m deep filled with sandy loam. Its proximity to F3 suggests that F24 may have been associated with it. No traces of burning were found anywhere on the site.

Feature 49 contained a small, upright, nearly complete pot of form Cam 231 (Fig 6.23, no 2). There was no cremated bone.

Feature 39 was a shallow depression apparently cut into the cover loam and filled with similar soil. Since it contained no finds and since its base coincides with the base of the cover loam, F39 is probably the base of the pit.

#### Finds

In all 173 sherds of flint-gritted pottery plus the half pot from F14 were recovered. The material is of Late Bronze Age date and is similar to pottery from Mucking North Ring (Bond 1988), Springfield Lyon (Buckley & Hedges 1988), and Lofts Farm (Brown 1988a), all of which are sites in Essex.

Features 1, 4, 10, 24, and 39 all contained sherds belonging to the 1st century AD as did the cover loam and the layer of brown sandy loam which sealed it. The significant pieces can be summarised as follows:

Feature 1  
samian Drag 27 (Claudian or Claudio-Neronian)  
samian Drag 15 (Claudian or Claudio-Neronian)  
samian Drag 24/5 (Claudian or Claudio-Neronian)  
Cam 94, 183, 218 x 2, 258 x 2, 270B & 273

Feature 10  
samian Drag 27 (Claudian)

Feature 37  
samian Drag 18 (1st century)  
samian Drag 27 (pre-Flavian)  
Cam 108, 191, 218 & 221  
Feature 49

almost complete Cam 231

Cover loam  
samian Drag 36? (1st century)  
samian plate (probably pre-Flavian)  
Cam 17a, 119, 243 or 246 & 254  
Brown sandy loam

Cam 16, 108, 183, 218, 270C  
Cam 16, 108, 183, 218, 270C coincides with the base of the cover loam, F39 is probably the base of the pit.

All the samian is South Gaulish.



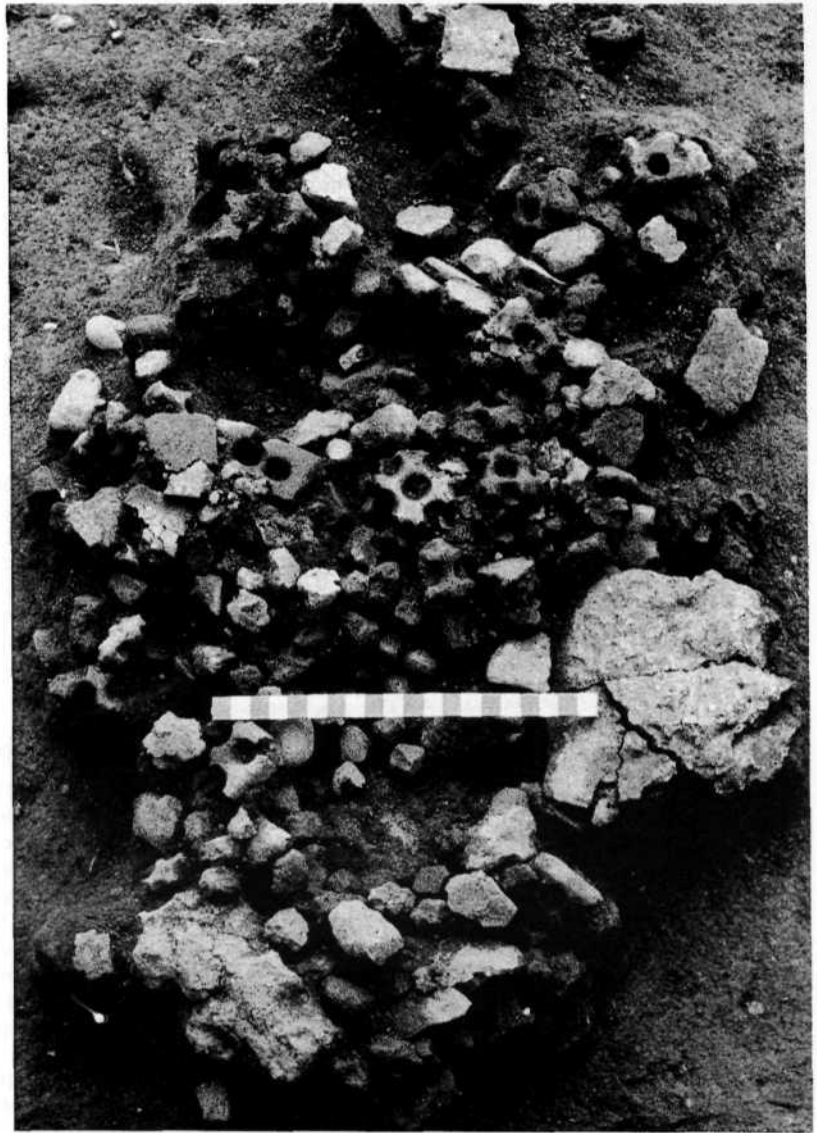


Fig 6.22 Kiln Road, Sheepen 1971 (no 66d).  
Broken coin-flan moulds. [Page 134]

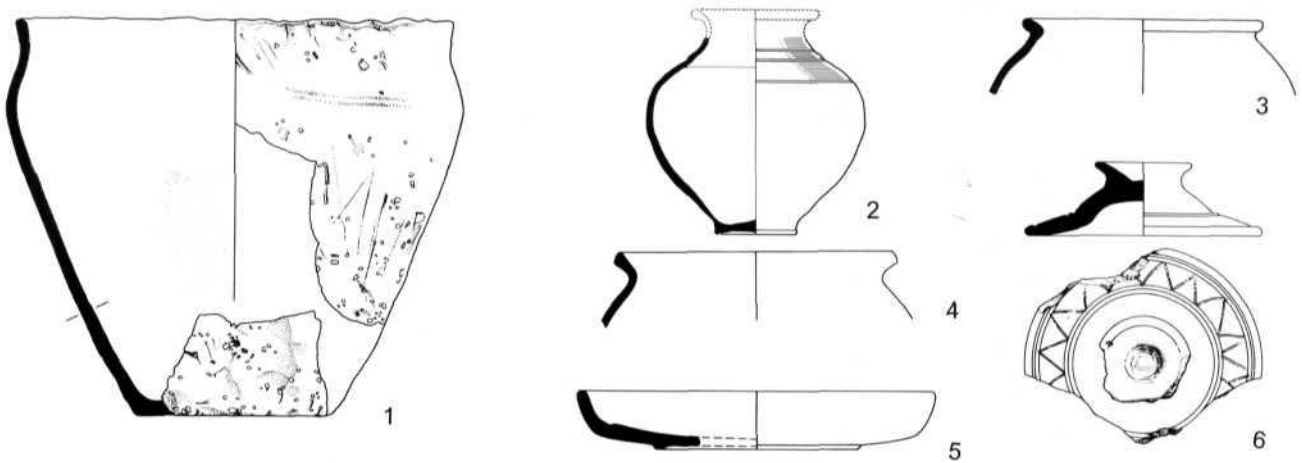


Fig 6.23 Kiln Road, Sheepen 1971 (no 66d). Pottery. 1:4. [Page 134]

On the basis of the associated finds, F1, F10, F37 and probably F4 are assignable to Sheepen Periods 2 to 6 whereas the dump of moulds and perhaps F24 to Period 1 when the Iron Age mint was in operation.

#### Discoveries made during the redevelopment of the site

In February 1972, whilst redevelopment of the site was under way, the builders uncovered a pit and part of a ditch (Fig 6.19). The pit contained pottery, two brooches, at least 56 fragments of coin-flan mould and a quantity of charcoal and vitrified clay. No flan-mould fragments seem to have been found in the ditch, although the finds have to be regarded as being unstratified since most of them were recovered by the workmen who mixed them all together.

The sherds securely stratified in the ditch include forms Cam 118 (Fig 6.23, no 3), Cam 256B (Fig 6.23, no 4) and Cam 270. The pottery from the pit includes Cam 12, Cam 30 (Fig 6.23, no 5), Cam 56 (TR 2), Cam 56C (TN), Cam 218 and a lid (Fig 6.23, no 6). The remaining pottery is unstratified and contains examples of Cam 8 (TN), Cam 12 (TN), Cam 232, Cam 257, Cam 266, and Cam 271. Also unstratified but presumably mostly if not all from the pit were another 97 fragments of coin-flan moulds.

The lid from the pit is not closely matched in the *Camulodunum* series. It is closest to lid form 16 (*Camulodunum*, 273, pl 85) but has a hollow knob, a characteristic of the native lids from the site. The lid is decorated with shallow diagonal lines and its surfaces are smooth but not burnished or slipped.

The sherd of Cam 118 (Fig 6.23, no 3) from the ditch is in so-called 'Romanising native grey ware' and is thus apparently a late example of its type. The sherd of Cam 256B (Fig 6.23, no 4), also from the ditch, is close in form to Cam 112. The rim sherd (Fig 6.23, no 5) is an example of Cam 30 of which only three were found during the 1930s' excavations (*Camulodunum*, 223).

#### The two brooches from the pit by Nina Crummy

i) Copper-alloy brooch (Fig 6.24, no 1), *Corpus* Type 26A (*Camulodunum* Type X, class A (ii)), the rosette- or thistle-brooch, cast in one piece with a small, solid, plain disc at the junction of the humped bow and foot; minimum length 52.5 mm. The foot, which 1930-29s excavated in straight and slightly curved, most (a 'multitude') were found either within or close by Sheepen

unlike the illustrated example from Sheepen which has a flat, splayed foot (*Camulodunum*, pi 93, no 68). The tubular spring-cover and pin are damaged. The bow and foot are decorated with groove and wavy-line ornament and there are traces of groove ornament on the spring-cover fanning out from the junction with

One example from Sheepen comes from a Period 1 deposit and is accounted among the pre-conquest imports on that site. Continental examples date the type to the Augusto-Tiberian period (*Camulodunum*, 314).

ii) Copper-alloy 'Aucissa' brooch (Fig 6.24, no 2), *Corpus* Type 51B (*Camulodunum* Type XVII), with an iron axis with end knobs. The pin and half the axis with its end-knob are broken off, the foot is distorted and its terminal knob missing. Length 38 mm.

The flat head is decorated with grooved transverse mouldings and a pair of internal notches and the semicircular bow has a stout grooved central rib and marginal mouldings. The tapering foot is separated

#### Discussion

The post holes are of significance in that they represent the first evidence of structures of the Late Bronze Age from Sheepen. The area examined was too small for an adequate appraisal of plan or settlement type but in conjunction with the known distribution of contemporary material (p 131) does at least establish the potential of the site for further work on this period. Unfortunately the area examined was also too small to reveal clearly if the buildings to which the post holes belonged were rectilinear rather than round.

The large part of a pot in F14 may have been used as packing or it may have been placed in the pit as part of a ritual in the way suggested elsewhere such as Broads Green in Essex (Brown 1988b and 1994, 10).



Fig 6.24 Kiln Road, Sheepen 1971 (no 66d). Brooches found during the watching brief of 1972-3. 1:1.

pit K1 (*Camulodunum*, 129). The 780 fragments found in 1971-3 alter the known distribution pattern and suggest at least two coin-making sites 200 m or so apart.

Feature 1 with its distinctive scoops belongs to the enigmatic series of so-called 'working hollows' described in detail for Little Woodbury (Bersu 1940, 64-78), and as postulated elsewhere at Sheepen (*Camulodunum*, 75-6, 101 & 105). Other features regarded as such include those at Tolland Royal (Wainwright 1968, 111-2), Boscombe Down (Richardson 1951, 127-8), Shearplace Hill (Rahtz & ApSimon 1962, 305), and Saffron Walden (Ecclestone 1994, 262). Working hollows are usually interpreted as being associated with domestic activities especially threshing. A further explanation, namely that these were small quarries (Bradley 1978, 43-4), is an attractive one; at least in the case of irregular hollows such as F1 at Kiln Road, because of the resemblance these have to groups of small, overlapping, grubbed-out pits.

I am grateful to Nigel Brown for his help with the discussion and with the Late Bronze Age pottery.

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67

Roman occupation, Colchester Royal Grammar School

TL 98802489  
Excavation  
P W Crittendon of the Royal Grammar School  
1964  
*Col Archaeol Grou Bulletin*, 10 (1967), 2-6

Dug on postulated line of the main Colchester-London road through the Balkerne Gate. No road found in area examined; instead a series of layers indicative of occupation or intensive activity in the area in 1st- and early 2nd-century times.

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68

Roman road, 12 Lexden Road

TL 98712489  
Excavation  
Colchester Royal Grammar School  
Mid 1950s  
Plan in CM

An unpublished excavation. In the garden of 12 Lexden Road and in the northern part of the adjacent plot to the south, pupils at the Colchester Royal Grammar School dug an extensive series of small trenches during the mid 1950s. Earlier work by Hull (*Rom Col*, 6-8) had already established the layout of the road junction here (the so-called 'three-track road' and two subsidiary roads — see Fig 6.1) and this work was to examine in more detail a small part of the area concerned. Unfortunately the museum contains little information about the work except a large detailed site plan. Little has been published about the work except in *CMR* 1954-56 (p 14) where Hull wrote, 'The area has proved rich in Roman remains of the first and second centuries, with many pits and trenches on both sides of the road, some of them even encroaching upon it and one crossing it. There are remains of burials in great quantity.'

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69

Enclosure, West House Farm

TL 97302600  
Excavation  
R J M Appleby  
1952  
CMR 1950-4, 53 and *MS* excavation report by Appleby in CM

A trench was dug across the south side of the enclosure at West House Farm. The ditch was about 3 ft 6 ins (1.1 m) deep below the base of the modern topsoil and about 16 ft (4.9 m) wide. The 'loam' shown in the section can probably be equated with silt at the base of the ditch whereas the 'gravelly loam' can be taken to be the backfill, presumably the bank redeposited into the partially silted-up ditch. In the 'loam' was found some pottery now lost but apparently datable from the 8th century to c 50 BC, whereas in the gravelly loam was found a piece of Roman tile. Although not proved conclusively, the earthwork may have been Early or Middle Iron Age or earlier.

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70

Prehistoric pits, St John's Abbey grounds

TL 99882478  
Excavation  
PC  
1972  
Figs 6.25-6.37

Two small pits (F19 and F20), filled with light grey sandy loam, were found during the excavation of a late Anglo-Saxon church and Roman cemetery (*CAR* 9, 205). Probably associated with these were four sherds of Late Bronze Age flint-gritted pottery, one of which was found in F20 and the other three as residual finds in later features.

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71

Berechurch Dyke and Barnhall Dyke

TM 00122330  
Excavation and observation  
A F Hall  
Hall 1960

Junction of Berechurch Dyke and Barnhall Dyke. See pages 24-5.

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72

Small ditches west of Berechurch Dyke

TL 99602070  
Excavation  
MRH with A F Hall  
1929  
CMR 1929, 6, small scale sketch plan on CM OS 6" map plus note 'Small parallel ditches found and partly excavated 1929. This area is full of pottery of 1-50 AD but also some 2nd century'.

**73**

Gosbecks-Colchester Roman road, Rayner's Farm

TL 97772340  
Excavation  
MRH  
1936

Report & section in *Rom Col*, 9-10, fig 2

See p 68.

**74**

Neolithic pit, Culver Street excavation

TL 99552509  
Excavation  
PC  
1981-2 & 1984-5  
CAR 6, 37-8

Pit containing Late Neolithic pottery.

**75**

Pitchbury Ramparts excavations, 1933 and 1973

TL 967290  
Excavation  
T Cruso and PC  
1933 & 1973

*Summary*  
[Fig 6.25]

Excavations at Pitchbury in 1973 and to a degree in 1933 revealed extensive plough damage to the interior of the hillfort. The defences were sectioned and the main rampart found to be probably of the dump variety. No structures were detected within the hillfort's interior, only a series of small, mainly sterile pits of unknown origins. No clear evidence of the date of the hillfort was recovered although on balance one within the 1st century BC would seem most likely, the occupation having been slight. Small quantities of finds indicate intermittent activity in the area from the Mesolithic to the early medieval periods.

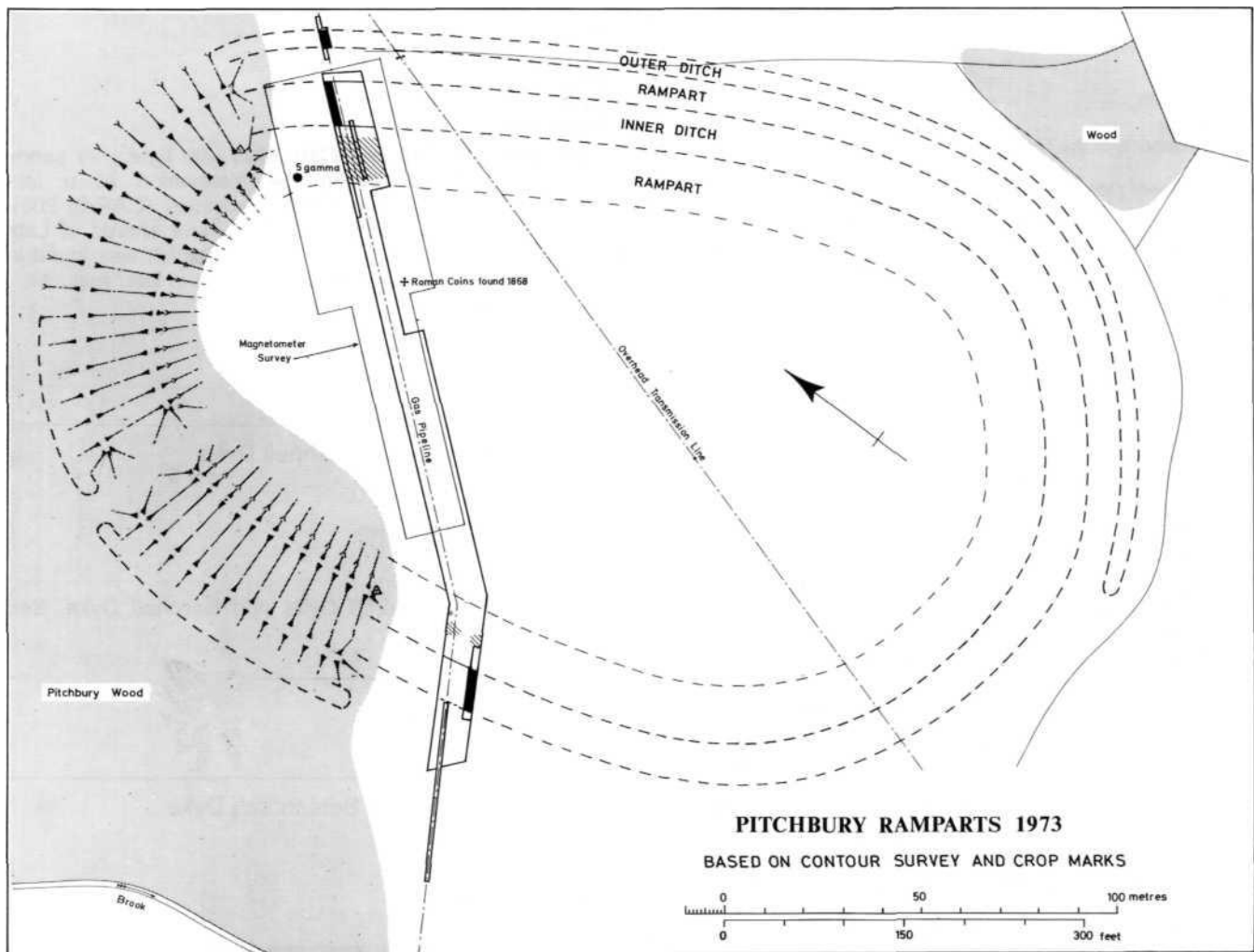


Fig 6.25 Pitchbury Ramparts 1973 (no 75). Plan of the hillfort. [Pages 138-51]

### Introduction

Pitchbury Ramparts (Fig 6.25) is a partly bivallate hillfort with a single entrance and an internal area of 4.9 acres (1.9 hectares). It lies to the north-west of the Colchester dyke complex and overlooks the valley of St Botolph's Brook. Like much of north-east Essex, the countryside here is flat and is contoured only by river valleys. The hillfort is 175 feet (53 metres) above sea-level and overlies glacial deposits of sand and gravel, capped by a layer of cover loam.

The hillfort has been examined by archaeological excavation on two occasions, both under the auspices of the Colchester Excavation Committee, once in 1933 using long hand-dug trenches and again in 1973 by area excavation. The excavation of 1933 was conducted by Miss Thalassa Cruso as a research project to supplement the long-term work then in progress at Sheepen (*Camulodunum*) and the work of 1973 by Philip Crummy as a rescue excavation prior to the construction of a gas pipeline.

Between 1840 and 1844, most of the rampart was levelled, the date of destruction being clear from two sources. The Tithe Map of the area, dated 1840 (ERO D/CT 184B) shows the line of the ramparts preserved still intact as a wide hedge or wooded strip. However William Wire, a clockmaker and local antiquarian wrote in his journal on 13th May 1844, 'Went to West Bergholt to do a clock. Came

home so as to cross the circular trenchment near Picts-bury Wood. About two thirds of their rampart is thrown down. What a pity works of antiquity should be so destroyed'.

On the Ordnance Survey six-inch map, it is recorded that in 1868 Roman coins were found at Pitchbury. Unfortunately, the relevant part of the Ordnance Survey's primary records was destroyed during the last war (the Object Name Book for the parish of Great Horkesley) and no further information about this discovery can be found elsewhere.

### THE EXCAVATION OF 1933

by Thalassa Cruso

[Figs 6.26-6.28]

[PC: There is no final report available for this work, but instead are here reproduced a paper given by Miss Thalassa Cruso to the Society of Antiquaries in 1933, an original site plan and some sections, all redrawn for this volume (Figs 6.26-6.27), and two site photographs found in the Hawkes archive (Fig 6.28).]

Pitchbury Ramparts, the subject of my talk this evening, is an oval enclosure, which lies on the high ground about 2.5 miles to the NW of modern Colchester. The earthwork

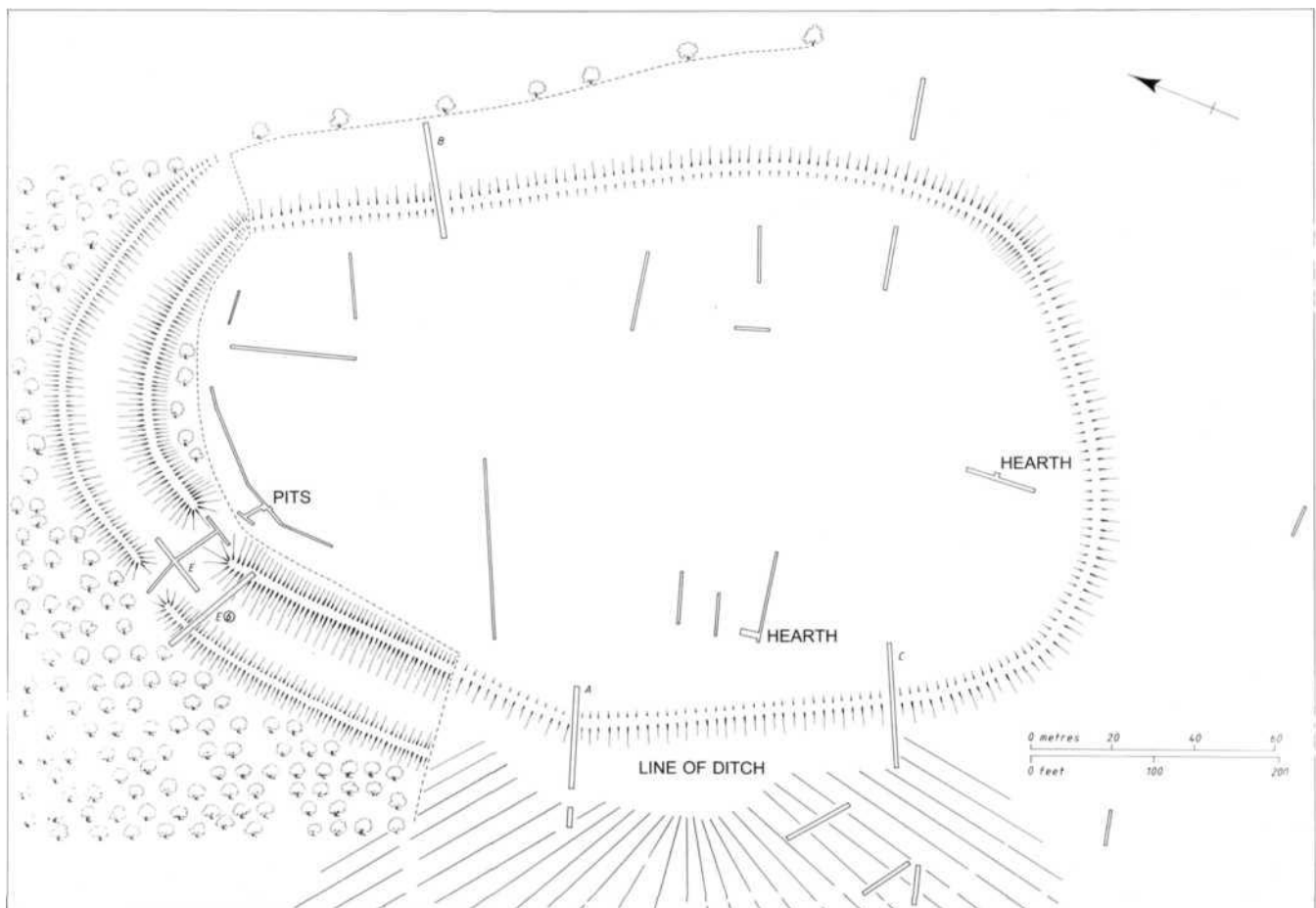


Fig 6.26 Pitchbury Ramparts 1933 (no 75). [Pages 139-43]

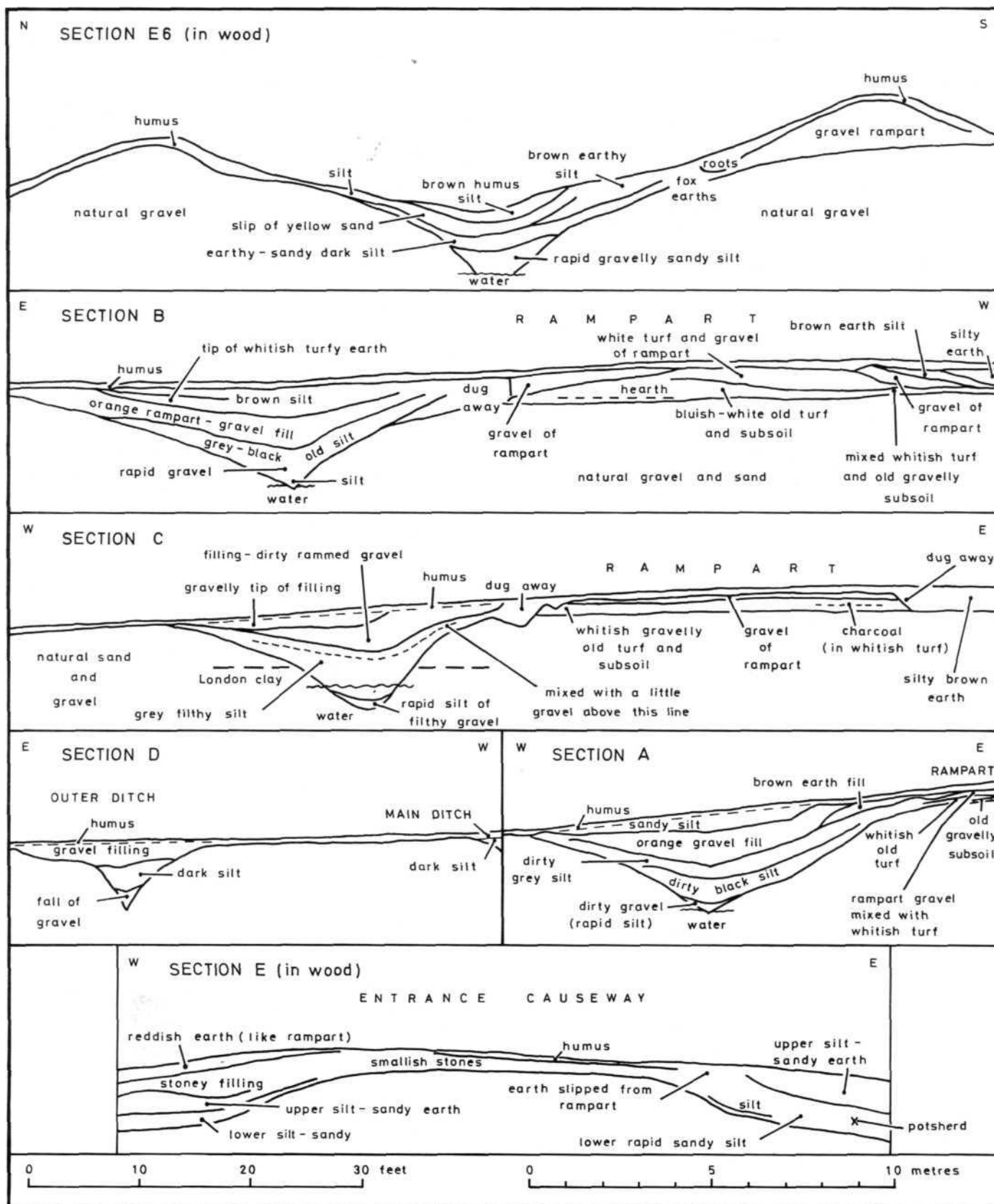


Fig 6.27 Pitchbury Ramparts 1933 (no 75). Sections. [Pages 139-43]

formerly consisted of a single bank, ditch and counterscarp, and until the year 1850 remained in a reasonably intact condition. At about that date the ground on which it stood was enclosed and the camp divided between two estates. The major portion was enclosed on farm ground and deliberately ploughed out for agricultural purposes. Incidentally this destruction was totally useless, for the field, owing to a stony-gravel subsoil, has since remained stubbornly barren. The remaining small section, which included the entrance, was left untouched, but was planted with a thick plantation. Outside the wood, practically all surface indications have been obliterated: a very slight mound marks the line of the rampart, while the ditch may in some places be traced by a thicker darker line of sorrel. Within the wood the rampart and ditch are still quite well marked, though their condition has not been improved by the growth of the plantation and the earths of innumerable foxes.

Work was undertaken on this site for the dual purpose of determining whether Stukeley, as quoted by Morant, had been correctly reported as stating that the main line of the Gryme's dyke crossed the Colne at New Bridge and continued to end in a circular earthwork on Horkesley Heath. All other maps and surveys of the system had consistently shown the dyke ending on the river Colne some 1.5 miles below the camp. It was further hoped that dating evidence would be found within the camp.

Sufficient voluntary labour was available to enable us to work on both these problems, and I would here like to bear witness to the efficiency and good temper of the dozen or so undergraduates who dug almost uncomplainingly for three weeks through iron-hard gravel into foul-smelling clay during some of the hottest weather last summer. Nor can I pass without acknowledging the unfailing kindness and interest of Mr and Mrs George Perkins, on whose ground the major part of the work took place and who gave us every possible encouragement and assistance.

As far as material finds are concerned, the work proved practically barren, but this in no way lessens the importance of putting the real picture as it then stood, for the season's work undoubtedly led us to in

The rampart was sectioned and the ditch traced in three trenches, and, permission having been obtained from Mr Higgins, owner of the wood, a final trench was cut through the rampart, ditch and counterscarp where they are still standing. In all these sections the same results were obtained. Out in the open, the upper layer of the ditch consisted, naturally enough, of a filling of rammed gravel from the destroyed rampart. Below the old silt, a heavy black marsh deposit, still full of well-preserved twigs and leaves, bearing eloquent testimony to the damp conditions. Beneath this again lay a considerable deposit of rapid silt, a mixture of gravel and marsh-clay, the ditch being cut down in every case into the thick stiff London-clay beds. In no instance were we able to clear this rapid silt right out, for though the whole district was suffering from an acute water-famine, the bottom of all these trenches stood in a foot or more of water, which reappeared as soon as it was bailed. There can be little doubt that the ditch must have had standing water in the bottom when it was constructed, and the appearance of the upper silt suggests continuous wet conditions.

The ramparts, except in the wood, appear to have had a mixed sand and gravel core, capped with a gravel top. In the wood the construction of the rampart was gravel throughout. In no section was there a clear indication where the rampart had ended and the ditch begun: the original work here had either slipped immediately after construction, or else had never been finished off. The unfinished appearance was further substantiated by the discovery of a small outer ditch at the NE end of the camp. The discovery was made late in the season when our labour had dwindled to a degree that made it impossible to do more than section the ditch through. Its extent consequently is still unknown, but from surface indications of sorrel it appears to have existed only round the turn of the rampart, possible as an additional support in view of the fact that the ground rises somewhat behind the camp at this point. The section showed beyond doubt an entirely unfinished condition, the ditch having almost perpendicular sides and a flat bottom, being in fact little more than a preliminary trench.

Finally on this point the entrance must be considered. The work here was not as complete as we had wished, for we obtained permission only on condition that we cut no bushes, dead branches or young saplings, and technically we were limited to one trench which was to be filled up immediately on completion. Sufficient work was done however to show a causeway of 35 feet passing through the outer-counterscarp and main rampart, the shallow ends of the ditch being traced on both sides. This causeway lay immediately under the spread from the rampart and followed the contour of the ground uphill. It bore no traces of use, the mixed gravel and sand being entirely clean. These three pieces of evidence then, the bad condition (in every trench) of the nose of the bank, the unfinished outer ditch and the clean unworn causeway may, I think, be taken as evidence that the camp never underwent any extensive occupation.

The interior was thoroughly trenched as a test. Two empty storage pits were discovered and two hearths. In all, three small pieces of pottery were found. One was picked up on the surface at a point where subsequent work proved the area entirely barren, one was found in the hearth here, and a third was discovered in the rapid silt of the causeway ditch. All three fragments agree in form. They are wheel-made of a greyish-black ware, and in the opinion of Mr Hawkes may be dated to the 1st century AD. Otherwise our search proved fruitless, but the discovery of these few remains confirm the previous surmise of a very slight occupation. A total blank would have given rise to the suspicion that we had missed the occupation altogether. These three fragments of pottery cannot of course be considered final dating evidence. There would have to be considerable enlargement of the present evidence before a final pronouncement could be made. At the most they can only be accepted as a possible indication of date, and on the season's work we may therefore conclude the camp to be an unoccupied work, left possibly in an unfinished state, and conceivably built in the 1st century AD.

I turn now to the other side of the season's work: the identification of the possible line of dyke connecting the camp with Gryme's Dyke. Working from the statement already mentioned, we trenched along all possible lines of connection. Nowhere was there the slightest trace of the dyke. Excavations were also carried out along the side of the small valley immediately below the camp where a dark line on the air photograph suggested the possibility of a filled-in ditch. The work here resulted in the untimely





*Fig 6.28 Pitchbury Ramparts 1933 (no 75). The excavations in 1933 viewed from the north-east. These overlapping photographs were taken from the top of a tree in the hedge bordering the north-east side of the hillfort. It shows the central southern half of the site. The position of the inner ditch on the south side of the fort is indicated by the dark band of sorrel on the left. Scarlet's Farm is visible in the centre distance. [Pages 139-43]*

discovery of a field-drain, but it also emphasised an important point, namely the extremely marshy character of the valley lands beneath the western side of the camp. It can be definitely stated that the dyke did not join on to the earthwork.

I now turn to the survey of the Lexden system made by Stukeley in 1758, and brought to our notice since the excavations took place by Mr E J Rudsdale of the Colchester Museum. You will notice that Stukeley's plan (Fig 2.7, p 16) does not confirm the statement previously attributed to him, that the rampart joins the earthwork; but it does show Gryme's Dyke crossing the river Colne and fading away on the high land two thirds of a mile further on. An examination of the geology of the district in association with this map and our work this season, gives, I think, the clue to the situation. In its north-eastwards sweep from the Roman River, Gryme's Dyke was built upon the gravel-lands facing immediately upon the boulder-clay to the west. It marked, that is to say, the end of the relatively open country and faced upon the forest lands. Near to the crossing of the river Colne a new feature enters the topography of the district. This feature is a small stream running from the NE to join the Colne just below New Bridge. The stream itself is of the utmost insignificance, but

our work this dry summer produced sufficient evidence of marsh conditions to enable us to realise the extensive marsh lands which must formerly have existed here. The valley of this small stream lies immediately parallel to the possible continuation of the Gryme's Dyke and flanks the NW side of the camp, and thus forms the natural continuation of the boundary dyke. We may, I think offer as a possibility the suggestion that the main Gryme's Dyke crossed the alluvial valley of the Colne and continued on to the high lands of the Horkesley Heath; up to this point it had been aligned upon Pitchbury Ramparts, its place as a boundary mark was then taken by this small marshy valley which would serve excellently for the purpose.

If this explanation of Stukeley's map is accepted, we are led to the possible conclusion that Pitchbury Ramparts formed part of the original Lexden system. In support of this conclusion, I would cite the slight dating evidence which accords with the results obtained last season in Lexden Park (pp 35-45), and also with the conclusions reached by Dr Henry Laver in his magnificent early explorations of the system (Laver 1902). I would also emphasise the fact that Pitchbury occupied a gravel promontory, for the boulder clay, hitherto running parallel to the line of the dyke, sweeps north-eastwards





immediately behind the camp. In the past, therefore Pitchbury Ramparts must have faced the forest on two sides. To sum up, I would suggest the possibility that we have in Pitchbury an outer redoubt or outpost to the Lexden system, which was never used and possibly never finished. Its connection with the main line of dykes must be sought not in a continuation of the bank and ditch, but in an appreciation of the geology of the region. •

#### THE EXCAVATION OF 1973

[Figs 6.25 & 6.29-6.37]

The 1973 excavation (Fig 6.25) lasted eight weeks and had to take place during the height of the summer when site conditions were at their most difficult. It was intended to examine a 10-metre wide strip along the course of the impending pipeline, but only part of the work was completed. The treatment of the various areas of the strip is summarised in Figure 6.31. Prior to the excavation, a magnetometer survey (Fig 2.1) was carried out by Mr A Clark from the Ancient Monuments Laboratory of the Department of the Environment but only a single anomaly of 5 gamma was detected. After the gas pipeline was laid a contour survey of the hillfort was undertaken (Fig 6.29) and profiles prepared of the surviving earthworks (Fig 6.30).

Plough damage over the interior of the hillfort has resulted in much of the cover loam being destroyed, particularly in the central area, where the ground level is slightly higher

than elsewhere and where the cover loam has almost all been ploughed off. Only under the line of the ramparts is the cover loam well preserved.

No structures were discovered, the only features being shallow, mainly sterile pits. Small quantities of pottery and flints of various periods were found over the area of the excavation. Some of these were found in features but most came from either the ploughsoil or, as a result of worm-action, the top 15 cm or so of the cover loam.

#### *The pits*

A total of 47 pits (Figs 6.32-6.33) were at least partially excavated. The fills of these were typically slightly stony or stony yellowish-brown loamy sand and their edges often difficult to differentiate from the natural. They varied in depth from 0.1 m to 0.9 m and had either rounded or roughly-flat bottoms. As shown in Table 6.1, the pits can be divided into three broad groups on the basis of their finds and their stratigraphical relationships, ie those with finds, those without finds and those which are medieval or modern. Finds in the upper 10 to 15 cm of a feature cannot be taken to have been originally in its fill because these could be intrusive as a result of worm-action.

#### *Defences (Fig 6.34)*

The inner ditch is 8.5 m across and 2.6 m deep at the east end of the excavated area and 10.0 m across and 2.3 m deep at the west end. The rampart had been pushed or shovelled

into the ditch so that at the west end (F5) it only survived *in situ* in two patches no more than 0.10 m thick. At the east end difficulty was experienced differentiating the make-up of the rampart (F8) from the underlying buried land surface but it seemed as if the lowest 0.10 m of the rampart remained in place. No convincing evidence was found of a revetment except perhaps the unlikely series of possible post holes or post-pits, F52, F62, F68 and F74 and it is therefore highly probable that the rampart was of the simple dump variety. The base of the rampart at the west (F5) contained one sherd of Group 3 (10th to 8th centuries BC) and a flint flake, and sealed two sherds of Group 5 (8th to 4th centuries BC). No pottery or flints were found in the silt of the main ditch at either end, but a sample of charcoal from fairly high up in this (Fig 6.34) was dated by radiocarbon analysis to 10 be  $\pm$  80 (HAR-452). When recalibrated on the basis of Stuiver & Pearson 1986, this equals cal BC 160 - AD 230 with confidence limits of 95% or cal BC 50 - AD 120 with confidence limits of 68%.

The backfill of the west ditch section (F34) is very distinctive and seals a well-preserved turf-line still containing leaves and twigs. However on the east side (F38), the sequence is not so clear and it is possible that by 1840-44 when most of the defences were levelled, this area had already been partly destroyed.

The outer ditch (F46), present only at the east end of the excavation, is 4.3 m wide and 1.5 m deep. It had silted up entirely, being filled with typically dark brown or dark greyish-brown loamy sand and contained only one flake blade (possibly Mesolithic) and one flake. No traces survived of its rampart.

#### *Features under or at the base of the rampart*

At or near the base of the rampart was a series of roughly circular patches of light grey cover loam in yellowish-brown sandy loam (Fig 6.35), the latter being indistinguishable from the body of the rampart. When sectioned it became apparent that the light patches were cut by the darker areas, the latter forming a unified network of shallow, round-bottomed gullies round the lighter patches. Well down in the light patches was an undulating layer of charcoal which, except where cut by the gullies, would appear to have extended over the whole area. The charcoal layer varied in thickness up to 5 cm and was composed of fine fragments of oak and a little beech. But for the charcoal, these features resembled 'plug involutions', a type of structure formed under periglacial conditions (Williams 1973, 25-6).

However a sample of charcoal was dated by radiocarbon analysis and produced the result 800  $\pm$  90 bp (HAR-453) which then recalibrated according to Pearson & Stuiver 1986 is equivalent to cal BC 800 - 250 with 95% confidence limits or cal BC 770 - 390 with 68% confidence limits. (I am grateful to Alex Bayliss of the Ancient Monuments Laboratory for help with the recalibrations.)

#### *The pottery*

On the basis of fabric, the pottery was divided into 12 groups of which eight are prehistoric. None merits illustration. Of the entire assemblage, only one rim sherd, which belongs to Group 8, is Late Iron Age. The pottery can be tabulated as in Table 6.2.

#### *The flints*

The flints were examined by Dr J J Wymer whose descriptions of them can be summarised as follows:

- 1 polished flint knife (Fig 6.36 no 1)
- 1 double-ended scraper (Fig 6.37 no 4)
- 1 core fragment (Fig 6.37 no 6)
- 1 core tablet (Fig 6.37 no 7)
- 1 petit-tranchet derivative arrowhead (Fig 6.37 no 8)
- 1 axe-sharpening flake (Fig 6.37 no 9)
- 1 small borer
- plus 12 flake-blades, 2 blades, 45 flakes and 4 pot boilers

Dr Wymer writes, 'This is an interesting assemblage of Mesolithic and Late Neolithic industries. The former is represented by the core rejuvenator and the fine example of an 'axe-sharpening' flake. Perhaps only one of the flakes looks like Mesolithic work. Late Neolithic flints are the petit-tranchet derivative arrowhead and the superb specimen of a discoidal polished knife. The latter is a rare type of flint implement (Clarke 1929).'

#### *Discussion*

Ploughing has effectively destroyed any clear traces of structures within the hillfort and all that survives is a series of small shallow pits and a spread of pottery and flints. The latter can be represented in tabular form (Table 6.3) where the finds have been divided up by area on the basis of the site grid. By including finds from the ramparts and ditches in the appropriate places in relation to the site grid, the distribution of finds from east to west can be shown in this

Table 6.1 *Pitchbury Ramparts 1973 (no 75). Finds from pits.*

#### Pits containing prehistoric finds

F23	1 flake blade
F28	2 sherds Group 1 (Late Neolithic), 1 sherd Group 3 (10th-8th centuries BC), 1 sherd Group 8 (Late Iron Age), 13 flakes, 1 blade, 1 flake-blade, 1 core fragment and 1 double-sided scraper
F32	1 flake
F43	1 sherd Group 1 (Late Neolithic)
F67	2 sherds Groups 5 and 6 (8th-4th centuries BC), 1 flake

#### Pits without finds or with finds only in upper 10-15 cm

F1, F8-F10, F21-F22, F24-F25	
F29	clay pipe (10-15 cm down)
F30-F31	
F35	flake in top 15 cm
F39	1 flake, 1 flake-blade (top 10 cm)
F41	Mesolithic core rejuvenator, 1 flake-blade (in top 15 cm)
F45, F47-F48, F52, F55-F58	
F59, F62-F63, F66, F68-F75	
F76	2 flakes, 1 blade, 1 large flake-blade, 1 flint discoidal knife (all found in upper 5-10 cm)

#### Other pits (modern or medieval)

F40	modern
F42	modern
F49	modern
F53	modern
F54	medieval
F64	medieval

Table 6.2 Pitchbury Ramparts 1973 (no 75). Pottery fabrics.

Date	Group	Fabric
Late Neolithic	1	Dark grey with yellowish-red outer surface and a little flint gritting.
Early Bronze Age	2	Thick friable sherds: in section reddish-brown outer half and dark grey inner half. Sparse flint grits and smooth surfaces.
10th-8th centuries BC	3	Flint-gritted sherds usually oxidised reddish-brown throughout but, in one case, with a dark grey inner surface.
8th-4th centuries BC	4	Dark brownish-grey fabric with in some cases a reddish-brown surface with a well-defined margin. Distinctively smooth surfaces but not burnished. The absence of a filler has resulted in easily-abraded breaks.
8th-4th centuries BC	5	Dark grey sandy fabric with reddish-brown outer (and in some cases inner) surfaces. Well-defined margins.
8th-4th centuries BC	6	Similar to Group 5 but surfaces more yellowish.
8th-4th centuries BC	7	Very similar to Group 6 but sandier and larger thicker sherds. Surfaces only slightly oxidised yellowish-brown.
Late Iron Age c 50 BC-AD 10	8	Dark brownish-grey fabric with brownish-grey soapy surfaces.
Roman (prob 1st cent AD)	9	Light grey fabric with dark grey surfaces.
11th-12th centuries	10	Yellowish-brown sandy fabric. Some sherds with grey cores.
11th-12th centuries?	11	Dark grey core with dark reddish-brown inner surface and dark grey outer surface. Sandy fabric with grog?
13th century	12	Light grey sandy fabric. Some pieces underfired and soft.

Table 6.3 Pitchbury Ramparts 1973 (no 75). Distribution of finds across the site.

	Flint	Pottery Groups						
		1 Late Neolithic	2 Early Bronze Age	3 Late Bronze Age	4-7 8th-4th cent BC	8 50 BC-AD 10	9 1st cent AD	10-12 early medieval
F46	2 **?	-	-	-	-	-	-	-
F38	4	-	-	-	-	1	-	32
140-50 m	3	-	-	2	23	1	-	1
150-60 m	1	-	-	-	3	-	-	1
160-70 m	3	2	-	6	16	1	2	3
170-80 m	2	-	-	-	-	-	-	1
180-90 m	3	-	-	-	-	2	-	-
190-200 m	2	-	-	-	1	-	-	-
200-10 m	24 **	4	-	1	-	2	-	-
210-20 m	2 *	-	1	-	-	-	-	-
220-30 m	2	-	-	-	-	-	-	-
230-40 m	-	-	1	-	1	-	-	-
240-50 m	1	-	-	-	-	-	-	-
250-60 m	4	-	-	-	-	-	-	-
260-70 m	1	-	-	-	2	-	-	-
F5	2	-	-	-	1	-	-	-
F34	1 **	-	-	-	-	-	-	-
290-300m	-	-	-	-	-	-	-	1

\* Late Neolithic

\*\* Mesolithic

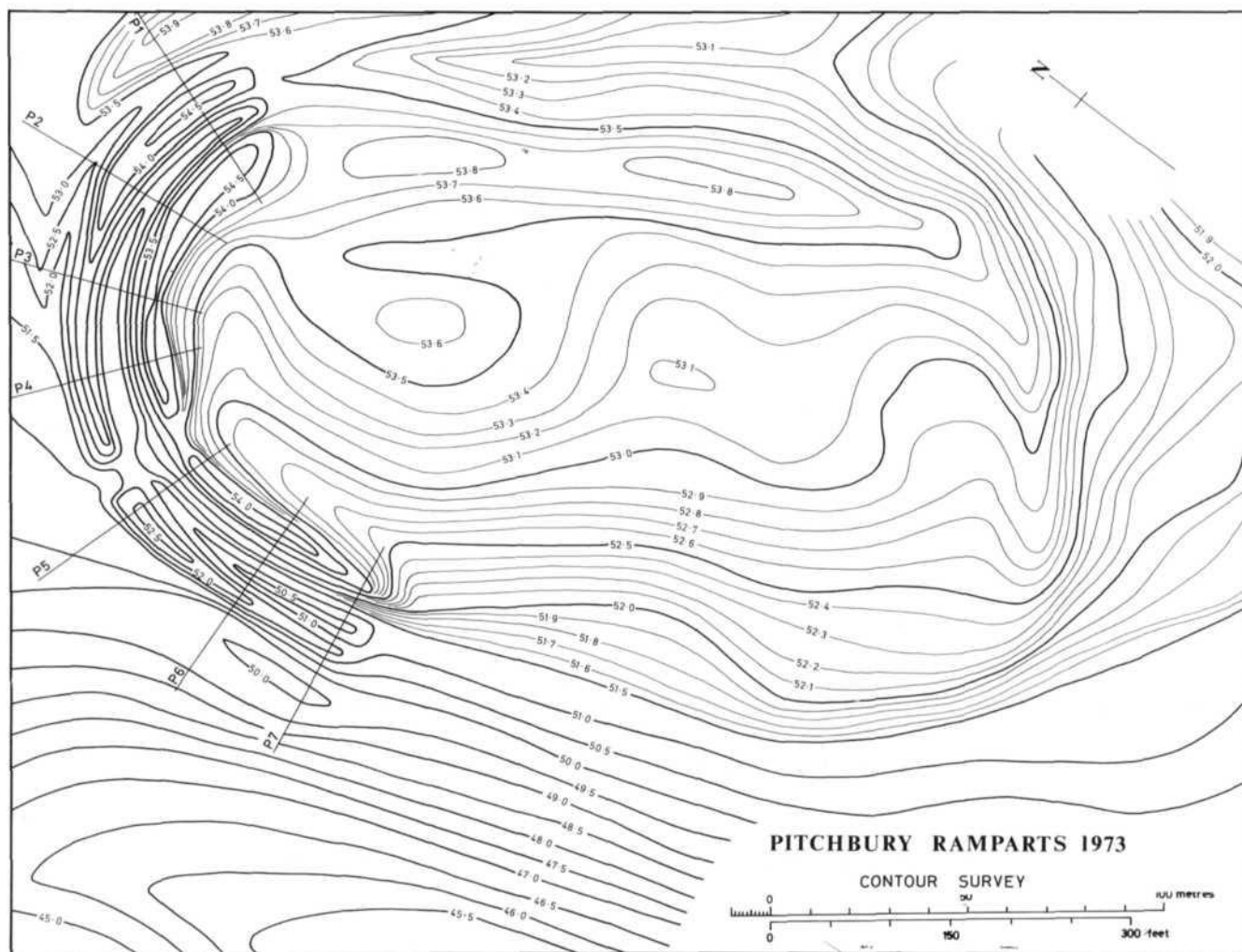


Fig 6.29 Pitchbury Ramparts 1973 (no 75). Contour survey. [Page 143]

manner. Since the numbers of finds within each context in this table are products of the excavation procedures adopted for that context, and since this procedure varied across the site (Fig 6.31), the distribution of finds does not reflect relative intensities of occupation over the excavated area. However differing distributions of one or more types of find to another can be significant. The most striking case in this respect is the distribution of flints compared with that of the prehistoric pottery. Most of the latter was found between the 140 and 180 metre grid lines whereas the flints are commonest in the 200 to 210 metre area. This pattern is matched by the occurrence of the Late Neolithic pottery (Group 1) and by the find-spots of the diagnostic late Neolithic flint implements. The distribution of all these finds centres around the area of later pit F28 and thus it is highly likely that in this area there is a late Neolithic occupation site. Features 58 and 76 have a symmetry suggesting a rectangular structure, although it is possible that these are both simply of periglacial origin. In contrast to this, the diagnostic Mesolithic flints are spread across the site, a fact which when combined with Dr Wymer's view that few of the rest of the flakes are likely to be Mesolithic, suggests that casual loss rather than occupation is the explanation for the presence of these finds.

The other significant distribution is that of the early medieval pottery, almost all of which lies east of the 170 m

grid line and most of which was found in the backfill of F38. Since much of this pottery is underfired, the kiln (or kilns) which produced the sherds is probably close by. Indeed, although very weak, the anomaly detected by Mr Clark in his magnetometer survey (Fig 6.25) might indicate the remains of such a kiln. This site would seem very suitable, being at the rear of the rampart and close to the plentiful fuel supply provided by the adjacent Pitchbury Woods. There is evidence in the form of wasters (Drury & Petchey 1975) and field-names for a substantial medieval pottery industry in the Horkesley area, of which this kiln or kilns would form part.

The distributions of the other pottery groups are too similar or concern too few sherds to enable meaningful deductions about them. Indeed it is not possible to be sure how many discrete periods of activity the finds groups represent. Groups 8 and 9 could be contemporary and perhaps so too Groups 3 to 7 and also Groups 1 and 2.

The pits without finds or at least without finds below the top 15 cm are difficult to interpret. Many of these could be periglacial or be small tree-holes formed during clearance of woodland in prehistoric times; an obvious context here is site preparation prior to the construction of the hillfort. Had these features been associated with occupation, then we might expect them to contain more finds than they did.

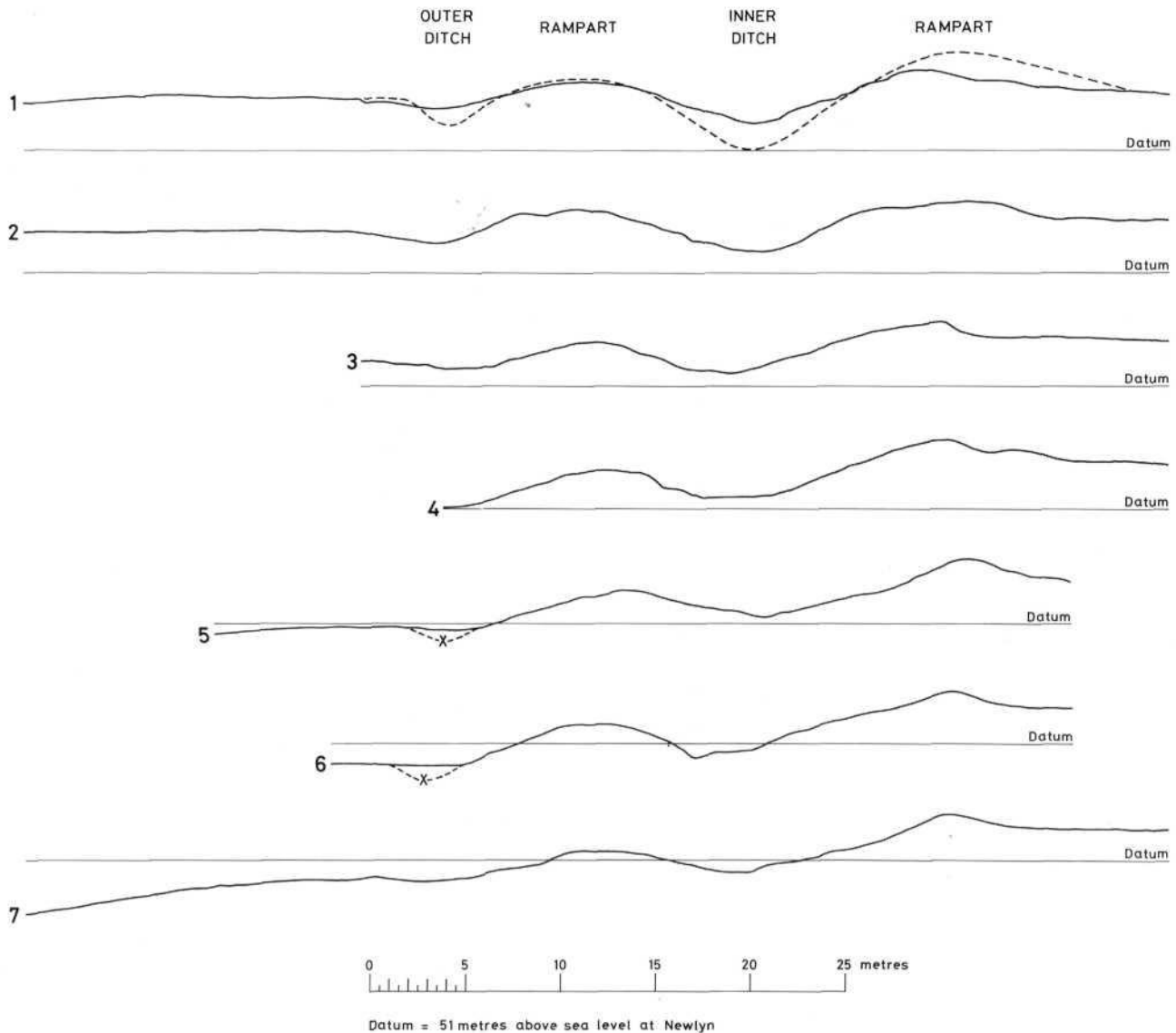


Fig 6.30 Pitchbury Ramparts 1973 (no 75). Profiles across the defences preserved in Pitchbury Wood, 1973. 1:360. [Pages 143-4]

Those in F28, the only pit to yield finds in any quantity, must be nearly all residual since the latest piece is Late Iron Age although most of them are Late Neolithic.

The central problem — the date of the hillfort — must remain unresolved. The pottery points to two possible periods, one in the 8th to 4th centuries BC and the other from c 50 BC to c AD 10. The absence of pottery of the intervening period would suggest that there had been no continuity of occupation between the two. Intuitively the period c 50 BC-c AD 10 is to be preferred, but the small quantities of pottery concerned must dictate caution.

The charcoal under the rampart has provided by radiocarbon analysis a *terminus post quem* of the 8th to the 3rd centuries BC, but its temporal relationship to the rampart is obscure. Does the charcoal predate the rampart by long enough for a thick topsoil to accumulate over it (ie the now 'weathered turf'), or is it associated with a much earlier phase of activity than the hillfort, or is it the result of

site clearance prior to the construction of the hillfort? Independent of this, the finds of pottery of Groups 5 and 6 found both in and under the rampart provide another *terminus post quem*, this time of the 8th to 4th centuries BC. The contexts of these hint that all the pottery of Groups 4 to 7 (ie all the 8th- to 4th-century BC pottery) predates the defences since the sherds in question must have been deposited before the defences were built. But this is not conclusive and the radiocarbon date for the sample from the ditch section is of little help here since it occurs well up in the silt (Fig 6.34).

The structure and arrangement of the defences themselves have some limited value as indications of date. The ramparts are almost certainly of the 'dump' variety. Although commonly adopted from the 3rd century BC to the 1st century AD, the technique has an older ancestry (Cunliffe 1974, 232-3) and therefore does not necessarily imply a late date. The occurrence of a second line of

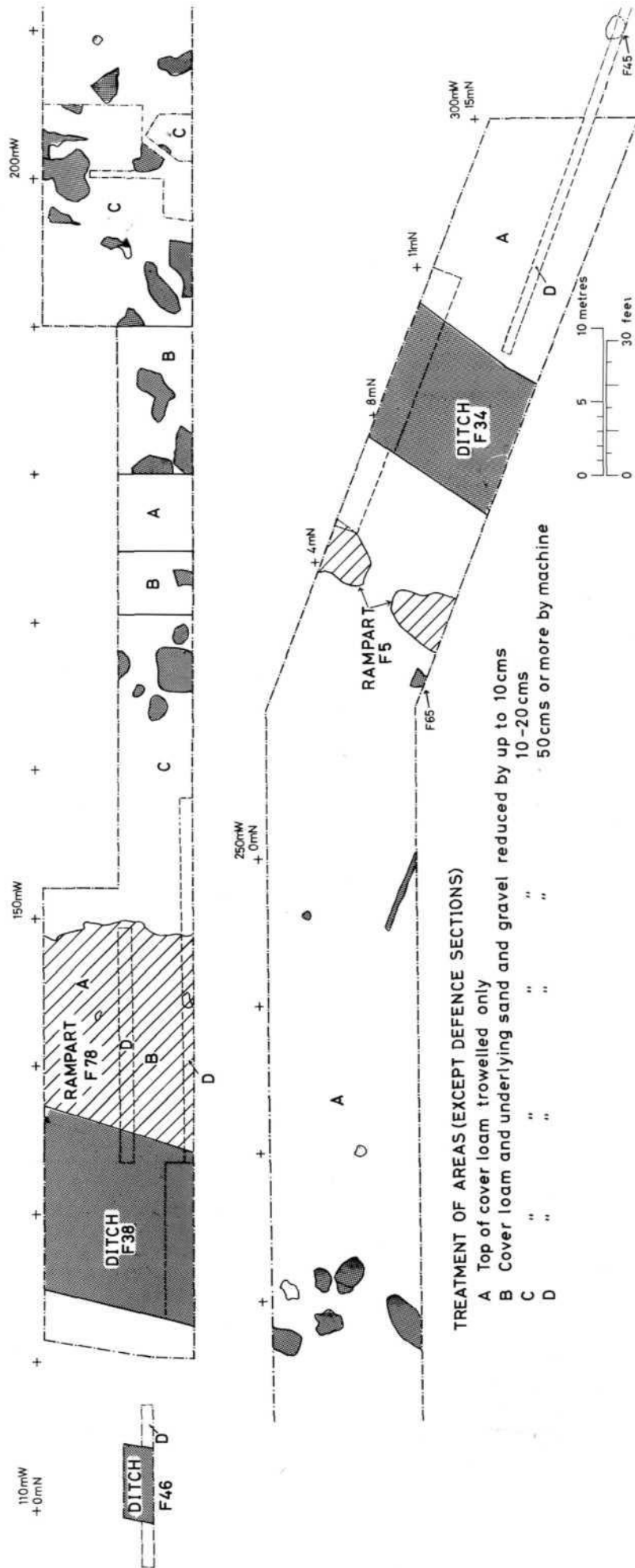


Fig 6.31 Pitchbury Ramparts 1973 (no 75). The treatment of the excavated areas. 1:400. [Page 143]

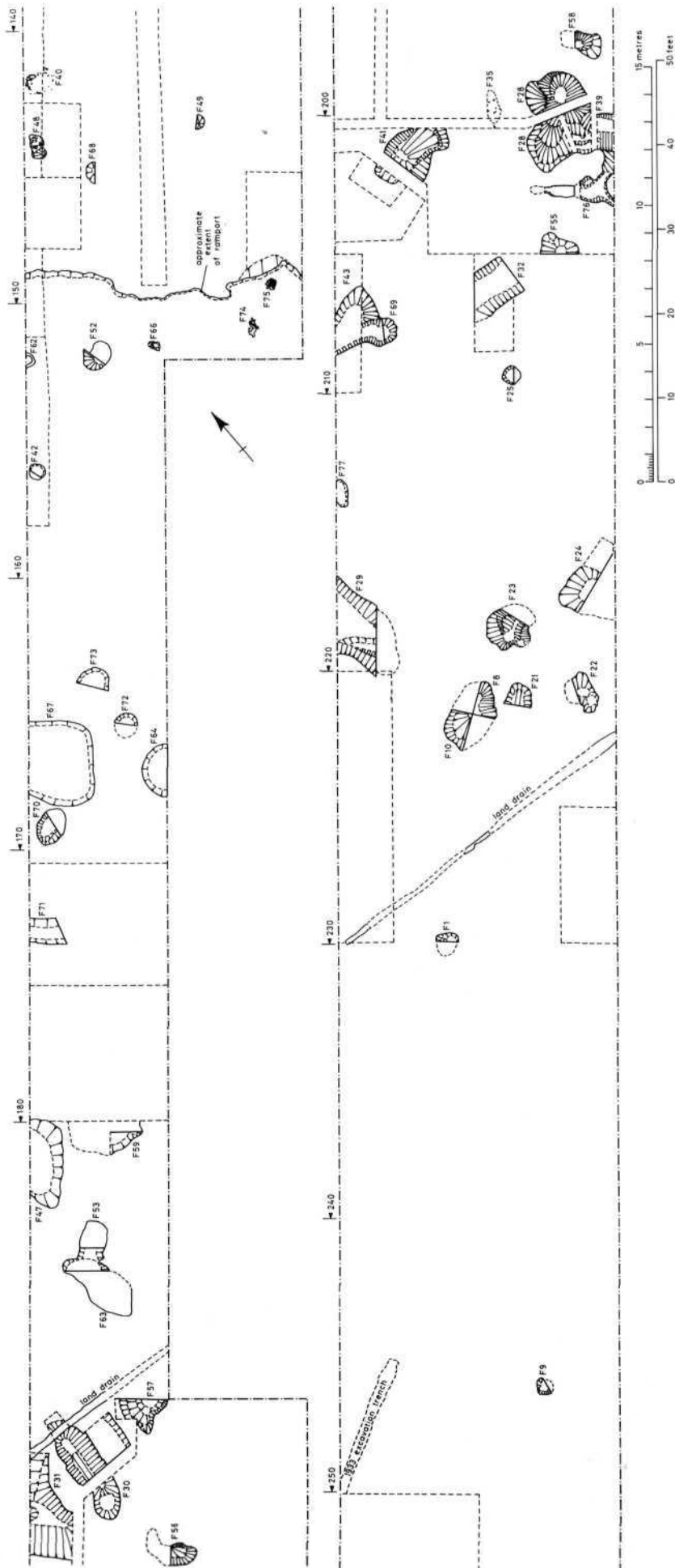


Fig 6.32 Pitchbury Ramparts 1973 (no 75). Site plan of main area of excavation. 1:220. [Pages 143-54]

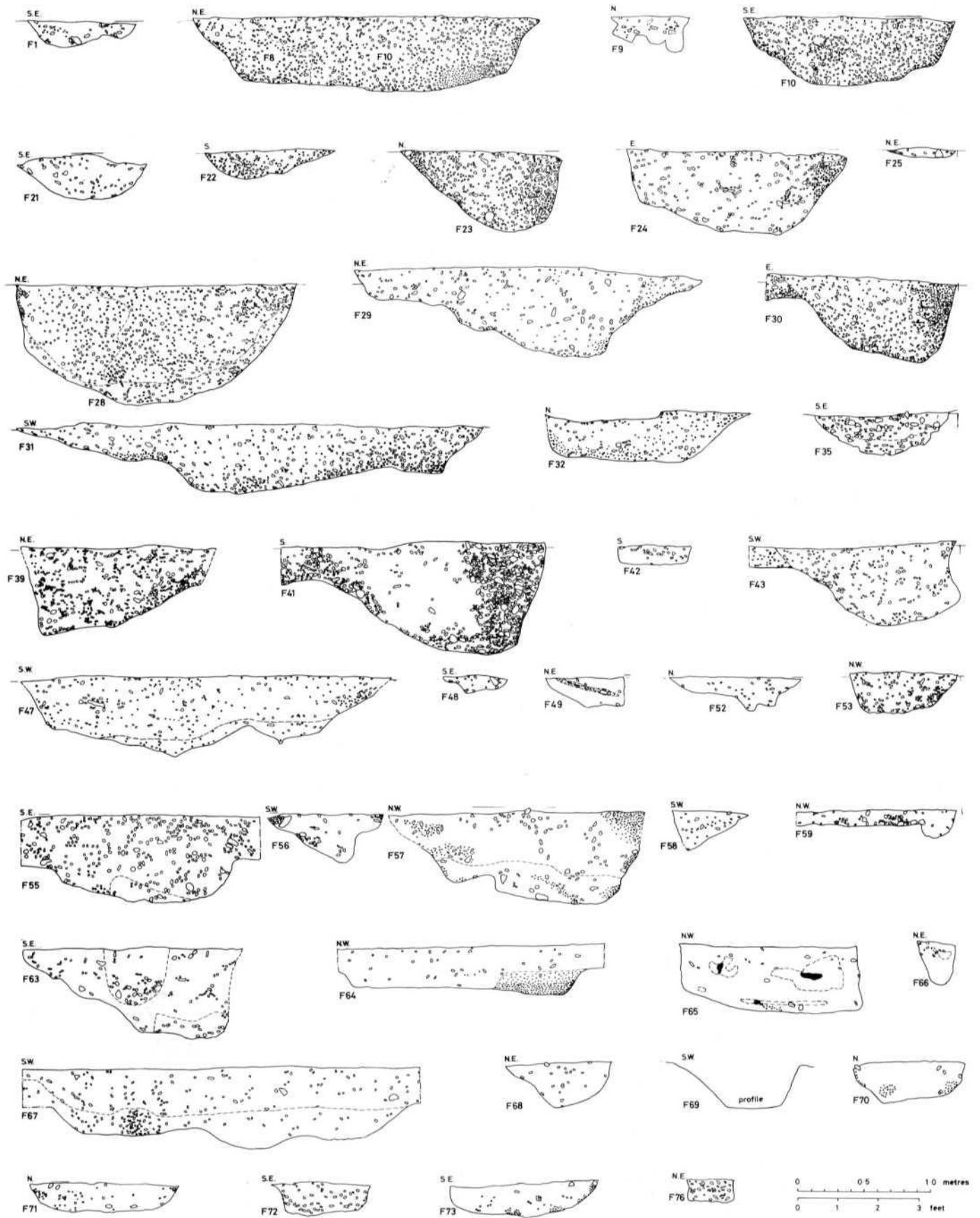


Fig 6.33 Pitchbury Ramparts 1973 (no 75). Sections. 1:40. [Pages 143-54]



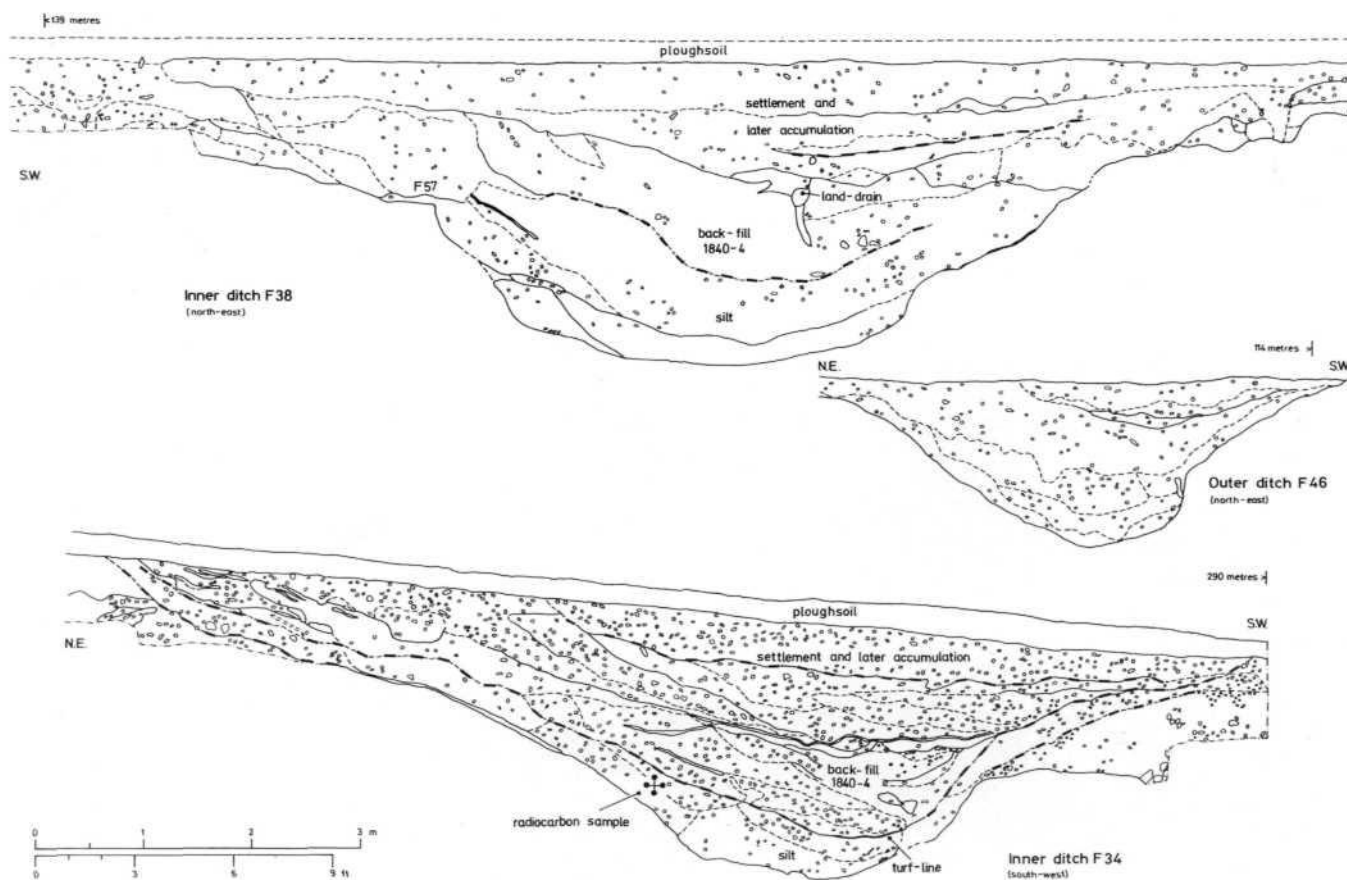


Fig 6.34 Pitchbury Ramparts 1973 (no 75). Sections across defensive ditches. 1:70. [Pages 143-4]

defences at Pitchbury would favour the hillfort having been in use late in the Iron Age since multivallation appears to be a late phenomenon perhaps of the 1st century BC and 1st century AD although, again, this is not conclusive (Cunliffe 1974, 233 and Harding 1974, 66). However taken *together*, dump ramparts associated with multivallation tend to confirm a late date. An alternative but less likely interpretation is that Pitchbury was an early example of a hillfort with a dump rampart which was built some time in the period from the 8th to the 4th centuries BC, abandoned and then refortified with the addition of a small outer bank and ditch in the 1st century BC.

In any event, whatever the dates of the ditches, one fact seems fairly clear; namely, that if not already abandoned, the hillfort had become redundant during the 1st century BC in favour of the massive system of earthworks then evolving to the south. Of significance too is the absence of pottery in any quantity in the silt of the ditches; this suggests that the hillfort never saw any lengthy periods of intensive occupation, but rather it was sporadic or slight or both. Although the evidence of occupation is very limited and no associated structures were found in 1933 or 1973, this could be the result of inadequate excavation; further area excavations carried out under favourable conditions might be more successful in this respect, especially if placed close by the ramparts.

In conclusion, the date of the construction of Pitchbury is obscure and is probably to be placed either in the 8th to 4th centuries BC or more likely in the 1st century BC. Certainly

by at latest the start of the 1st century AD, the hillfort was a remote and minor feature and an anachronism on the fringes of the rapidly-growing network of linear defences of the Iron Age settlement.

#### Acknowledgements

The excavation in 1973 was supervised by Gerry Pratt with help from Tessa McCormick, Nick Smith, Carl Crossan, Gill Bright (later Crossan) and Bob Sylvester. The plans were redrawn for publication by John Hayes who also carried out the site surveys. The other drawings were prepared by Bob Moyes and Alison Drysdale and the photographs by Jerry Lockett and Alison Colchester. The Eastern Gas Board and Mr Henley-Welsh, the farmer, gave permission to excavate the site and granted access to it. Miss Wallis kindly allowed part of her land to be used as a campsite for the volunteers. To these people, especially Gerry, Tessa, John and all the volunteers, I extend my warmest thanks.

I am also indebted to the following who provided expert specialist advice: Mr A Clark for his magnetometer survey, Dr J Wymer for his flint report, Mr J Barrett for his pottery report, Mr R Allan of the Soil Survey, Writtle, Dr C Turner of the Department of Botany, University of Cambridge and Dr R Williams at the University of Sussex.

Finally, grants for the excavation were gratefully received from the Department of the Environment, Essex County Council, Colchester Borough Council, Eastern Gas, and Lexden and Winstree District Council.

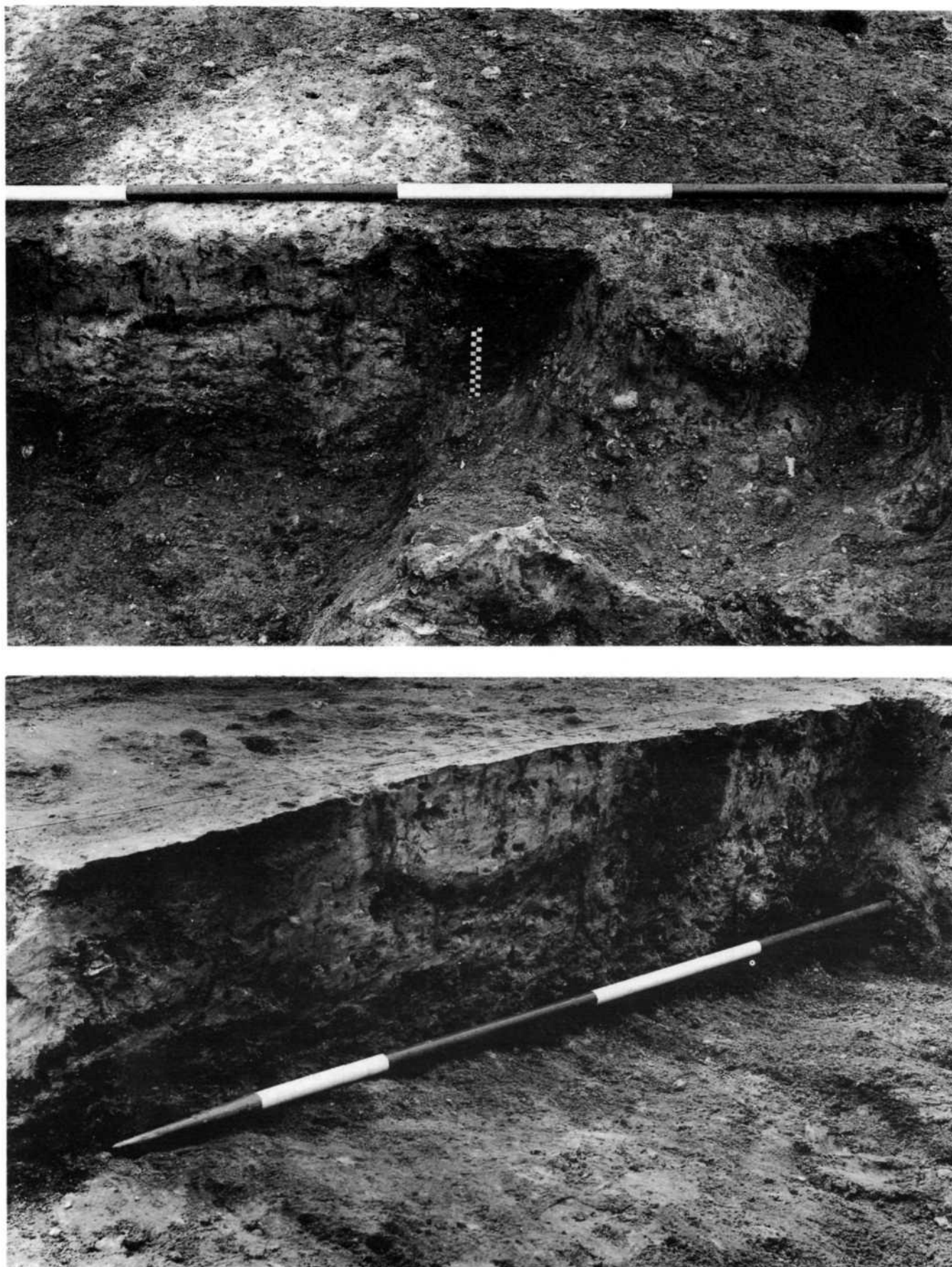


Fig 6.35 Pitchbury Ramparts 1973 (no 75). Features under the main rampart at the east end. [Page 144]

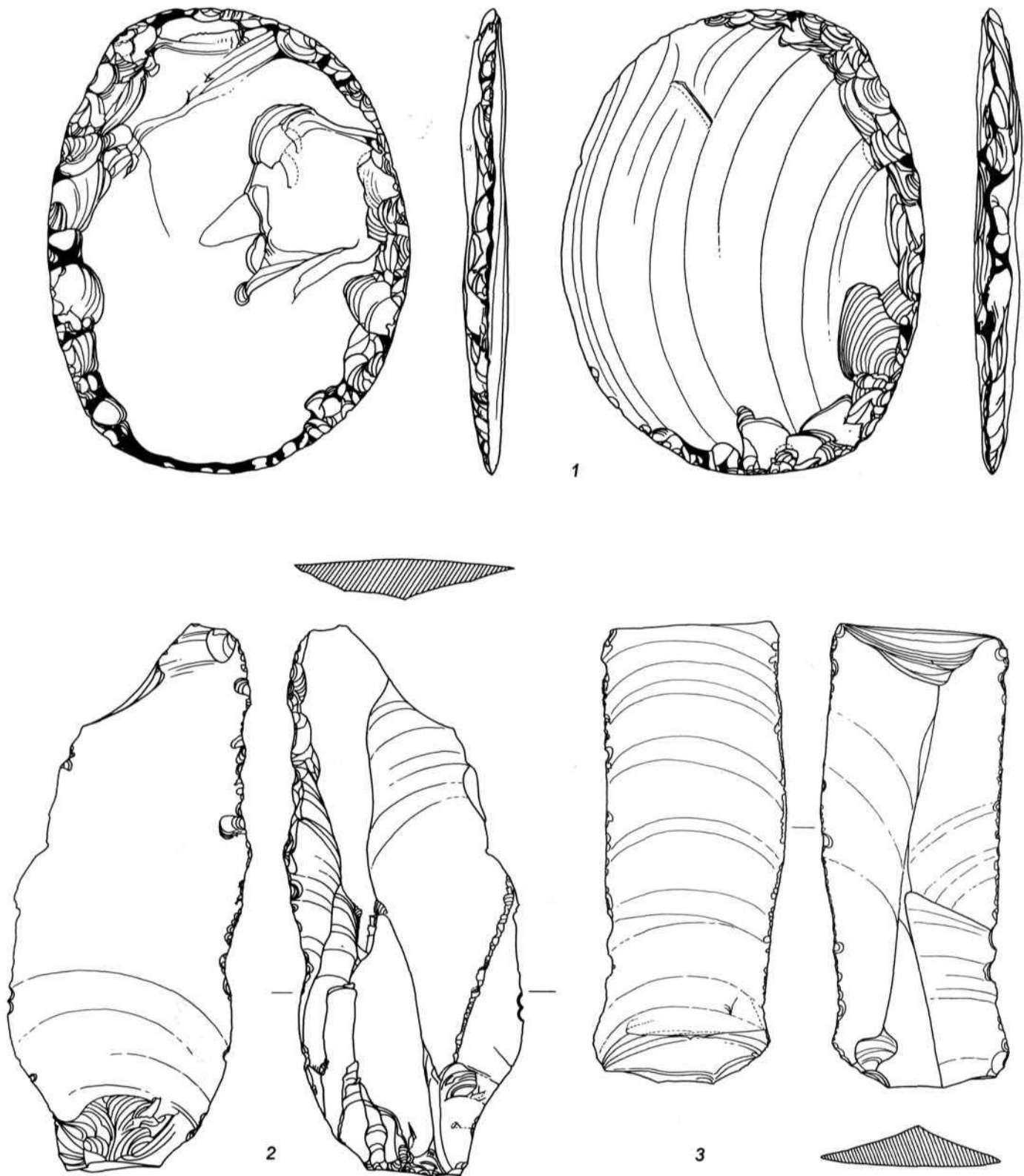


Fig 6.36 Pitchbury Ramparts 1973 (no 75). Selection of flints. 1... discoidal polished flint knife; 2... large flake blade with some secondary working; 3... large blade with signs of use and a hinge fracture at each end. All are from the top few cms of F76. 1:1.  
[Page 144]

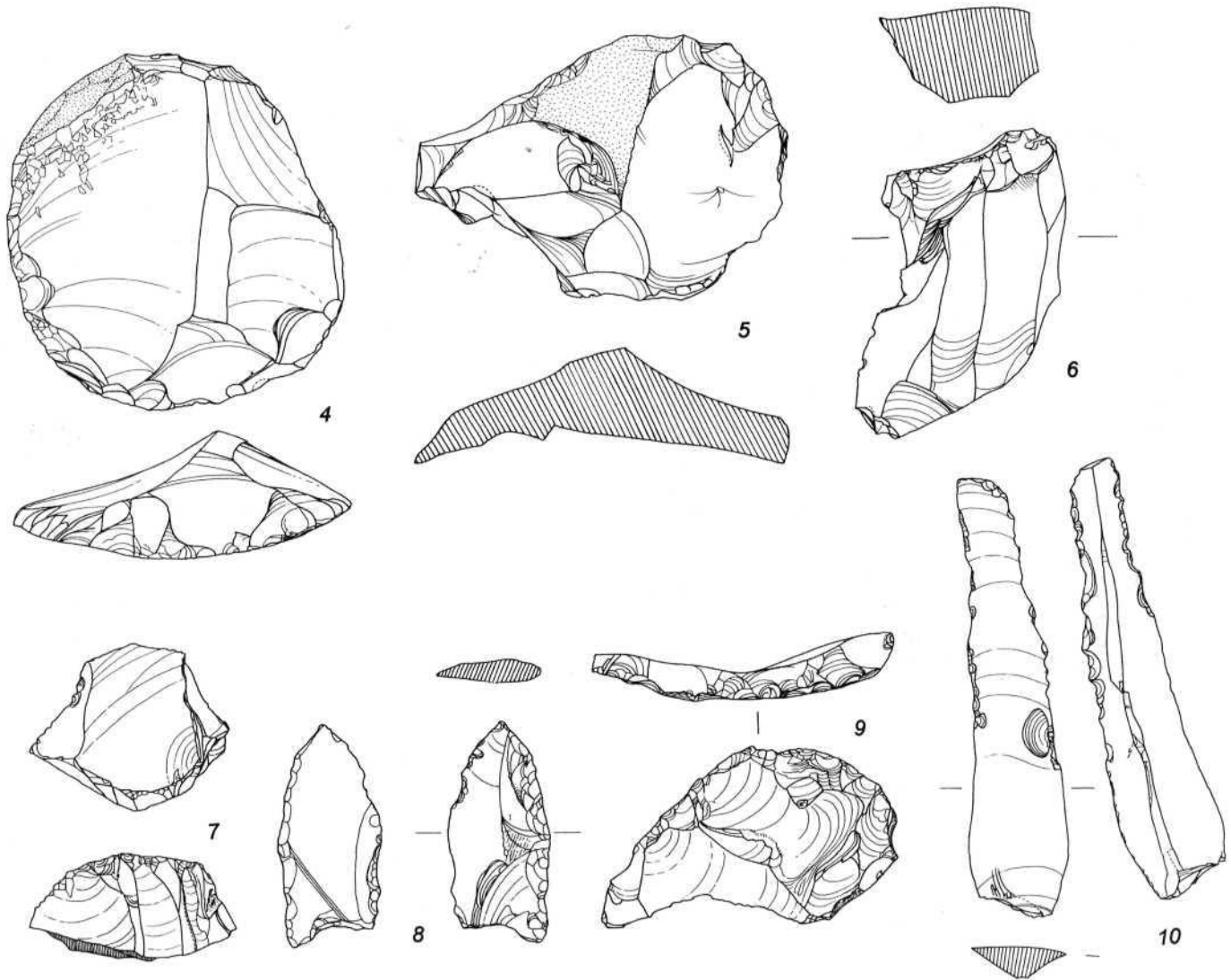


Fig 6.37 Pitchbury Ramparts 1973 (no 75). Selection of flints. 4... double-sided scraper; 5... flake; 6... core fragment; 7... core tablet struck across top of the core; 8... petit tranchet derivative arrowhead with unusually smoothed edges; 9... axe-sharpening flake; 10... thin blade with secondary working. Flints 4, 5, 6, and 10 are from F28, flint 7 from F41, flint 9 from F34 and flint 8 from the cover loam at 212 m. [Page 144]

76

Lexden Dyke Middle, excavation at Bluebottle Grove in 1987

by G Carter

TL 97382459

[Figs 6.38-6.40]

#### Introduction

Bluebottle Grove, a guardianship monument, is the remains of the south-eastern end of the Lexden Dyke. This is a defensive bank and ditch which dates from the Late Iron Age and is part of the defences of Camulodunum. The monument takes the form of a long wooded hollow, with a well-used footpath along the bottom.

At the request of the HBMC, a small excavation was undertaken to determine the original profile of the ditch and the extent of the bank. Information was required to assist in proposed repairs to the earthwork. This was needed as a result of the removal of trees in the 1960s

which had left several large shallow holes in the site.

The trench was positioned to avoid damage to the trees growing on the monument (Fig 6.38). A narrow trench (1 m wide) was dug to minimise the impact on the site. The trench had to be widened to 2 m at the centre of the ditch. This allowed shoring to be used, so that the original ditch bottom could be reached, 2.75 m below present ground surface.

The only previous work on the dyke was a section dug by Professor Hawkes in 1932 (*Camulodunum*, 9-19). This work took place within the unscheduled part of the dyke to the north of Bluebottle Grove.

#### Modern features

Feature 1 is a tank-trap dug during World War II along the inner lip of the ditch. The digging of this feature led to the destruction of the junction of the bank and the ditch. Where any evidence of timber reinforcement would have occurred, the feature had been backfilled with topsoil and building rubble.

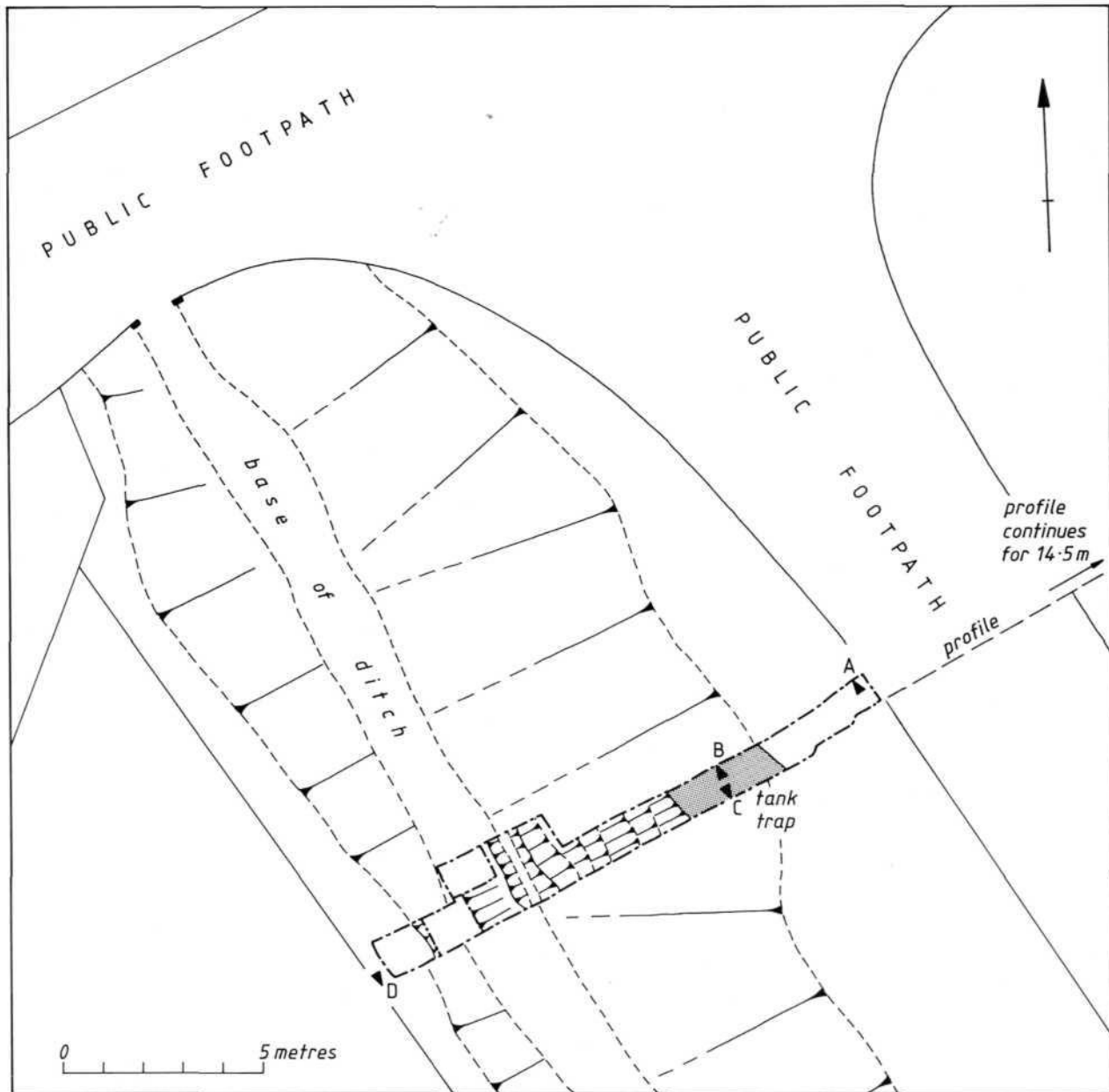


Fig 6.38 Lexden Dyke (Bluebottle Grave 1987) (no 76). The location and plan of the 1987 section. 1:155. [Pages 154-9]

Along the top of the bank, under a layer of modern levelling and topsoil (L4), is the remains of a gravel surface L5. This surface predates the construction of the fence and current tarmac path along the north-east side of the ditch.

On the opposite side of the ditch is a build-up of humic material (L23) which is up to 0.85 m deep. This deposit seems to be derived from garden waste thrown over the fence from the adjoining properties.

#### The ditch

The ditch (Figs 6.39-6.40) has a symmetrical 'V' profile cut into sand and gravel and, when dug, would have been about 10.6 m wide and 4.1 m deep (Fig 6.39). The average slope is about 38°, though at the bottom of the ditch is a much steeper cut, about 0.45 m deep. The latter is created

by a marked steepening of the inner slope to 58°. Since there is a slackening of the slope toward the bottom of the ditch, this feature is best seen as a deliberate attempt to improve the effectiveness of the ditch. An additional factor to be considered is erosion. If heavy rain occurred shortly after the digging of the ditch, run-off water could scour a deeper channel along the bottom of the ditch. If this occurred, the central part of the ditch could originally have been even steeper with erosion tending to lessen the slope. It is filled with sand (L21), which may well represent initial silting derived from loose sand left after construction.

The sand (L21) filling the very bottom of the ditch is sealed by L17 which is a 47 mm thick layer of clay loam. This contrasts strongly with the sandy layers above and below it, and could be seen as indicating a period of stability without quantities of sand and gravel being eroded into the



Fig 6.39 Lexden Dyke (Bluebottle Grove 1987) (no 76). The 1987 section. 1:60. [Pages 154-9]

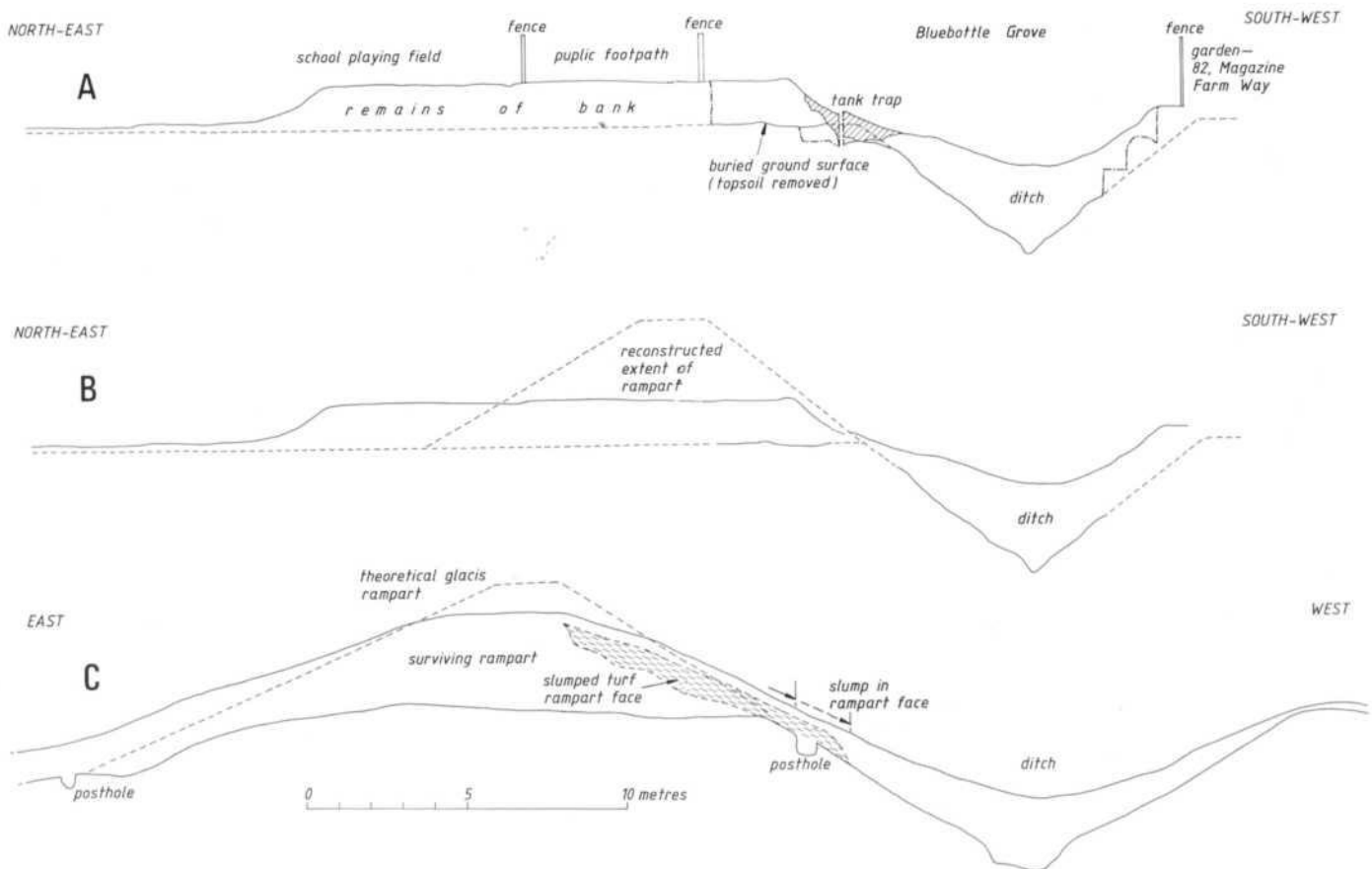


Fig 6.40 Lexden Dyke (Bluebottle Grove 1987) (no 76). A: simplified drawing of the 1987 section. B: reconstruction of the dyke at Bluebottle Grove. C: reconstruction of the 1932 section across the Lexden Dyke. [Pages 155-9]

ditch. This implies that the face of the bank and perhaps even the ditch had been stabilised. It is argued below that this would be achieved by facing the bank with turf.

The silting of the ditch is roughly divisible into two phases, although, in broad terms, it seems to have occurred fairly slowly and evenly. Predictably, initial erosion of the bank produced a greater volume of more sandy material on the inner slope. Layer 18 is the primary fill on the inner slope, and consists of sand and gravel, contrasting with L19 which is a sandy silt loam on the other side of the ditch. The situation in the centre of the ditch is more complicated. There, three layers (L16, L15, & L13) indicate that different soil formation processes occurred in the centre of the ditch. This is probably due to finer silt and clay particles washing into the centre of the ditch, which would also tend to collect humus. It is therefore understandable that L16 is a loamy sand and that the subsequent deposits L15 and L13 are sandy silt loam.

The second phase of silting is represented on the inner side of the ditch by L8, which is a sandy silt loam almost identical to L13 on the opposite side of the ditch. This similarity indicates that the differential silting caused by the erosion of sand and gravel from the bank was no longer occurring at a significant rate. This implies that the bank was now in a more stable condition. This gradual deposition of sandy silt loams seems to have continued to the present day, though L12 is a particularly stony deposit in the centre of the ditch for which there is no easy

explanation. The continued subsidence of the ditch is demonstrated by the topsoil-filled hollow in the centre of the ditch.

The fill of the ditch tends to indicate that the bank eroded fairly slowly with no evidence of collapse or deliberate spoiling. This strengthens the argument for a sloping 'glacia-style' rampart as opposed to a timber reinforced vertical or near-vertical rampart.

#### The bank

The remains of the bank (Figs 6.39-6.40) seal a buried ground surface 1.4 m below the current surface. A box section cut through this surface showed 0.4-0.5 m of a silty loam subsoil (the so-called 'cover loam') which appears undifferentiated, without any indication of an overlying topsoil. This apparent lack of topsoil indicates that the area may have been stripped of turf before the bank was built and that perhaps this turf was used to stabilise the top and sides of the rampart.

One feature in the buried ground surface is a slight depression up to 100 mm deep, behind the lip of the ditch. This may be a natural feature, or alternatively this could represent deformation of the ground surface due to the weight of the bank. If this is the case, it would be the only evidence for a steep-faced rampart concentrating its weight at the front.



The initial deposit in the bank is a redeposited cover loam (L14). This is followed by two layers of sandy loam (L24 & L10). Layer 24 shows a marked dip to the north west. This implies that material was being added to the rampart from the south-east. This suggests that this part of the ditch was dug from south-east to north-west.

Above the main bank material is a modern gravel surface (L5) which is covered by a layer of topsoil both of which probably relate to the construction of the modern tarmac path.

#### *Reconstruction of the bank*

Figure 6.40b shows a reconstruction of the bank at Bluebottle Grove where it has been taken to have been 3.9 m high to give a 13.0 m long slope to the rampart face. The rampart has been assumed to have been of the glacis style. This type is more stable and easier to maintain than timber-reinforced ramparts, and by the Late Iron Age was the preferred type (Cunliffe 1974, 227-39). Since the original width and height of the bank are unknown, the reconstruction is based on the amount of material derived from the digging of the ditch. With a glacis-style rampart, the angle of the front of the bank can be assumed to maintain the 38° angle of the ditch. The reconstruction also assumes that the tail of the rampart would be at 30°, this being the angle of the rest of the sand and gravel. This angle would maximise the height of the rampart, whilst maintaining stability and minimising erosion and slippage on the rampart tail.

The volume of material from the ditch was calculated on the assumption that the natural sand and gravel would, when dug up, bulk up from 2.2 tons to 1.75 tons per cubic metre. It is interesting to note that each metre of ditch (along its length) would produce about 50 tons of sand and gravel.

The bank in Figure 6.40b has been reconstructed assuming about 31 cubic metres of uncompacted material per linear metre. Theoretically this could be compacted by up to 20 per cent, but it is unlikely that any deliberate attempt would, or could, be made to compress the material to this extent.

The remains of the bank have a very flattened appearance, the bank being 35 per cent wider than the suggested reconstruction. This discrepancy cannot be explained away by simple erosion but suggests that a deliberate attempt was made to flatten the bank in the recent past.

The volume of the residual bank is 23 cubic metres per linear metre including the part which has spread out towards the east. This, plus the 3 cubic metres or so of bank material which has eroded into the ditch, gives a total volume of about 31 cubic metres of material derived from the ditch allowing for compaction of about 20 per cent. This proves that the residual bank (Fig 6.40a) is quite compatible with the reconstruction in Figure 6.40b.

The front unstable portion of the rampart (that portion in excess of the angle of rest) would, according to the proposed reconstruction, have been about 3 cubic metres in volume per linear metre. It is interesting to note that this is about the same as the amount of bank in the primary fill of the ditch, hence strengthening the validity of the reconstruction.

#### *Comparison with the 1932 section*

The only previous work on the Lexden Dyke was a section dug by Professor Hawkes near the entrance in 1932 (pp 35-40). There are significant differences between this section and that at Bluebottle Grove. Professor Hawkes's section shows that the ditch is 50 per cent wider, though the shallowness of the ditch angle (30/32°) means that at 5 m the ditch is only slightly less than 20 per cent deeper. The section shows a slot at the bottom, which, as at Bluebottle Grove, is steeper on the inner slope. This strengthens the argument that this is a deliberate feature to increase the effectiveness of the defence.

Figure 6.40c shows a reconstruction of the rampart, calculated on the same basis as Figure 6.40b, the reconstruction of Bluebottle Grove. It has been assumed that the post holes found by Professor Hawkes mark the extent of the bank. The reconstruction is therefore of a glacis rampart maintaining the slope of the ditch and built with material derived from the ditch.

One interesting observation on this section is that, if the rampart is defined by the post holes, then the centre of the bank is immediately above the crest of the small ridge visible in the buried ground surface. This is the optimum position for the bank, but for the top of a glacis rampart to coincide with the crest of the ridge would require either about 40 per cent more material than could be provided by the ditch or an unrealistically shallow angle of slope.

The presence of the post holes at the front of the rampart tends to suggest that the rampart may have been timber reinforced and hence may have had a steep front. No evidence of this style of construction was noted at Bluebottle Grove. Since the 1932 section was near the entrance, it may not have been typical in terms of structure. There is a possibility that the hole is a repair or reinforcement for a glacis-style rampart. If this post hole represents the remains of a revetment, even one of limited height, then it would have reduced the effectiveness of the rampart. A revetment would have created a blind spot and would have been susceptible to being fired.

In favour of a glacis-style rampart is the placing of turfs flat against the face of the rampart. This is incompatible with a more steeply-angled rampart which would require material laid horizontally for maximum stability. Also a steeply-angled rampart will, once the reinforcing timbers have rotted, collapse into the ditch, and this would be evident from the section. Given the shallow 32° angle of the suggested rampart face (which is close to the 30° angle of the rest for sand and gravel), much of the turf-facing may have remained intact. It could therefore be suggested that the turfs facing shown in the 1932 section have merely slipped down the face following the rotting of timbers represented by the post hole. The dimensions of this block would fairly closely fit the proposed glacis-rampart reconstruction. The exact nature of the timber-work represented by the post hole is, in the context of this reconstruction, best seen as holding in place the bottom of the turf facing of the rampart and thus would have extended upwards by only a little way.

#### *Conclusion*

It would be unwise to give too much weight to evidence obtained from a single section through such a large field monument. The digging of the tank-trap has destroyed the most important area of the rampart, ie where the bank and ditch meet. This serves only to cloud the issue of how the



bank was constructed in the light of Professor Hawkes's section which had a post hole in this position.

On balance the evidence is convincing for a glacis-style rampart stabilised probably with turf which eroded into the ditch at a fairly modest rate.

77

Barnhall Dyke

TM 00802350  
Excavation  
A F Hall  
1945  
Hall 1960, 10; Hall 1961

Three sections were dug across the ditch of the dyke. The ditch was found to be 25 ft wide. It appeared to be unusually shallow (4-5 ft) in two of the trenches but more as might be expected in the third (no depth given). Pottery near the assumed base of the ditch in Trench 2 was thought at the time to be Anglo-Saxon but is now dated to the Iron Age.

78

Berechurch Dyke

TL 99572156  
Excavation  
PC  
1984  
Fig 6.41

A section across the rampart and upper part of the ditch was exposed during the excavation of a trench for new services. The section was drawn (Fig 6.41) although no excavation was possible. Below the modern road, the rampart proved to survive to a height of a metre. The upper sides of the ditch indicated that it must be around 3.75 m deep and thus that the dyke was equivalent in scale to the major dykes on the west side of Camulodunum.

79

Berechurch Dyke

TL 99702040  
Excavation  
A F Hall  
1945-6  
p26

A section across the ditch and rampart in a wood called Charlotte's Grove. The location of the section within the wood as marked on Figure 6.1 is approximate.

80

Shrub End Dyke

TL 96572440  
Observation  
PC  
1979  
CAR 6, 924-5, fig 13.262 & 13.278; p 171

A trench was cut along Heath Road as part of the construction of a major sewer in the Lexden area. The full length of the trench was carefully examined at the west of the street and only one ditch could be found. It was 6.1 m wide and 3.2 m deep. Its size and alignment indicate that this was the ditch of the Shrub End Dyke. It appeared to align with the eastern ditch of the Triple Dyke as if it was a continuation of that part of the dyke.

81

Shrub End Dyke

TL 96602420  
Observation  
PC  
1984  
CAR 6, 1045-6, fig 13.46

The ditch of Shrub End Dyke was cut east-west in two

Sketch section across Berechurch Dyke

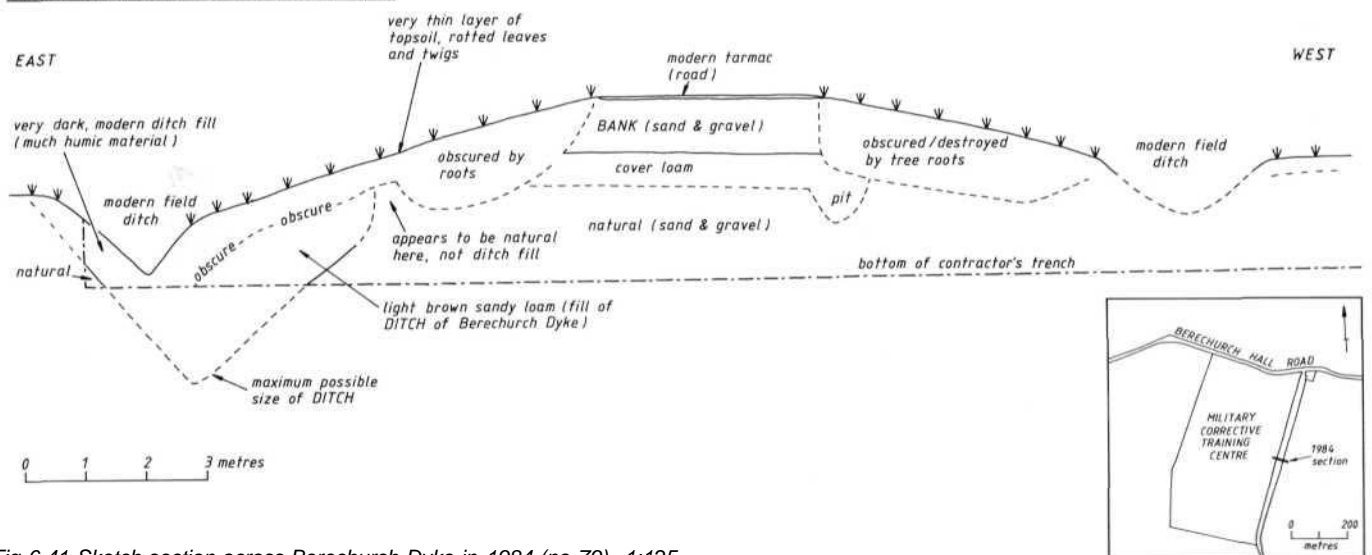


Fig 6.41 Sketch section across Berechurch Dyke in 1984 (no 79). 1:125.

places during construction work at 'Kingsmead' on Straight Road. As at Heath Road (no 80 above), only one ditch was observed rather than the three that would be expected if Triple Dyke had extended this far south. Neither trench cut the ditch to its full depth, but it was clear that the ditch was just over 3 m deep and 6 m wide.

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**82**

Heath Farm Dyke, 274-98 Straight Road

TL 96832360  
Excavation  
Carl Crossan  
1984

A trench, 18.1x 3.5 m, was excavated across the course of Heath Farm Dyke at the rear of 274-98 Straight Road. The base of the rampart was not well defined and the ditch proved to be shallow (approx 1.5 m deep from the surface of the field).

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**83**

Roman road, 26 Lexden Road

TL 98392490  
Excavation  
P R Holbert  
1966  
Holbert 1966

Two trenches were dug across the line of the road in the garden of 26 Lexden Road.

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**84**

Roman road, 24 Lexden Road

TL 98512487  
Excavation  
James Fawn  
1984

The Roman road was partly sectioned in the garden of Eastwell House, 24 Lexden Road. Although metalling was found, its edges could not be located precisely.

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**85**

Berechurch Dyke

TL 99972301  
Excavation  
A F Hall  
1944

A section in which the ditch and base of the rampart were located (p 26).

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## 7: Camulodunum - a review

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### Introduction

There are various important issues which, unfortunately, time did not permit Christopher Hawkes to reconsider in the light of more recent evidence and research. There is also a need for an overview of the current state of knowledge of the dykes and an account of the known burial areas. This chapter therefore is an addition to the volume as originally planned and offers observations on the following topics:

- 1) the relationship between Cunobelin and the Sheepen site,
- 2) the structure and function of the dykes,
- 3) burial sites,
- 4) the interpretation of some of the major earthworks,
- 5) the interpretation of the political history from coins, and
- 6) the origins and development of Camulodunum and its dykes.

### Cunobelin and the Sheepen site

The accession of Cunobelin and the construction of the Sheepen Dyke have been seen as linked events. This view was encouraged by the apparent start of occupation at Sheepen being put at cAD 5, the date at which the coin evidence was thought to point to the arrival at Camulodunum of Cunobelin. Earlier in this book (pp 52-4), CFCH repeats the argument in *Camulodunum* that there were two systems of dyke, one focused on Sheepen and the other on Gosbecks, and that these reflect two separate centres brought about by political change. This, added to the apparent richness and importance of the Sheepen site as shown by the finds, particularly the coin flan-moulds, made irresistible the idea that Cunobelin was its founder and that the site was the heart of his Camulodunum (*Camulodunum*, 16 & 29). However, such a personalised connection between Cunobelin and the establishment of Sheepen seems unlikely, since the latter was probably just one of a series of sequential developments within Camulodunum as a whole. Moreover the existence of two different dyke systems is not as clear-cut as CFCH believed, because its identification depended to a large degree on the position of a part of the Shrub End Dyke which now does not appear to have existed (see further on pp 170-71 below).

The start-date for the occupation at Sheepen has provoked much debate. However, freed from the need to link the first occupation with the arrival of Cunobelin, the problem ceases to be as important an issue as it once was thought. The apparently disproportionate number of Dressel 1 amphoras is the nub of the difficulty. The trouble is that there has as yet been no detailed numerical comparison between Sheepen and another site which started in the late Augustan period and ended a few decades later. We might suspect that if there had been, the same high numbers of Dressel 1 would be seen to occur there too. Paul Sealey has surely rightly pointed to the explanation as being related to the fact that amphoras could be in use long after they were made (Sealey 1985, 101-8). This phenomenon must apply to all sorts of ceramics, but is made apparent with amphoras because the painted inscriptions which occur on some of them allow fairly accurate fixing of the date when their production ceased (Sealey 1985, 25-6).

CFCH tells us that up to his death M R Hull favoured c 5 BC for the start-date of Sheepen which, of course, would inevitably sever the special link argued for the site with Cunobelin. Colin Haselgrove has suggested that occupation began first near the river and then expanded uphill later (p 77; Haselgrove 1987a, 163-71). Certainly the pattern and dating of the occupation are likely to be more complicated than currently perceived, since Rosalind Niblett's large excavation of parts of Hawkes and Hull's Regions 3 and 4 showed the occupation there to be almost wholly post-conquest (Niblett 1985, 1), which seems to be at variance with the earlier work in those areas. In Rosalind Niblett's excavations, the pre-Roman occupation was indicated mainly by large quantities of residual material in Roman deposits, not by contemporary cut features and layers.

A critical issue in terms of the development of Camulodunum is not only when and where occupation started at Sheepen, but when the Sheepen Dyke was built. The construction of the dyke and the start of occupation on the site do not of course have to coincide. While it is unlikely (but obviously not impossible) that the dyke was built some time after the start of occupation at Sheepen, the start of the occupation could have followed the building of the dyke by many years. This offers no explanation for the large proportion of Dressel 1 amphoras at Sheepen, but it does mean that the building of the dyke does not have to be dated by them.

The only direct dating evidence for the construction of the Sheepen Dyke is provided by 'sherds of two bowls, f. 217, 218, of soapy-smooth superior ware, and one of

a coarse store-jar, f. 271' and 'a few indeterminate scraps' in the rampart base or under it (*Camulodunum*, 60). Unfortunately this pottery cannot now be re-examined so its value is limited. However, from its description, the material would seem to be different to the pottery from the early phase of the Lexden cemetery and therefore later than it, although we cannot be sure that this does not reflect the difference between a domestic assemblage and a funerary one. A sherd of Cam 218, which was found in the Lexden Tumulus, is datable to c 15-10 BC although there is some uncertainty about its stratification because of its 'Romanised' appearance and the fact that only one sherd now exists (Foster 1986, 116-18 & 123). Nevertheless, on the basis of Hawkes and Hull's description of the pottery in and under the bank, it seems that c 50 BC is the very earliest date that the dyke could be, and that probably it was sometime later than this.

## The dykes

### Function and size

CFCH gave most of the dykes their names. His rule seems to have been that any earthwork which was constructed, or at least appeared to have been constructed, as a linear defensive earthwork was to be labelled a 'dyke'. The rule was not applied to earthworks where the function was less obvious, ie the 'Laver ditch', the 'palisaded earthwork', and the Prettygate Dyke in its Phase 2 form (or 'Prettygate Dyke reversed' as CFCH referred to it).

It is generally assumed that the dykes were anti-chariot devices and certainly they would have been very effective against such vehicles. They would have been less so against mounted warriors, unless there were wooden palisades along the tops of the ramparts (which for this very reason there could have been). Berechurch Dyke is often thought to have provided Camulodunum with protection from sea-borne forces, although it was more likely intended as a defence against a land-based assault from the south-east.

Unlike hillforts which were comparatively small, Camulodunum was too large to besiege easily and open enough to allow the attacked to escape if needs be. Its open, unstructured layout was its strength. It was built like an obstacle-course with different lines of defence, each giving those under attack time to retreat and regroup or flee. Caesar's account of his assault on the stronghold of the British leader Cassivellaunus is very apposite here.

*'Cassivellaunus' stronghold ... was protected by forests and marshes, and had been filled with a large number of men and cattle. He marched to the place with his legions, and found that it was of great natural strength and excellently fortified. Nevertheless, he*

*proceeded to assault it on two sides. After a short time the enemy proved unable to resist the violent attack of the legions, and rushed out of the fortress on another side'(Caesar, *Bell Gall*, v. 21, 2-5).*

This description could have applied to Camulodunum and, as will be seen below (p 174), there is even a slight possibility that it did just that.

The dykes were probably also useful for the management of stock. Coupled with the Colne and the Roman River, the dyke system could have been used to contain large numbers of grazing animals and give them some protection against theft. Cattle raiding may have been a problem in the late Iron Age just as it was, for example, across the Scottish and Welsh borders at other times in the past. However, the arrangement of dykes on the western side of Camulodunum as sequential and consistently west-facing shows clearly that these earthworks were intended primarily as defensive structures.

The dykes vary a great deal in size although the largest of them (Gryme's, Kidman's, Berechurch and Lexden/Moat Farm) are much the same, all having ditches around 4.0 m (13 ft) or more deep. Surprisingly perhaps, Sheepen Dyke seems to have been distinctly smaller than this group, with a ditch of around 3 m (10 ft) in depth. As far as can be judged, the ditches of the others seem to have been between 1.7 m (5.5 ft) and 2.4 m (8 ft) in depth.

It is difficult to tell how significant, if at all, size really was. The Laver ditch and the ditches of the 'palisaded earthwork' and Prettygate Dyke Phase 2 are all much the same — around 1.7 m (5.5 ft) deep. All of them are questionable as defensive works. Yet they are also similar to the ditches of the Dugard Dyke and Heath Farm Dyke (of which the latter at least was surely primarily defensive). Thus it would appear that, for smaller earthworks, depth alone is not necessarily an indicator of function. And nor did size have any bearing on date, with the construction of the smaller dykes (Heath Farm Dyke, Shrub End Dyke, and Dugard Dyke) being distributed throughout the dyke sequence.

### Relationships to topography

Unlike size, relationships to the topography do show differences in terms of date. Some earlier dykes were sited to overlook rising ground where they follow the contours. This is clear with Heath Farm Dyke, Kidman's Dyke, and the Lexden Dyke. On the other hand, the later dykes seem less concerned with contours. Some of them were laid out as straight lines incorporating sharp bends (ie Gryme's and Berechurch/Barnhall). This practice seems to betray the influence of Roman surveying as evident in Roman roads. Indeed one of these late dykes, Gryme's Dyke South, is unusual in that the land immediately behind it slopes downwards away from it. This reinforces the view, apparent from its relationship with the dykes to the east, that its purpose

was to protect the Gosbecks site rather than the land immediately behind it.

Some of the dykes blocked valleys (ie Lexden/Moat Farm, Oliver's/Layer, Berechurch/Abberton). Some, either singly or in combination, provided barriers stretching from the river Colne to the Roman River (notably Gryme's, Heath Farm with Lexden, Heath Farm/Kidman's with Triple Dyke/Shrub End, and Berechurch/Barnhall). North of the river, greater reliance was placed on river valleys to provide protection, with 'The Rammers' linking the ends of Salary Brook and the Black Brook (p 50). Pitchbury hillfort, although close by, does not seem to have been part of the dyke system. Thus presumably not only did the hillfort predate it, but the dyke-builders did not consider Pitchbury worth incorporating in their work. The system's extension south of the Roman River may have consisted only of the valley-blocking dykes Layer and Abberton.

The dykes were presumably sited to protect crossing-points over the Colne and Roman River. There is no evidence where these places were, although it may be significant that today there are crossing-points at most of the places where dyke meets river. The river valleys, and the woods and marshes along them, must have been more effective defensively than the water-courses themselves. The Roman River in particular must have been quite a minor affair and easily crossed anywhere along its course. Caesar's description of his infantry wading across the Thames up to their necks in water (Caesar, *Bell Gall*, v. 18, 5) shows that the Romans at least would have had no difficulty in crossing either river at any point they chose.

### Structure of the ramparts

The structure of the ramparts is uncertain although they seem most likely to have been of the 'glacis-style' (p 158). There is some limited evidence for the use of timber and turf, so that the dykes need not all have been built in the same way, or indeed of the same construction throughout their lengths. Only the ramparts of the Sheepen Dyke, Lexden Dyke, Prettygate Dyke (p46), and Gryme's Dyke have been examined in any detail and nowhere was the evidence conclusive. In each case there were some indications of a front revetment which, at the Lexden Dyke (p 37) and possibly the Sheepen Dyke (*Camulodunum*, 60), seems to have been of turf or clay. The section across Gryme's Dyke contained a rather tenuous indication of a vertical or near-vertical timber revetment but no turf or clay (pp 109-110). The large post found in 1932 in the Lexden Dyke is the best evidence to date for a timber revetment, but CFCH thought this was perhaps a localised arrangement made necessary by a loose core (p 40). Following the 1987 section across the Lexden Dyke, G Carter suggested that the front of the rampart was faced with turf and had a slope of 32°. Like CFCH, he prefers to see the post found in 1932 as part of a localised aberration (p 158).

There is no evidence for the presence or absence of

palisades along the tops of the ramparts and, as far as can be judged, there were no berms (but see p 40) between the ramparts and their ditches.

### Gateways

Like revetments, the evidence for wooden gateways is patchy and inconclusive, and their presence is yet to be firmly established anywhere. It is possible that many entrances were simply blocked by felled or fallen trees in times of threat.

Six entrances through dykes have been excavated, three through the Sheepen Dyke (*Camulodunum*, 60, 110, 81) and others through Gryme's Dyke (pp 27-9), Heath Farm Dyke (p 31), and the Lexden Dyke (pp 42-4). Post holes, apparently for a timber gateway, were found in the entrance at Gryme's Dyke (p 28, Fig 2.13). The plan is uncertain and incomplete, there being apparently only three post holes known. CFCH believed there to have been a timber gateway at Lexden Dyke, in the entrance excavated in 1932. No structural remains were found, but the steepness and size of the butt ends of the rampart suggested to CFCH that they must have been supported by a timber gateway which was more than simply a pair of posts flanking the entrance (p 43).

### Areas of occupation

The main areas of occupation within the dykes were Gosbecks and Sheepen. As already discussed (pp 97-8 & 104-5), Gosbecks seems primarily to have been agricultural in character whereas Sheepen seems more concerned with trade and manufacturing, and substantial markets are likely to have been held periodically in both places. Another important occupation area may have existed to the north of the Barnhall Dyke but little is known about it (p 170).

The extent to which Sheepen was inhabited in the pre-Roman period is problematic and must remain so until further excavation. The 'occupation sites' in *Camulodunum* (such as site A5, p 91) are as yet not recognisable as a house-type elsewhere but they remain the only candidates for pre-conquest habitations on the site. The question remains therefore of whether post holes and other relatively slight indications of such buildings could have been missed in the 1930s work. Certainly the excavation of 1971 clearly shows this to be possible with the finding of post holes and other features between trenches of the 1930s (Fig 6.20). *Camulodunum* was, and still is, outstanding for its treatment of the pottery and the historical evidence, but we should remind ourselves that the book had to be bedded on the excavation techniques of its time. The site was explored by digging

trenches and opening out where the sides seemed interesting. Picks and shovels rather than trowels were the preferred tools. Although reliance for skilled input was placed on volunteers, particularly university students, much of the work was done by inexperienced workmen drawn from the Corporation work gangs and the dole queue.

## Burial sites

### Lexden cemetery

[Figs 7.1-7.5]

The Lexden cemetery has never been published although references to it have been made from time to time, notably in *Roman Colchester* (pp 252-3) and

*Camulodunum* (pp 13-14), and Anne Birchall (1965) and Isobel Thompson (1982) both include and discuss some of the Lexden pottery in their surveys of such material from south-east England. Mr M R Hull started to prepare an inventory for publication of all Colchester's burial-groups but, although he documented over 600 burials, the task was never completed (CAR 9, 257-61).

More recently, it proved possible to plot many of the discoveries in the Lexden area with the aid of early street directories and other records and, as a result, a hitherto undetected cluster of early graves dating to within the period c 50-10 BC has emerged. The group consists of around 27 vessels from an area no more than about 75 m across (Fig 7.1). Unfortunately none of the vessels were excavated in an archaeological fashion and identification of individual grave groups is very uncertain. At least ten groups can be identified but some must represent more than a single grave. Recognition of the cemetery is significant since not

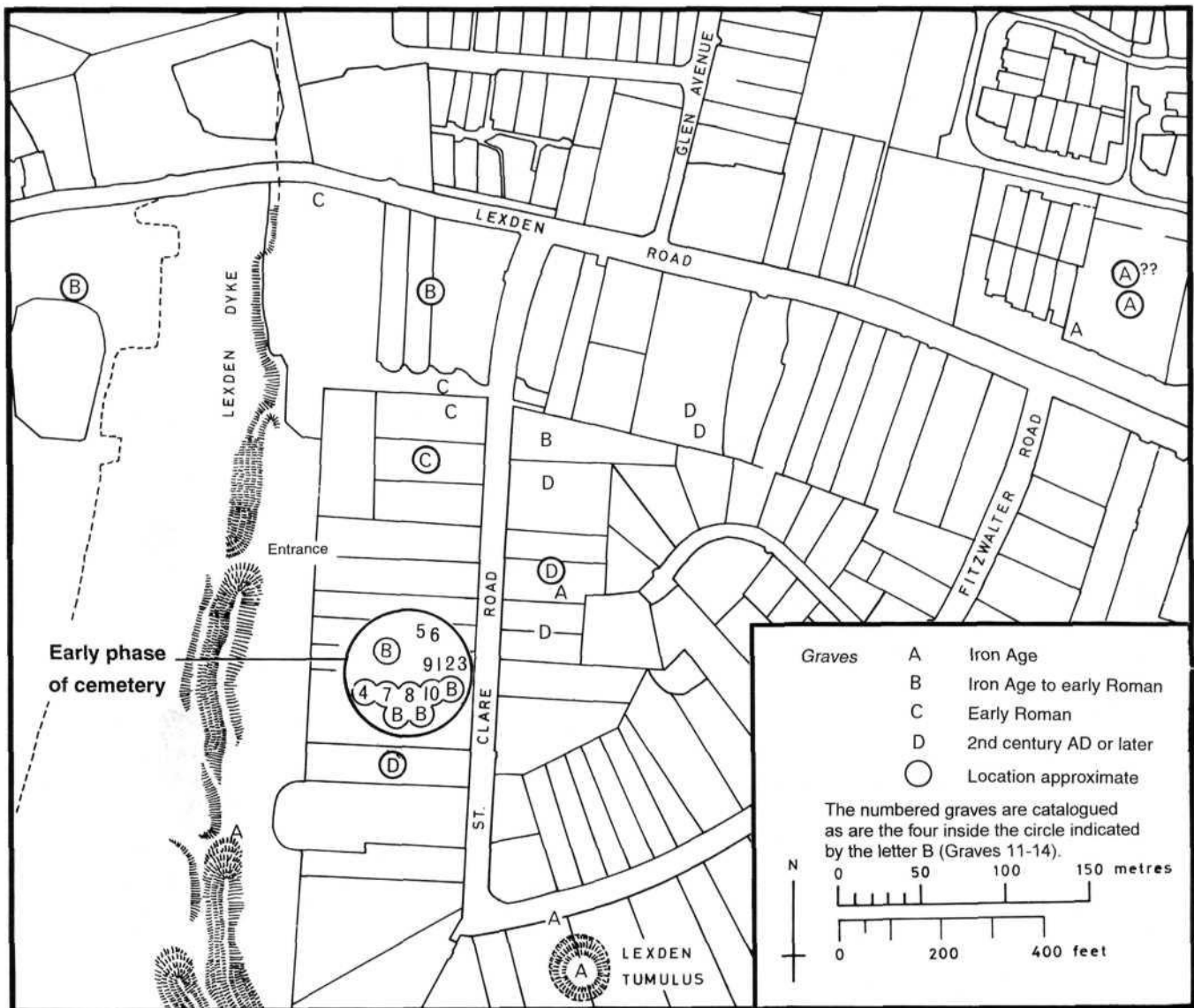


Fig 7.1 The Lexden cemetery. [Pages 164-9]

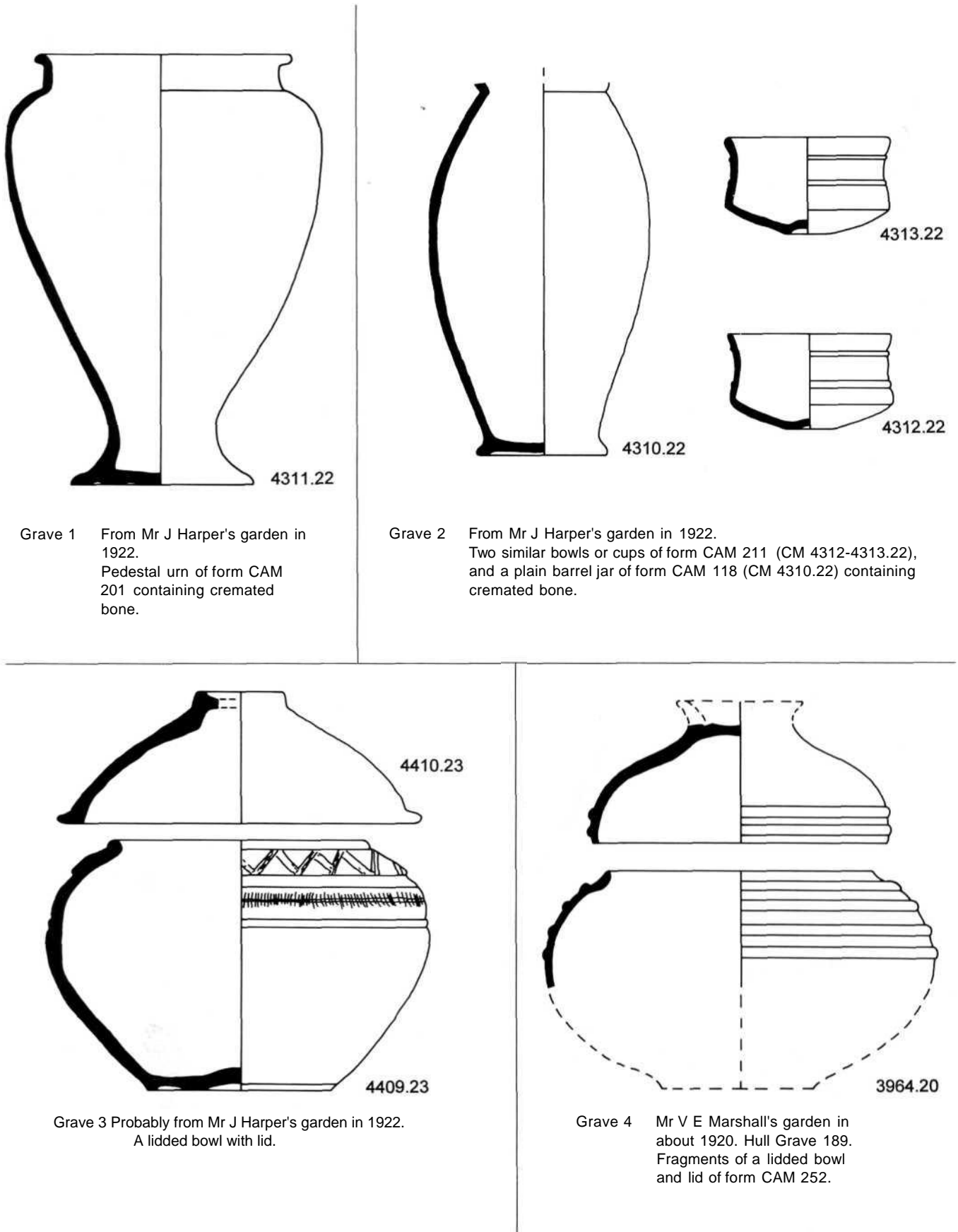
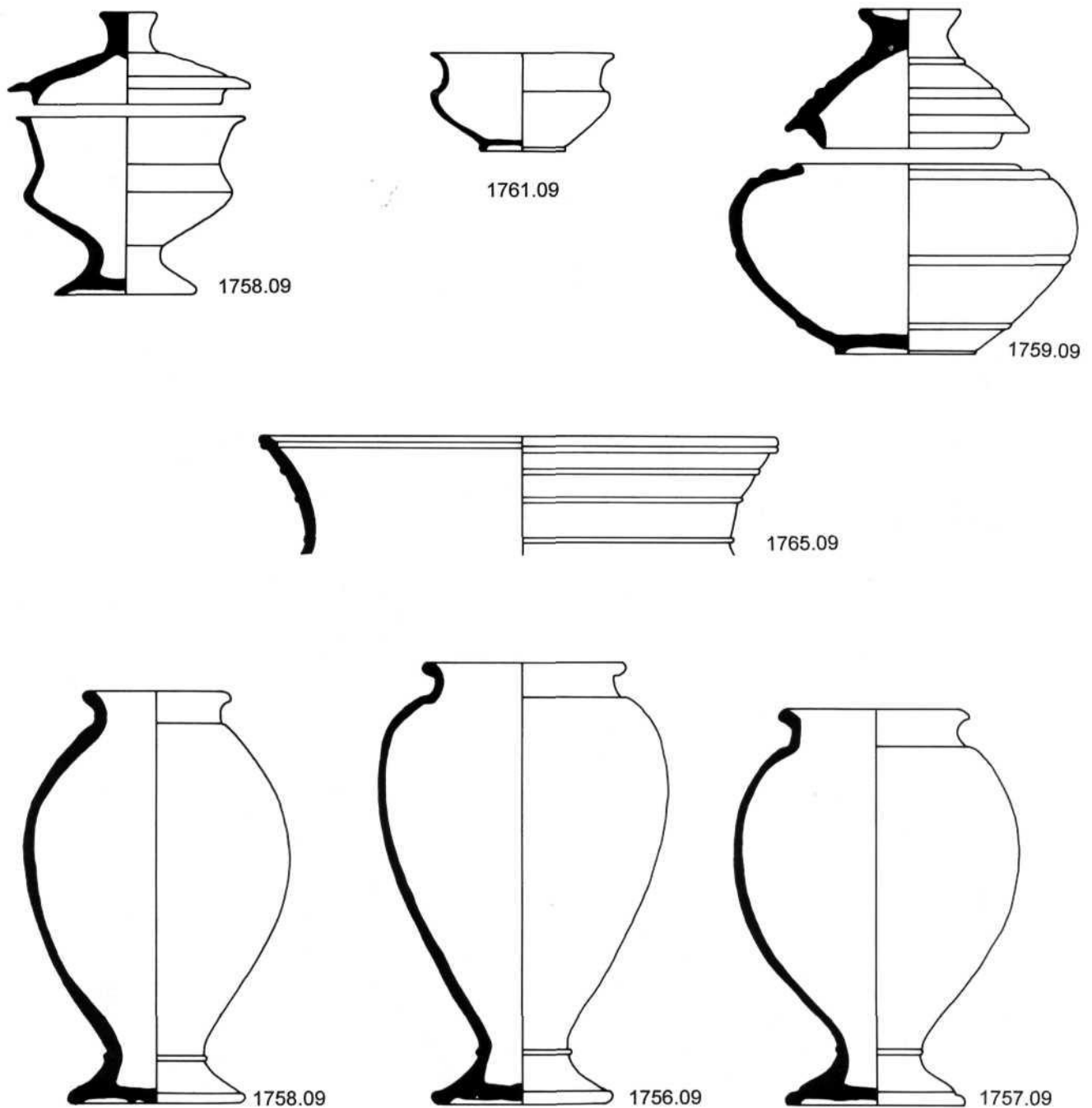


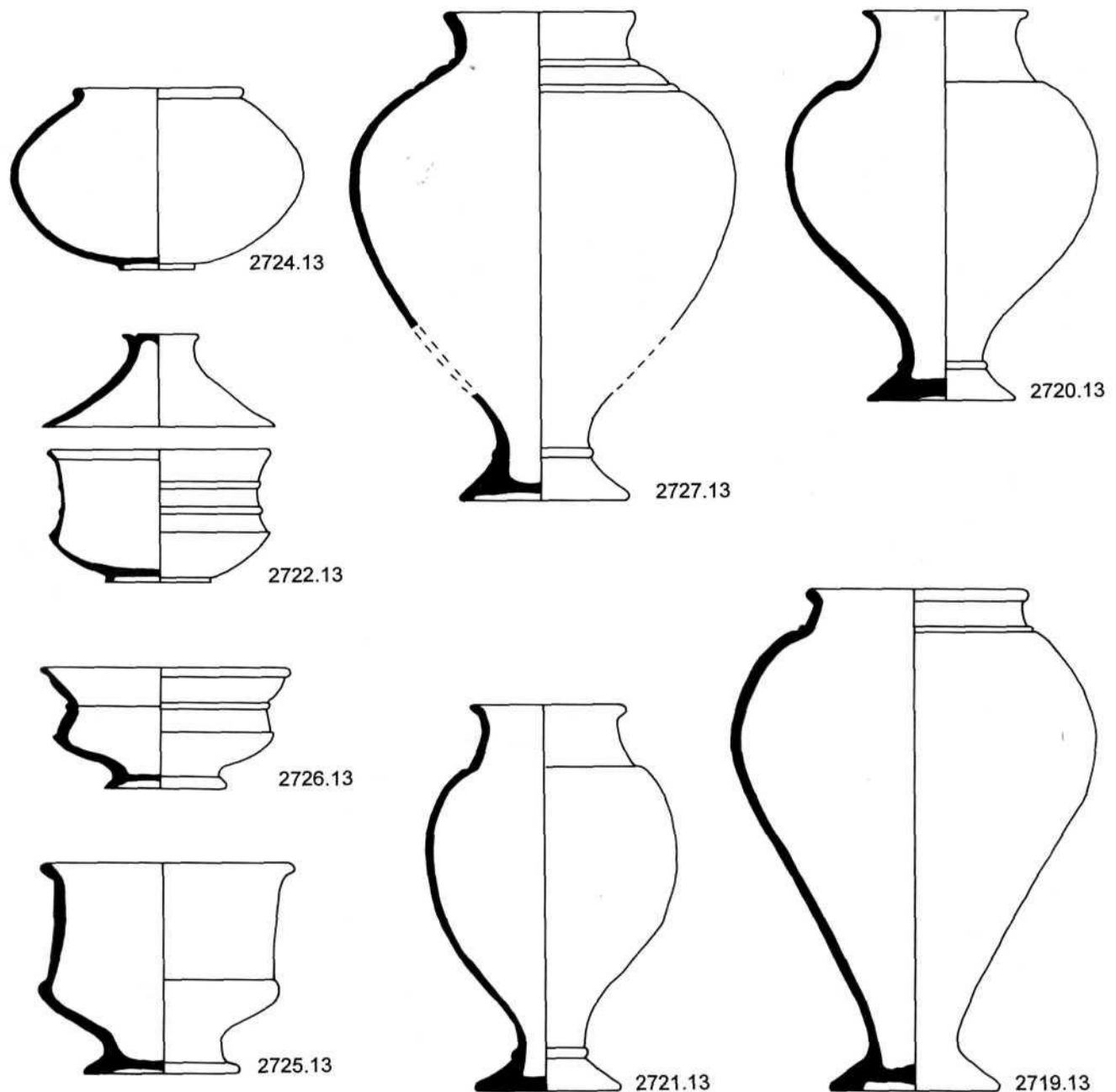
Fig 7.2 Possible grave groups from the cluster of early graves at Lexden. 1:4. [Pages 164-9]



Grave 5 All 'found during recent excavations in Lexden Park' (CMR 1909, 4). Almost certainly found in 1908 at the spot marked on the Ordnance Survey's 25-inch map of 1920-21 where 'Celtic urns were found in 1908-13'. Seven pre-conquest vessels plus sherds (CM 1766.09), presumably representing more than one grave. Hull's Grave 166. One pedestal urn of form CAM 201 (CM 1758.09), two pedestal urns of form CAM 202/3 (CM 1756.09 & 1757.09 resp), a cordoned bowl and lid of form CAM 252 (CM 1759.09), a small bowl or cup probably of form CAM 221 (CM 1761.09), a small pedestal bowl which has a lid and cannot be paralleled at Sheepen (CM 1760.09), and a fragment of a shale tazza (CM 1765.09). The group contains three post-conquest pots (CM 1762-1764.09). At least one (CM 1764.09) was not found with rest, but when a water-main was being laid in Lexden Park.

Fig 7.3 Possible grave group from the cluster of early graves at Lexden. 1:4. [Pages 164-9]

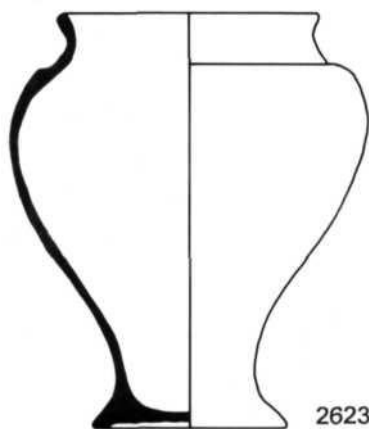




Grave 6 Almost certainly from the spot marked on the Ordnance Survey's 25-inch map of 1920-1 where 'Celtic urns were found in 1908-13'. Presumably from several graves. Hull's Grave 174.

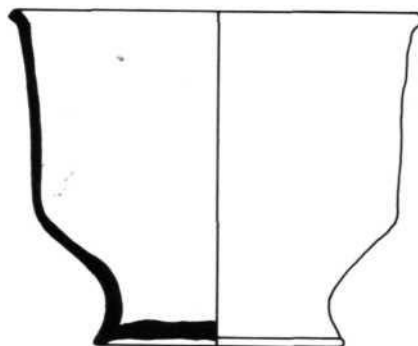
A pedestal urn probably of form CAM 201 (CM 2727.13), two pedestal urns of form CAM 201 (CM 2720.13 & CM 2721.13), a pedestal urn of form CAM 202/3 (CM 2719.13), a bowl or cup and lid of form CAM 211 (CM 2722-2723.13), fragments of a shale bowl of form CAM 210 (CM 2726.13), a pedestal bowl of form CAM 214b (CM 2725.13) and a jar similar in form to CAM 249 (CM 2724.13). (The museum holds a wooden copy of a pot (CM 2861.13) which probably represents the shale vessel CM 2726.13.)

Fig 7.4 Possible grave groups from the cluster of early graves at Lexden. 1:4. [Pages 164-9]



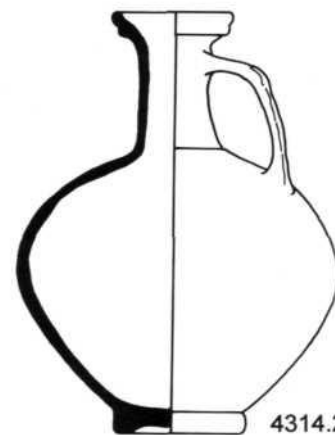
2623.12

Grave 7 From Lexden Park. Given to the museum in 1912 by Dr P Laver and thus likely from Mr V E Marshall's garden. Hull's Grave 172. Pedestal urn of form CAM 202-3 containing cremated bone.



2606.12

Grave 8 From the site of Mr V E Marshall's house in 1912. A variant of form CAM 214b. Hull's Grave 174.



4314.22

Grave 9 From Mr J Harper's garden in 1922. Buff flagon with 3-ribbed handle (form CAM 136A) and a so-called 'bucket'. The meagre remains of the latter show that it was a good deal more complicated than a bucket. Needs research.

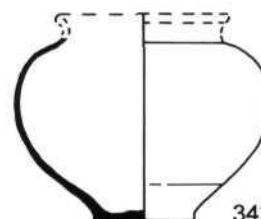
Grave 10 From Mr V E Marshall's garden in 1922. Lower part of a small late Iron Age pot with a cordon on the body and a small bead on the base (CM 4336.22). The fabric hard, brown, and micaceous and the pot is burnished externally including the base. Not illustrated.

*Probably all post-conquest:*

Grave 11 Presumably from the garden of Mr V E Marshall in or before 1916. Hull's Grave 177. A pot of form CAM 266 (CM 3420.16) and a variant of form CAM 234 (CM 3419.20).



3419.16



3420.16

Grave 12 Found in Mr J Harper's garden in 1922. Pot containing a cremation found 'not far from the pedestal urn CM 4311.22' (CMR 1923, 17) (part of our Grave 1). Hull's Grave 198. Form CAM 268 and 'of grey ware'. Not illustrated.

Grave 13 Given to the Museum by Mr V E Marshall in 1917. No details of the discovery or find-spot but the pot presumably came from the donor's garden. Hull's Grave 178. Pot of form CAM 266 (CM 3576.17) containing a cremation. Not illustrated.

Grave 14 Given to the Colchester Museum by Mr V E Marshall and therefore presumably found at the same time as Grave 14. Hull's Grave 179. A pot of form CAM 266 (CM 3577.17) containing a cremation and the remains of a small *unguentarium* of green glass. Not illustrated.

Fig 7.5 Possible grave groups from the cluster of early graves at Lexden. 1:4. [Pages 164-9]

many cemeteries of this period are known. Characteristically all are small (Fitzpatrick 1994, 110), the Lexden group being comparatively large.

All the material from the Lexden area deserves full publication, but in the meantime a summary of the graves from the early cluster is given here in Figures 7.2-7.5. Two of the vessels are shale, the rest pottery.

Anne Birchall's (1965, 310-11 & 343-5) and Isobel Thompson's (1982, 759-66) appraisals of material of this kind suggest that none of the pottery forms present in this group need be of pre-Caesarean date. Moreover the absence of Gallo-Belgic imports provides an approximate *terminus ante quem* of c 15-10 BC when the factories which produced these wares were established (Camulodunum, 202-3).

The cemetery cannot be related with confidence to the dyke system and its evolution. It might appear that the early cemetery at Lexden is likely to post-date the construction of the Lexden dyke since all its burials lie within the protected area, just to the south of the trackway which presumably passed through the entrance close by. However the cremated bone, found during the 1932 excavations in Lexden Park (p 42, and indicated by the letter A on Fig 7.1 south-west of the encircled area), showed that at least one cremation had been buried in the area before the dyke was

constructed. Thus the bones are critical, although unfortunately the pit containing them was said to lie under the tail of the rampart rather than under the main body of it, and there must therefore be some doubt about its relationship with the dyke. The Lexden Tumulus, dated by Jennifer Foster to c 15-10 BC (p 88; Foster 1986, 178), is not part of the early cemetery and presumably post-dates it.

The later burials (Fig 7.1) range in date from the late 1st century BC to the 3rd century AD or so, most being of the 1st century AD. They show no obvious clustering, although overall the Lexden burials form a discrete group in their own right in terms of the known burials from the town. Their distribution thus justifies the identification of the 'Lexden cemetery' as an area characterised by the presence of burials although, like many of the so-called cemeteries in Colchester, it is very doubtful if that was ever the area's sole use (CAR 9, 261).

### Stanway burials

[Fig 7.6]

When the excavation of the high-status funerary site at Stanway is finished, the results are likely to have a

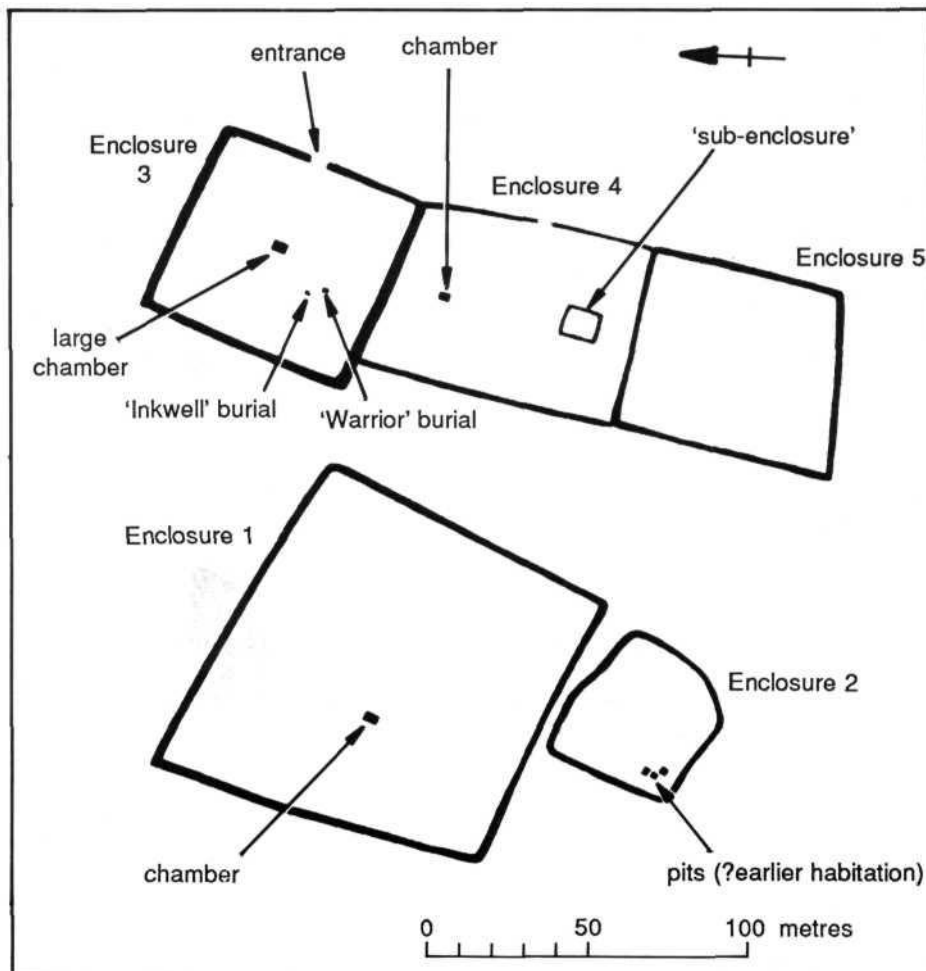


Fig 7.6 Simplified plan of the enclosures and burials at Stanway, drawn before the excavation of Enclosure 5 and most of Enclosure 2. [Pages 169-70]

considerable impact on attempts to understand the development and political history of Camulodunum.

The site lies outside the dyke system, about 1.0 km due west of Gosbecks. Its excavation is to be in two phases, the first being completed in 1992, the second yet to start. There are five ditched enclosures of which three and part of another have been excavated to date. At least four of the enclosures contained the remains of nailed wooden mortuary chambers, all being placed either centrally or axially within their enclosures. The grave goods were smashed in antiquity and, together with small quantities of cremated human bone fragments, were scattered through the backfill. At least one of chambers (that in Enclosure 3) was destroyed before being backfilled. It was burnt at the same time. Provisionally it seems as if the earliest of the excavated chambers (the one in Enclosure 1) dates from the late 1st century BC. This was followed by the chamber in Enclosure 3 (c AD 25-35), which in turn was followed by that in Enclosure 4 (c 60 or later). The latter was probably for a woman since it contained beads from a broken necklace.

Enclosure 3 contained two secondary burials in square pits without chambers. The grave goods were intact and provide dates for both burials of around c AD 50-60. One grave contained an inkwell indicating literacy and the other a substantial collection of imported and other objects including arms in the form of a spear and possibly a shield.

Considered discussion of the implications of the Stanway site must await the completion of the excavation and the analysis of the dating and other evidence. However, the small number of burials concerned and the elaborate nature of the rite suggest that the dead were all related members of the local British elite. If the earliest of the chambers really does date from the late 1st century BC, then this would suggest that during the decades leading up to Cunobelin there was some continuity and stability amongst the ruling class as opposed to the change advocated in the past and repeated here by CFCH (pp 88-94 & 52-4). Moreover the continued practice after the Roman conquest of what must have been a very public burial rite, and the appearance of arms in one of the secondary graves, both indicate that the people concerned enjoyed special privileges and no doubt Roman favour. In return a pro-Roman faction such as this, with appropriate ancestry, could have played a part in helping the Roman authorities manage and control the newly-formed *civitas*.

### **Barnhall cemetery**

A group of vessels on the east side of Camulodunum indicates the presence of what is likely to have been a substantial burial area. The material needs to be brought together and studied as a group, particularly since it is likely to be associated with an important area of occupation in the vicinity. The provenances of the

vessels suggest that if such an area of occupation existed, then it was next to the Colne (perhaps with a function like Sheepen) and north of Barnhall Dyke. It would explain the presence and location of the Barnhall Dyke.

The find-spots are shown on Figure 6.1. Nos 1-4 indicate the sites of four possible burial groups from the Borough cemetery in Mersea Road (CM 50-51.1946; CM 91-92.1946; 129.1946; CM 1.1948), no 5 shows the location of a pedestal urn from a sand pit in Bourne Road (CM 505.1963), no 6 relates to a lidded bowl with copper-alloy and iron rings from Winsley's Almshouses (CM 1001.1805, Birchall 1965, no 211), and no 7 shows the find-spot on the Abbey Field of two vessels of Sheepen Period 1 (CM 851-852.1904, Birchall 1965 nos 171 & 172).

## **Some interpretative problems**

### **The supposed dual dyke system, and the Triple Dyke and the Shrub End Dyke as one work**

CFCH believed that the dykes embodied two distinct systems: a Colne-based system centred on Sheepen and a separate system focused on Gosbecks (pp 52-4). Parts of the Colne-based system appeared to lie directly between the Sheepen and Gosbecks sites, suggesting that the latter was at some point excluded from the main defensive 'umbrella'. Warwick Rodwell developed this idea and postulated six phases of development for the dykes as a whole (Rodwell 1976, 339-59). However recent observations, which CFCH and Warwick Rodwell were not aware of, have provided evidence which indicates that such earthworks as there were directly between Sheepen and Gosbecks dykes were not as substantial as once thought and not significant enough to argue for a down-grading of Gosbecks in favour of Sheepen.

The Colne-based system consisted of Sheepen Dyke, Lexden Dyke, and the Shrub End Dyke, whereas the Gosbecks system was made up mainly of Heath Farm Dyke and Kidman's Dyke. The two stretches of earthwork thought to lie between the Sheepen and Gosbecks are Shrub End Dyke South and Lexden Dyke South. These are critical to this view since they would have had the effect of cutting off Gosbecks from Sheepen. However in the case of Lexden Dyke South, the part of the dyke concerned is of reduced scale, and Shrub End Dyke South probably did not exist at all.

The ditch of Lexden Dyke South was about 8 m wide, showing it to have been more substantial than Heath Farm Dyke but distinctly smaller than Lexden Dyke Middle. Its course was traced for about 75 m, there being no indication of its full eastern extent. Shrub End Dyke South did not extend south of where it met Heath Farm Dyke and may not even have continued beyond Kidman's Dyke. The Borough perambulation of 1671

has been taken as the best evidence of the southwards continuation of Shrub End Dyke. The section concerned is as follows:

*And from thence up along the road homewards on Kingsford Heath, to a gate and barres called Kingsford Heath Gate; through which is the turning out from the said heath to a lane or way leading by Gosbecks to Lexden Heath all along by the side of the hedge, under which there is a ditch called Grymes Ditch to Peddef's Cross; which is the three way leet, one of which points down to Stanway Hall, another is the way we went by Gosbecks, and the other to Colchester by the Shrub Wood side (Morant 1748, Bk 1, 93).*

The perambulations are written accounts of the boundaries of the Borough whereby each place is described in the order in which they are encountered on an anti-clockwise tour. The foregoing extract has been taken to mean that there was a dyke by the side of the lane leading north-west to Pedder's Cross at the junction of what is now Gosbecks Road and the Maldon Road (Rodwell 1976, 343-4, following others). The fact that the dyke is called Gryme's Dyke in the perambulation is glossed over. However the perambulation does not provide evidence for a ditch in this position but does in fact refer to the Gryme's Dyke that we know by that name today. The section 'all along by.... called Grymes Ditch' is out of sequence and refers to the Lexden Heath, which was of course bordered by Gryme's Dyke.

The absence of the Shrub End Dyke south of Heath Farm Dyke has been corroborated on two occasions. In 1994, as part of an archaeological evaluation prior to redevelopment, a machine-dug trench, 50 m long, was placed at right-angles to the frontage on Gosbecks Road in the field immediately west of Oliver's Lane (Benfield 1994; Benfield & Brooks 1994) specifically to check for the presence of a dyke. No traces were found. Further north, in 1979, a deep trench for a new sewer along part of Maldon Road was carefully inspected where it crossed the conjectured course of Shrub End Dyke (opposite the north end of Gosbecks Road). Again no evidence of the dyke was found (CAR 6, 924-5).

Thus while it is certain that the dyke did not extend south of Heath Farm Dyke, its continuation south of Kidman's Dyke is more problematic. A cropmark was noted in this position (p 52 above and see *Camulodunum*, pl 1) and a section across it was drawn by Brian Blake in which he shows its ditch to be only 2.0 m wide (p 51, no 35) which is much too small to have been part of a dyke. The ditch of Shrub End Dyke has been seen in section on three occasions between Kidman's Dyke and the Triple Dyke. The best exposure was in 1979 in Heath Road when it was found to be 6.1 m wide and 3.2 m deep (CAR 6, 924-5). Further south it was only partly sectioned but in one place sufficient was cut to reveal that it was still around 6 m wide (CAR 6, 1045-6, fig 13.278).

It would seem therefore that Shrub End Dyke may have stopped at Kidman's Dyke and, like Dugard Dyke to the

west, may have abutted it. If this is the case, the question arises as to what happened to Kidman's Dyke as it passed eastwards. The so-called 'camp' recorded by Jenkins and others (p 20) may be Kidman's Dyke itself, especially if a short distance east of its junction with Shrub End Dyke the earthwork turned sharply southwards again to meet Heath Farm Dyke.

The Triple Dyke was recently replotted using the 1933 aerial photographs, and it appears that it consisted of two straight stretches which meet at a slight angle where the course of the earthwork crosses the modern London road. The point at which the dyke changes alignment presumably corresponds to the position of an entrance through the dyke. If the Shrub End Dyke did not continue south of Kidman's Dyke as considered quite likely above, then the Shrub End Dyke must also have been composed of two straight sections sharing the same entrance as the Triple Dyke. Indeed, seen in this light, the Shrub End Dyke and the Triple Dyke look as they were the same work which, for some unknown reason, has its innermost component extending further south than the other two.

The replotting of the Triple Dyke was done after CFCH died. His figures in Chapter 2 have been amended as necessary, since no doubt this is what he would have wanted. Unfortunately it is impossible to guess to what extent, if any, he would have revised his text so the relevant parts have been left alone.

### **The Laver ditch, the 'palisaded earthwork', Prettygate Dyke, and the Dugard Dyke**

As a group, the Laver ditch, the 'palisaded earthwork', and the Prettygate Dyke are hard to understand. Although it is not possible at present to offer convincing solutions, it is still worth outlining some of the difficulties if only to underline how provisional any overall interpretation of the character and development of the dyke system must be.

The convergence of the Laver ditch and the so-called 'palisaded earthwork' at the west end gives the appearance that they both post-date Gryme's Dyke even though the two features are thought to have been of different dates (pp 59-60, Fig 2.36). Since Gryme's Dyke is post-conquest (p 115), then both features are presumably post-conquest too. They seem puzzling as defensive features, although they would be a little less so if they were somehow related to roads through Gryme's Dyke.

CFCH considered that the Laver ditch and the 'palisaded earthwork' had nothing to do with the entrance through Gryme's Dyke since they converged at a point well south of it. In 1977 road metalling was uncovered in the garden of no 149 Straight Road (p 107, no 13). The metalling is about 30 m north of the 'palisaded earthwork' so that if it was part of road which was parallel to the earthwork it would have met Gryme's

Dyke at its entrance (see Fig 6.1). The 'palisaded earthwork' owes its name to a slot (our Ditch 11, Fig 6.2) found in the 1950s and thought to have held a palisade along the front of a rampart on the north side of the ditch (our Ditch 10, Fig 6.2). By implication, the earthwork was believed to face south-east. However no traces of the slot were noted in 1977 during the Oaklands Avenue excavation of 1975 (limited though that was), so the existence of the palisade and indeed a rampart on the north side is dubious.

It is not possible to plot accurately the locations of the Laver ditch and the 'palisade trench'. It was thought at one time that the triplification of the Shrub End Dyke was to be explained in part by the 'palisaded earthwork' turning northwards on meeting Shrub End Dyke. However, as made clear above (p 171), the triplification definitely starts well north of this point so that we have no idea what happened to the palisaded enclosure when it reached the Shrub End Dyke. On the other hand it looks as if the line of the Laver ditch did continue beyond Shrub End Dyke. There is some uncertainty about its relationship to Prettygate Dyke but it does appear, by plotting its course from aerial photographs (Fig 2.11), that it continued eastwards as the southern ditch of Prettygate Dyke.

The apparent continuity of the Laver ditch with Prettygate Dyke supports the original idea, put forward by Henry Laver many years ago, that this was the route of a Roman road. CFCH agreed, calling it Roman Road 2, but felt the road never continued east of Prettygate Junction as was intended (p 68). He thought that the rampart of Prettygate Dyke had been partly removed and the surviving base used as the agger of the road. However such a treatment would be distinctly odd. There are no convincing examples of any road in the Colchester area having an agger. Also a road on an agger flanked by ditches of the depths postulated here would be dangerous, particularly for wheeled traffic in wet or icy conditions. The land is flat and well drained and there is no reason why an agger should be necessary. However, in support of the road theory is the fact that if the line of the Prettygate Dyke is projected eastwards, it would end up at the conjunction of Roman roads in the Grammar School (as CFCH observed on p 68).

CFCH thought that the Dugard Dyke only extended as far north as the 'palisaded earthwork' (p 26). However Gilbert's map of Colchester and its liberties (dated 1854) shows what he describes as 'Roman entrenchments' continuing as far north as the London road. These can only be the Dugard Dyke. There are at least two possible explanations: either Gilbert was wrong to show the dyke extending so far north or the 'palisaded earthwork', and possibly Laver's ditch, already existed when the Dugard Dyke was built, which would explain why the Dugard Dyke stopped on reaching the 'palisaded earthwork'. The second possibility seems more likely, especially if the 'palisaded earthwork' and Laver's ditch were associated with roads, but it would seem to imply that the Dugard Dyke was later than Gryme's Dyke.

If the Prettygate Dyke was constructed as a defensive work, it seems strange that it was not linked to Kidman's Dyke. Its relationship to Kidman's Dyke will be even harder to understand if, as discussed above, Kidman's Dyke continued east of Shrub End Dyke.

### **The historical background: Camulodunum as a Catuvellaunian colony**

CFCH's view of the sequence of kings at Camulodunum as expressed here (pp 88-94) is a refined version of the one in *Camulodunum* (pp 5-7 & 133-4). Each dynastic change is generally seen as being the result of force, with the next ruler driving out the previous one or taking advantage of a period of weakness caused by the death of his predecessor. The sequences pivot around the dates attributed to the Lexden Tumulus (c 15-10 BC) and the flight of Dubnovellaunus (in or before AD 7), both of which have been revised since the publication of *Camulodunum*. The refined sequence differs in one important respect from the original, namely that Addedomaros, a presumed Trinovantian, is seen as having moved to Camulodunum from the Braughing area around 25 BC.

Warwick Rodwell (1976, 249-65), endorsed by Andrew Fitzpatrick (1992, 26), interprets the coinage in much the same way as CFCH's revised version. He also sees Addedomaros coming to Camulodunum from the Braughing area in c 25 BC. However an important difference is that Warwick Rodwell believes that there were two kings called Dubnovellaunus, one based in Kent and the other, a Trinovantian, based at Camulodunum.

Daphne Nash (1987, 130-33) follows the original *Camulodunum* sequence whereby Addedomaros is thought to have regained control from Tasciovanus before giving way to Dubnovellaunus. The only substantive difference in her sequence is that she believes Rodwell's line that there were two kings involved called Dubnovellaunus and that the Essex one was Trinovantian. Robert Van Arsdell (1989, 349-94) has a more radical view. He believes that the coins of Tasciovanus, Addedomaros, and Dubnovellaunus occurred sequentially as one series, rather than as two roughly contemporary series (Tasciovanus and Addedomaros/Dubnovellaunus). By using metallurgical investigations, he has postulated a unified series which has an earlier beginning than previously accepted (with Addedomaros 40 BC-30 BC), and a break of perhaps 20 years between Tasciovanus and Cunobelin in which the situation is unclear (his 'interregnum'). He also believes that there were two kings called Dubnovellaunus and that the Essex king ruled before Tasciovanus, although he does allow that he may have come afterwards. His interpretation has not found much

support among specialists (eg Fitzpatrick 1992, 26; Burnett 1989).

Cunobelin's ancestry is a critical factor in any consideration of the political history of Camulodunum. The sources are very limited but they do seem to indicate that he was Catuvellaunian. So important is the point that it is worth repeating the evidence.

First there are the coins of Cunobelin on which he proclaims himself to be 'FIL TASCIO' (with variations), namely son of Tasciovanus. Epaticcus did the same, from which we can deduce he was Cunobelin's brother. By the same means, Tincommius, Eppillus, and Verica in succession claimed themselves sons of the Atrebatian king Commius. Because of this, some numismatists have been sceptical about whether these FIL epithets can be trusted since, in the case of Verica, he would have been a king 97 years after his father's famous dealings with Caesar (assuming of course that he was the *Berikos* of Dio (Ix. 19,1); Burnett 1989,236). This is not as unlikely as it seems. If, for example Commius was 25 in 54 BC and 60 when Verica was born, then Verica would have been 62 in AD 43. On the contrary, it is hard to imagine that statements like this about lineage were not true since they were made so publicly; of course sons could be adopted but that would not make such statements any the less accurate.

The other piece of evidence is the passage in Dio Cassius (Ix. 20, 1), 'Plautius... first defeated Caratacus and then Togodumnus, the sons of Cunobelinus, since he himself was dead. When they had fled he [Plautius] won over by agreement a section of the Bodounni whom they had ruled although they [Caratacus and Togodumnus] were Catuvellauni.' From this text and the FIL inscriptions on coins we can deduce the vital fact that Cunobelin and his father must have been Catuvellaunian.

With the possible exception of Van Arsdell's, the various sequences proposed for dynastic change at Camulodunum all assume that they were achieved by forceful means whereby there was a change of ruler at least three times over a period of thirty years or so. We have seen that Camulodunum was in essence a large farm. No doubt its political prestige must have been a major attraction for an external king, but as a working agricultural estate, it would need to be carefully managed and it would need a substantial work-force familiar with its requirements. The population of Camulodunum was probably never very large and presumably was made up largely of people who were dependent on the ruler personally in one way or another, namely his family, his retainers, many slaves, various craftsmen (notably moneyers) and agricultural workers, and mercenaries. It is hard to believe that Camulodunum was such an important and desirable place that each new king would feel the need to impose himself on it, effectively

re-populate it, and then attempt to continue to work it as before. In contrast, we have seen evidence at the funerary site at Stanway pointing to continuity for more than half a century during the period starting in the late 1st century BC (pp 169-70).

We need therefore to seek out alternative explanations for the dynastic sequence at Camulodunum. At least two such explanations can be offered. Robert Van Arsdell's view that the Catuvellauni and the Trinovantes were one tribe from the reign of Addedomaros onwards is one. He surely goes too far in claiming that the tribes were combined, but it could well be that the ruling elite of both groups had become intertwined through inter-marriage and other mechanisms to the extent that they effectively formed one group. One problem with this explanation is that the coinage of Addedomaros, Dubnovellaunus-in-Essex, and Tasciovanus needs to be sequential, which is a view that most numismatists do not share.

Another explanation is that Camulodunum was a Catuvellaunian colony on Trinovantian land, from its foundation to the Claudian invasion. Certainly its exceptional defences are in keeping with the notion that the settlement was in hostile territory. If the Catuvellauni were indeed Belgic immigrants, then Camulodunum could have been an initial toe-hold on the coast through which the immigrants passed to land beyond the Trinovantes which was more easily acquired for settlement. Alternatively, and more likely, Camulodunum may have been the product of the reverse of this process and be a coastal site presumably held and acquired by force to help Catuvellaunian long-distance trade.

The second explanation — that Iron Age Camulodunum was always a Catuvellaunian colony — demands that all its rulers were Catuvellaunian, a possibility that is not as unlikely as it might at first seem. Although, as we have seen above, there is evidence about the ancestry of Cunobelin and Tasciovanus, there is no equivalent information about Addedomaros and Dubnovellaunus-in-Essex. The latter are assumed to have been Trinovantian because they are seen as issuing coins in parallel with Tasciovanus, but there are no good reasons why they should not have been Catuvellaunian. Catuvellauni could issue coins away from their heartland, in parallel with those of their relatives. Epaticcus, Cunobelin's brother, did just that. He issued coins in the south-west corner of the Catuvellaunian 'kingdom' when Cunobelin was doing the same thing at Camulodunum, which was similarly displaced from the traditional centre of Catuvellaunian power. And of course Cunobelin's sons, Caratacus and Amminius, subsequently issued their own coinage (although not in gold) in different areas south of the Thames.

A move by Addedomaros from the Braughing area<sup>2</sup>

1 I am grateful to Ernest Black for this translation.

2 Or perhaps the Baldock-Sandy area, both sites having been investigated since Rodwell 1976 was written (C Haselgrove).

postulated by CFCH and Warwick Rodwell from coin distributions seems more plausible if Addedomaros had been Catuvellaunian. If this view is correct, then we may suspect that Addedomaros replaced an earlier Catuvellaunian occupant since otherwise Camulodunum would need to have risen to great prominence very quickly.

If Addedomaros and Dubnovellaunus-in-Essex had indeed been Catuvellaunian, then those responsible for the Whaddon Chase coinage are likely to have been Catuvellaunian too. If Cassivellaunus had indeed been Catuvellaunian (for which of course there is no clear evidence), then the Trinovantes may have been subject to the Catuvellauni since the mid 1st century BC, despite the intervention of Caesar whose instatement of Mandubracius, a Trinovantian, as king may have been short-lived. Dio Cassius (Ix. 20, 1) tells us, in the context of the relationship between Caratacus, Togodumnus and the Bodounni, that the Britons were not free and independent but were ruled by various kings. The long-term subjugation of the Trinovantes by the Catuvellauni is thus not only plausible, but Caesar in his account of his dealings with Mandubracius reveals the Trinovantes to be a tribe struggling for its own autonomy (*Bell Gall*, v. 20), even although he did not tell us the name of the tribe to whom Cassivellaunus belonged.

## **The development of Camulodunum**

[Figs 7.7-7.11]

The earliest archaeological evidence for occupation at Camulodunum is the early phase of the Lexden cemetery which suggests that there was an important settlement here by c 25 BC, a date which suits the putative move by Addedomaros to the Colchester area. The appearance of CAM and CAMLV on early issues of Tasciovanus shows that the place not only existed by this stage but was sufficiently well defended, presumably by dykes, to merit the description 'dunum'. Generally these coins are dated to c 15 BC although Van Arsdell (1989, 365-6) would have them up to a decade earlier.

Excavation of the farmstead enclosure at Gosbecks would no doubt provide key information about the foundation date for Camulodunum. We can for the moment only guess if it was in existence at the time of Caesar. Could its presence have been behind the troubles which resulted in the death of the father of Mandubracius, the Trinovantian prince? If so, could Cassivellaunus have built it? Or, even, could Camulodunum have been the very stronghold of Cassivellaunus which Caesar attacked and captured? Caesar's description of the place (see above) could apply to Camulodunum, except that one doubts very much if it could be described as being of great natural strength.

The three earliest dykes are likely to be Heath Farm Dyke, Lexden Dyke, and the Sheepen Dyke. Understanding the sequence of these dykes is thus central to understanding the development of the dyke system as a whole. The dykes can be arranged in the four possible sequences as shown in Figure 7.7. The sequences are based on the facts and assumptions summarised in the same figure. Pitchbury hillfort is assumed to be earlier and not an important component of the whole system, if indeed it was ever one at all. The ditch and bank forming the farmstead enclosure could well prove to have been the first of the defences (Pitchbury hillfort apart), but excavation is needed to clarify the matter.

As far as the rest of the dykes are concerned, Gryme's Dyke is post-conquest, possibly even post-Boudican (p 115). CFCH wanted to see part of the dyke as Iron Age (pp 53-4) but there is no dating evidence to support such an attribution. The complicated plan of the dyke certainly suggests a multi-phase origin for it and the dating evidence, although from just one place, points very strongly to a post-conquest date (pp 112-15). Moreover, as discussed above (pp 162-3), the linearity of the various components making up the dyke give the earthwork a Romanised appearance and thus support a post-conquest date for all of it.

CFCH argued that the Triple Dyke was constructed by the Roman army because of the flattened bases of all three ditches which he interpreted as 'shovel-trenches' (p 59). However this does not seem to be a very reliable guide since the ditch of the legionary fortress did not have such a trench (*CAR 3*, Sxs 61 & 75, Sheet 6). He also believed the Triple Dyke to have been Roman because he thought that one of its ditches turned south-west to become the 'palisaded earthwork', which, because of its relationship to Gryme's Dyke, must have been Roman (p 59). It has since become apparent that no such relationship existed between the Triple Dyke and the 'palisaded earthwork' (p 171). Nevertheless an early Roman date for the Triple Dyke can still be supported. In the construction sequence, it must have come between the Lexden Dyke and Gryme's Dyke. Thus a Roman date for the earthwork is possible, particularly if, as seems likely, Gryme's Dyke dates to just after AD 60 rather than AD 43 (p 115). Moreover the linearity of the Triple Dyke argues for a Roman date (PP 162-3).

If, as seems probable, the Shrub End Dyke abutted Kidman's Dyke, then the same argument could apply to this dyke as regards date. In this case, the dyke would probably have been straight without the south-eastwards curve assumed in the past to accommodate its supposed, but now disproved, continuation south of Heath Farm Dyke. The eastern component of the Triple Dyke does align itself with Shrub End Dyke, but this does not necessarily mean that the Triple Dyke has to be later since both could have been built as part of the same design.

As we have seen (p 172), there is some evidence that the Dugard Dyke extended as far north as the present London road and thus it may even have continued



Sequence no	Date Unknown	Between c 50 & 25/10 BC	Between c 25 & 10 BC	Between c 5 BC & AD 5	Subjective assessment
1	Heath Farm Dyke	Sheepen Dyke	Lexden Dyke		Most likely ↓ Least likely
2		Heath Farm & Sheepen Dykes as one	Lexden Dyke		
3	Heath Farm Dyke		Lexden Dyke	Sheepen Dyke	
4		Sheepen Dyke	Heath Farm Dyke	Lexden Dyke	

**Facts:**

- 1) the Lexden Dyke was later than the Heath Farm Dyke (p 32),
- 2) the Sheepen Dyke is no earlier than c 50 BC (pp 161-2),
- 3) some, but not necessarily all, of the cremations in the Lexden area predated the Lexden Dyke (pp 169 & 42).

**Assumptions:**

- 1) the Lexden Dyke was constructed around the time of the most significant apparent activity in that area, ie 25-10 BC (an assumption supported by Fact 3),
- 2) Sheepen Dyke could have been built well before the main period of activity there (which Hull dated from c 5 BC, others from AD 5: see pp 72 & 80).

Fig 7.7 The four possible sequences of development for the three main early dykes. [Pages 174-5]

beyond it to reach the Colne. Thus Dugard Dyke, although comparatively small in scale, seems to mirror Shrub End Dyke/Triple Dyke in its location and alignment and could have been part of the same scheme.

Berechurch Dyke, with Barnhall Dyke forming its north end, is even more problematic in terms of date than most of the other dykes. Its physical appearance, with the straight lines and sharp bends, suggests that it is Roman, or at any rate, very late Iron Age, which is why the threat of invasion by Caligula is usually seen as the likely context for its construction (p 54).

Much more excavation is needed to understand the evolutionary process of Iron Age and early Roman Camulodunum. The junction of Shrub End Dyke with Kidman's Dyke, the south end of the Triple Dyke, and the most easterly extent of Kidman's Dyke (wherever the last two prove to be) are all places where excavation could provide vital new evidence on the development sequence of the dykes. Examination of the ramparts, in the limited places where this has proved possible, has usually been as narrow sections; area excavations of longer sections of rampart are likely to be more informative about structure and date. And of course the key to the origins of Camulodunum almost certainly lies in the farmstead enclosure at Gosbecks which, as yet, has seen practically no excavation at all.

The Roman road system is poorly understood but should eventually prove useful in interpreting the sequence of Roman dykes. The date of the London road in particular ought to be important if it turns out to be very early, because of the way the road appears to have kinked as it passed through Gryme's Dyke and the Triple Dyke. (Each kink suggests a pre-existing earthwork.) Similarly the dates of two roads to the north-west which converge on the entrance in Gryme's Dyke (Fig 2.32) are of interest since they presumably post-date the dyke.

**Summary: a speculative view**

It will have become apparent that there is much to be discovered about the dates of the dykes and the development of the system as a whole. One interpretation of the evidence for the evolution of Camulodunum follows here and is summarised in Figures 7.8-7.11. It assumes that, as CFCH argued, the Triple Dyke and the 'reversing' of Prettygate Dyke (p 61) were the work of the Roman invasion force. It would not be surprising if, in time, much in this sequence proves to be wrong, given the limitations of all the evidence.

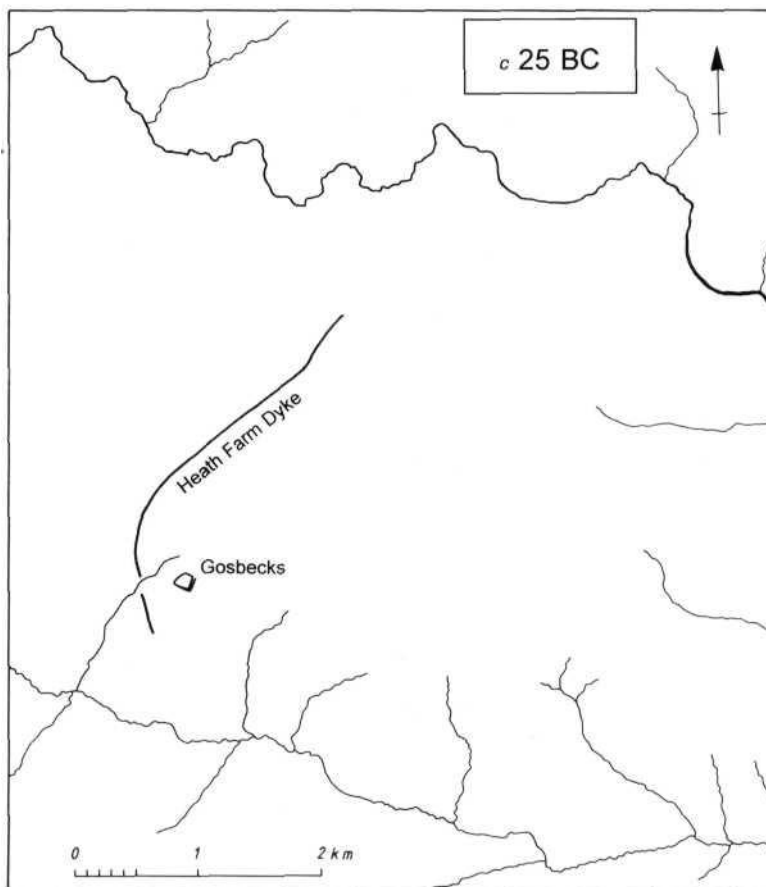


Fig 7.8 Camulodunum in the time of Addedomaros (c 25 BC) [Page 178]

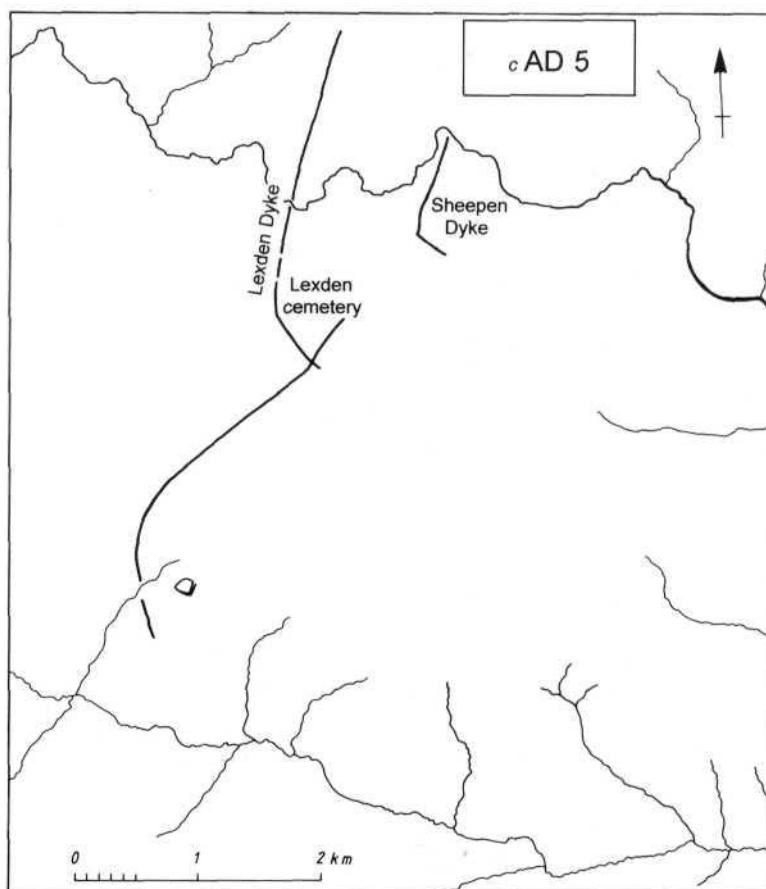


Fig 7.9 Camulodunum in the time of the accession of Cunobelin (c AD 5). [Page 178]

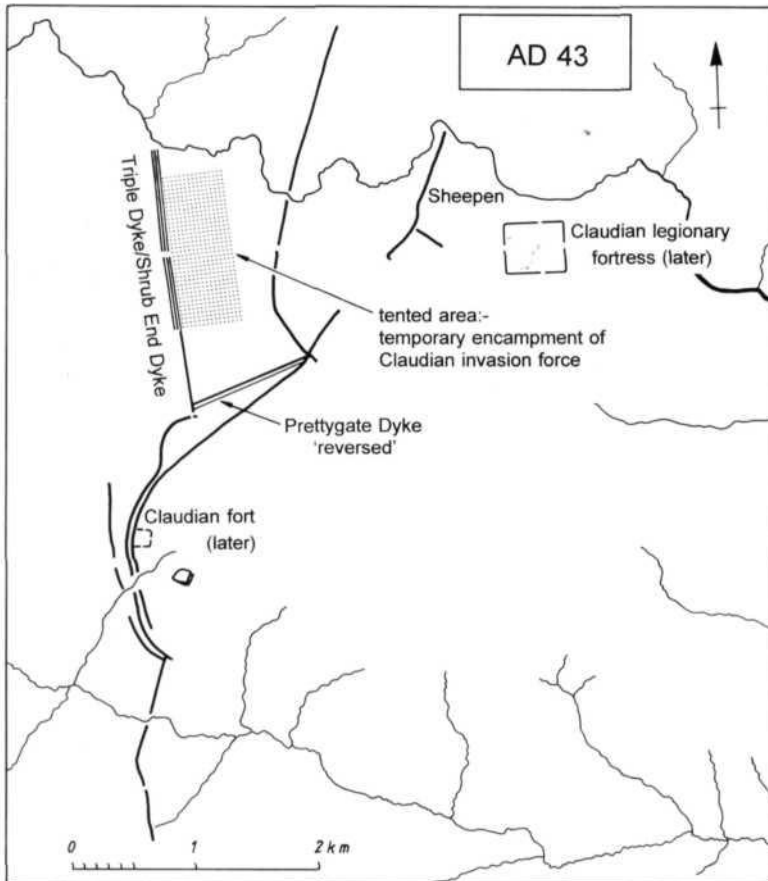


Fig 7.10 Camulodunum in the time of Claudius (AD 43). [Page 178]

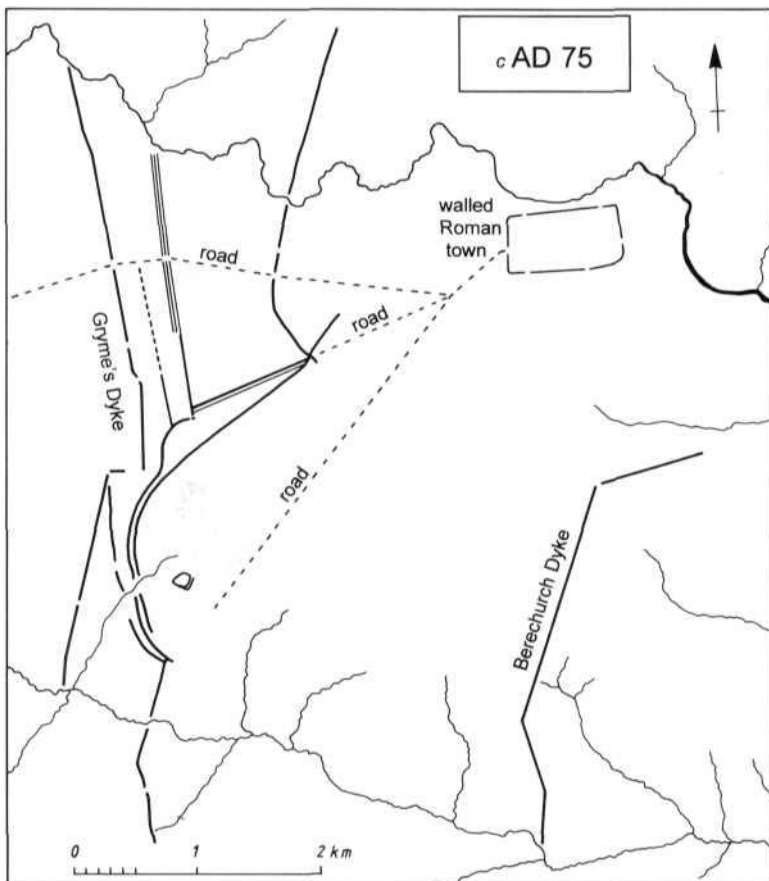


Fig 7.11 Camulodunum (c AD 75) after Boudica. [Page 178]

Camulodunum was founded as a Catuvellaanian colony in Trinovantian territory, if not by 50 BC, then by 25 BC at the latest (Fig 7.8). Although Camulodunum was a large agricultural estate centred on a defended farmstead, its primary purpose was to provide the Catuvellauni with a trading centre on the coast. The establishment of the colony was linked with the subjugation of the Trinovantes by that tribe.

There followed a succession of Catuvellaanian kings. The first whose name is known is Addedomaros. He may have moved from the Braughing or Sandy-Baldock area to Camulodunum around 25 BC. He was succeeded by Tasciovanus, Dubnovellaunus-in-Essex, and Cunobelin (Fig 7.9).

Although today Camulodunum is always identified with Cunobelin, ironically he probably did not build many of its dykes. He simply may not have needed to, because there was never a sufficiently strong threat. Those he may have been responsible for include Kidman's Dyke, Prettygate Dyke in its original form, and the south-west extension to the Sheepen Dyke.

The arrival of Claudius triggered another spate of dyke building, this time by the Roman army. Triple Dyke and Shrub End Dyke, built as one unit, provided a defensive barrier between the river Colne and Kidman's Dyke behind which the invasion army encamped (Fig 7.10). The 'reversing' of the Prettygate dyke was presumably linked with this work. (The size of the presumed temporary encampment shown on Fig 7.10 is little more than a guess. It is based on Hyginus (*Liber de Munitionibus Castrorum*; see Crummy 1993, 111-13) and assumes that the whole invasion force was present including the troops which Claudius brought with him over the channel.)

With the departure of Claudius and most of the army, a

small fort was built at Gosbecks. Within a few years, a legionary fortress was constructed overlooking the Colne valley. The base may have replaced a smaller fort on the same site (CAR 3, 5).

The final phase of dyke building (Fig 7.11) followed the defeat of Boudica and the traumatic destruction of the Roman colony which, according to Tacitus (*Annals*, xxx), was easy because its builders neglected to provide it with defences. The rebuilt colony was given a stone wall (CAR 6, 62-4), and the defences of the former Iron Age settlement were improved with the addition of Gryme's Dyke (in stages), Berechurch Dyke, and Dugard Dyke (if it had not been built by the army in AD 43).

Gosbecks, once the focal point of Catuvellaanian power and authority within Trinovantian territory, became the administrative centre of the Roman *civitas* that followed the Roman invasion. The Roman theatre and the portico provided the setting for the Romanised equivalents of whatever functions of state the site fulfilled in the days of Cunobelin. These pre-Roman functions may have extended to Gosbecks being the meeting place of a major confederation of British tribes. If true, then it may have evolved in the Roman period into the meeting place of the provincial council, as well as being the centre of the local *civitas*. The obvious parallel is Lugdunum where representatives of the Gallic tribes met in an amphitheatre at the outskirts of town, next to a large sanctuary containing a huge altar to Rome and Augustus (Drinkwater 1983, 111-14). Maybe there was something similar within the portico at Gosbecks although the primary intention of the site at Lugdunum was the promotion of the imperial cult which, at Colchester, was apparently focused on the Temple of Claudius in the colony.

## Bibliography

### Abbreviations

BAR	British Archaeological Reports
CAT	Colchester Archaeological Trust
CAR	Colchester Archaeological Report
CAR 1	Philip Crummy, <i>Aspects of Anglo-Saxon and Norman Colchester</i> , 1981
CAR 3	Philip Crummy, <i>Excavations at Lion Walk, Balkerne Lane, and Middleborough, Colchester, Essex</i> , 1984
CAR 4	<i>The coins from excavations in Colchester, 1971-9</i> , Nina Crummy (ed), 1987
CAR 6	Philip Crummy, <i>Excavations at Culver Street, the Gilbert School, and other sites in Colchester, 1971-85, 1992</i>
CMR	Colchester Museum Report
CBA	Council for British Archaeology
ERO	Essex Record Office
RCHM 1922	<i>An inventory of the historical monuments of Essex, 3, North-East Essex</i> , Royal Commission on Historical Monuments, 1922
<i>Rom Col</i>	M R Hull, <i>Roman Colchester</i>
<i>Camulodunum</i>	C F C Hawkes & M R Hull (1947), <i>Camulodunum</i> , Report Research Committee of Soc of Antiq of London, 14

### Ancient authors

Caesar	<i>De Bello Gallico</i>
Cassius Dio	<i>History</i>
Horace	<i>Odes</i>
Strabo	<i>Geography</i>
Suetonius	<i>Lives of the Caesars</i>
Tacitus	<i>Annates</i> <i>Aghcola</i>

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