7. Critique of the research

We may now examine the claims that have been made so far. How might they be criticised and what response might be made to such criticism?

It is unlikely that the computational methods can be regarded as problematic. They are in principle an extension and automation of earlier work by others. It was assumed that a centuriated cadastre can be modelled by computer-generated map coordinates, according to elementary principles suggested 35 years ago. This assumption was justified by results in the case of the extension of the Orange B cadastre to the Cèze valley, and in the case of the Saône plain system it accords perfectly with a proposed centuriated survey, even if the existence of a single administrative system in this area is doubtful on historical grounds[166]. Statistical methods based upon this model have respectable antecedents in the work of others. The estimation of the likelihood of observing oblique relationships uses a well tried, monte-carlo, method. The use of GIS software describes a new application in this growing field.

All these techniques probably have some value in the study of Roman cadastres, and even the simplest, the calculation of coordinates, can be seen to offer significant gains in time and accuracy when compared to other methods.

However, the claims and suggestions which have been made concerning the existence of centuriated cadastres in Britain are in a different category. Almost without exception, those scholars who are qualified to talk about the landscape of Roman Britain seem to regard the non-existence of such systems as a matter of fact. A wholesale revision of this fact, as suggested here, would probably entail a revision of views about major characteristics of the province, such as the nature of relationships between its native and

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166 (M. Clavel-Léveque, pers. comm.). This area was probably divided by a provincial boundary.

'foreign' inhabitants, and its economic status within the Roman world.

Given that so much is at issue, it would probably be convenient from the conventional viewpoint to dismiss the evidence for Roman cadastres in Britain. This could be done in two ways: firstly by showing that such suggestions are incompatible with the known history of development of Roman cadastres in the first century of our era and secondly by showing that the evidence is inadmissible because it has been treated unscientifically.

7.1 The historical possibility

Traces of what could be Roman cadastres are as visible in Britain as in other parts of the north-western Roman empire; and the empirical evidence supports the idea that such systems were, in fact, implemented. However, a plausible story must support this hypothesis. An explanation is needed which can be reconciled with our conception of contemporary events, and with at least some of our present knowledge of the capabilities of the *agrimensores* and their activities in the first century AD.

At first glance, such a reconciliation seems difficult. Simon Cleary's view is that, whatever the *agrimensores* were doing in this period, they were not laying out new centuriated cadastres. Talking about *Camulodunum* (Colchester), where a *colonia* was established by Claudius in AD 49, but around which no convincing trace of a centuriation has yet been found, he says that "Huge areas of formal *limitatio* seem to have passed out of fashion at the beginning of our era, ..." (Cleary 1987: 58, 195).

The key word here is 'seem'. To Cleary it seems that there is no centuriation at *Camulodunum* and he tells us that this also applies to the rest of the empire, not only at the date of the foundation of the *colonia* but since the start of the first century AD. In this latter claim he is almost certainly mistaken, since the southern Tunisian system, the largest centuriation currently known, is very plausibly

dated to AD 29-30 *(see below)*. Even if we take him to mean that centuriation, as a basis for land administration, became gradually (rather than suddenly) unfashionable during the first half of the first century, he is probably still wrong. In Gaul, at least, centuriation seems to have returned in the second half of the century, on a large scale. The all-embracing Béziers A cadastre was almost certainly established during the Flavian period (Clavel-Lévêque 1989: 276-278), in the 80s AD (Clavel-Lévêque 1991: 155) and, as we have seen above, it is likely that a centuriated survey, supposedly of similar date (Chouquer and de Klijn 1989: 286), existed in the Saône plain, even if the corresponding cadastres were not fully implemented.

But, in Cleary's defence, was there perhaps a temporary interruption to the growth of centuriations? Rapid reading of what Raymond Chevallier told us many years ago (1958: 101) about Africa may give that impression:

> Alors que Tibère et Claude ne s'étaient, apparemment, pas souciés beaucoup de colonisation et que Néron n'avait fait qu'accélérer l'évolution du régime de la propriété, fonctionnaires et officiers supérieurs se taillant de grands domaines et l'empereur travaillant, de son côté, à devenir le premier propriétaire africain par achats, confiscations ou héritages plus ou moins provoqués, le règne de Vespasien, après les troubles consécutifs à la mort de Néron, marque une étape importante de la colonisation tunisienne.

So in Africa the latter part of the first century saw important steps in colonisation. This was accompanied by the establishment of centuriated cadastres, such as that at Vespasian's *Colonia Flavia Augusta Emerita Ammaedara* (modern Haidra), which were roughly contemporary with cadastres in Gaul, some of which overlaid earlier systems.

Nevertheless Tiberius and Claudius apparently lacked interest in founding colonies and Nero encouraged the consolidation of existing holdings into large estates. So perhaps (if Cleary is right to some degree) this was part of a general trend; perhaps centuriation temporarily "passed out of fashion"; perhaps for some unknown reason

there was a transient change in procedure, followed by a return to "standard" systems. If so one might envisage one *colonia* founded by Claudius at *Camulodunum* with a cadastre[167] based upon pre-existing, non-standard, forms of land division, and another established at *Ammaedara* 20 to 30 years later, with a cadastre established in a standard, even exemplary[168], fashion.

This argument is defective. As with Cleary's original attempt, it generalises from the available empirical evidence for a particular period, and a particular province, to the empire as a whole. We note that the aim of Chevallier's article is to establish a chronology for Roman centuriations solely in Tunisia. In the remarks quoted above he says nothing about any interests Claudius and Nero might have had elsewhere.

Another line of argument against the presence of centuriations in Britain is presented by Fulford (1990: 26), who seems to be aware of Roman Britain's current anomalous position in this respect. He argues that centuriations are ruled out by the evidence of continuity of British pre-Roman landscapes, which could imply "a Roman political decision not to interfere". Such an explanation has more substance than Cleary's idea, since it has been plausibly argued that, following the defeat of Boudicca, the Romans conducted a policy of "winning the support of the local aristocracy" (Salway 1981: 125). Nevertheless the thinking is based on two unstated and unsubstantiated subsidiary premises: firstly that Roman systems would necessarily make a *tabula rasa* of existing systems, and secondly that the creation of formal cadastres must be detrimental to the interests of existing landowners.

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167 Here the word 'cadastre' is used in the restricted sense of land registry records. A Roman town such as *Camulodunum* almost certainly had such records, but it is conceivable that they could refer to areas of land holding which already existed prior to the establishment of the *colonia*.

168 Hyginus (Blume, Lachmann and Rudorff 1848: 180) "*haec est constituendorum limitum pulcherrima*"

These ideas are not supported by the evidence. With regard to the first point, we have seen that new Roman systems could incorporate parts of earlier (Roman and pre-Roman) landscape and that even contemporary developments could look distinctly non-Roman *(figure 2.1)*. Thus the presence of fields of an indigenous type does not imply the absence of a centuriated system. With regard to the second point, the evidence from an area of multiple superimposed cadastres in southern France (Clavel-Lévêque and Laubenheimer 1984) indicates that there was a link between the frequency of modification of cadastral structures and economic activity. Those areas which have the most superimposed cadastres are also those with the most factories producing amphorae for the shipping of wine, which was then (as now) a major agricultural product. Given this apparent link between high output and re-surveying, it looks unlikely that the re-surveying of these areas was, or was intended to be, to the detriment of existing landholders. There may be several different motives for a cadastral survey, but even in a colonial case like Orange B (Piganiol 1962), settlement of veterans was not the only aim. The indigenous people were also catered for.

Cleary and Fulford are not convincing. Cleary's supposed general trend away from formal *limitatio* at the start of our era probably never existed and Fulford's argument is weakened, since it seems to be based on the inaccurate view that such systems are totally destructive of earlier landscapes and necessarily harmful to existing land holders' interests. A more accurate picture of the contemporary activities of the *agrimensores* may perhaps be gained by looking at what they were doing on the southern fringes of the empire, not long before the conquest of Britain.

The centuriation of South Tunisia, "une bonne illustration de la technologie romaine de l'arpentage à son apogée" (Trousset 1978: 153), can be securely dated within the first part of the first century AD. This is because twenty *termini* have been discovered, some apparently in situ. Fifteen have remains of inscriptions, of which four can be interpreted to state that the *limitatio* was implemented by *Legio III Augusta* in the third proconsulate of *C Vibius Marsus,* i.e. 29-30 AD (Trousset 1978: 131). These stones, being at the south

eastern extremity of the cadastral grid, were probably some of the last to be positioned so we may suppose that the surveying had been going on for some time. Nevertheless, as Trousset points out, it could not have commenced before 24 AD. Thus the cadastre antedates the initial conquest of Britain by around fifteen years.

This system has two remarkable characteristics. It is very large, and it is primarily conceptual.

The enormous size of the cadastre is suggested by coordinates inscribed on twelve of the *termini*. They have the following extreme values, in centuries:

DD. 65	UK 305	(305 to the south east of the *kardo maximus*)
SD 45	UK 265	(45 to the north east of the *decumanus maximus*)
DD 110	UK 295 ?	(110 to the south west of the *decumanus maximus*)

So, even if the system did not extend to the north west of the *kardo maximus*, it would have covered approximately 155 by 305 centuries or 110 by 215 km. This is an area of 47,275 centuries, some 2.5 million hectares[169]. This may be compared with the proposed colonial cadastre of *Lindum*, about 180 x 180 or 32,400 centuries, i.e. 1.6 million hectares, which covers all of Lincolnshire and parts of five neighbouring counties. Even allowing for the possible existence of this large British cadastre, the southern Tunisian system remains the largest known example of such a survey.

The conceptual nature of the south Tunisian centuriation is suggested by the almost total absence of recognisable agricultural structures (fields) which correspond to it. The *termini* themselves

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169 This is not the same as the figure (26,900 km^2) given by Trousset (1977: 144). He seems to err in using a figure of 65, rather than 45, centuries for the distance of the terminus of Graïba to the left of the *decumanus maximus*. He gives 45 in his table, which is the same as the figure given by Legendre (1957: 149).

are remarkably well preserved[170], but if they had not been found, the cadastre would be unknown, since there were no traces on the ground, or visible on aerial photographs, to attract the attention of scholars (Trousset 1977: 126).

Trousset (1977: 134-135) would like to retain the idea that there were local allotments of land within the cadastral framework, but it is also the case that there are many traces of antique cultivation, within the area of the cadastre, which do not conform. To him these terraces, oriented according to contour lines, "semblent relever beaucoup plus d'une tradition technique commune aux montagnes présahariennes que de l'histoire des centuriations romaines".[171]

As Trousset says (1977: 158), we have here, not a centuriation developed in detail, but an outline framework based on *termini quintarii* positioned at important points doubtless represented on a *forma* at Rome or Carthage. This *forma* was used to grasp the extent of the territory and allow for strategic decisions, made centrally, to be implemented at local level. This is clearly evidenced by two inscribed stones marking the common boundary of two tribes, the Tacapitani and the Nybgeni. These say that:

ex auctoritate imperatoris Neruae Traini Caesaris Augusti ... secundum formam missam sibi eo posuit

They were established by the order of the emperor Trajan according to the cadastral plan sent to the surveyor (Trousset 1977: 135).

The essence of this cadastre thus resides, and probably always resided, not in physical symbols of its presence on the ground, i.e. in agrarian structures, but in the conceptual form of a cadastral base map and its associated files. The presence of copious physical

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170 In this cadastre there are 12 stones with coordinates inscribed; in southern Gaul, which has more than 15 known cadastres, there are none.

171 Compare this with Bradford's (1957: 200) description of internal divisions of centuries in northern Tunisia as " 'contoured' and *not* at right angles to the *limites*".

evidence of contemporary and apparently unrelated land divisions does not rule out the existence, as a social and historical reality, of a vast centuriation.

This then is what the *agrimensores* could do on the eve of the conquest of Britain; and it is clear from the cases of Béziers 'A' and the Saône plain system that fifty years later these skills had not been lost.

But who were they, and how might they have been organised? Few names of individuals are known, but we know that teams of surveyors were supported and trained by the Roman army[172], and that these teams were sometimes augmented by highly qualified civilians (Sherk 1974: 541). These latter senior surveyors include the unnamed ex-army surveyor who was in charge of a survey in Pannonia in the reign of Trajan (Sherk 1988: 161); and we note that Frontinus, who had an illustrious career in the imperial civil service, and was for a time governor of Britain (74-76 AD), was one of the *agrimensores* and so may at one time have been an active member of the agrimensorial élite.

We have seen that in the case of the great south Tunisian cadastre, *mensores* attached to the third legion were capable of impressive technical feats; and it is probable, to judge from the Pannonian example already cited, that they were managed by one or more experts who could, by means of the *forma*, conceive of the cadastre as a whole.

It is suggested by Sherk (1974: 556) that such a senior surveyor would probably have his plot of land in the new colony "and continue to live and work there". This is possible, but there is also the probability that someone managing the implementation of a vast system such as that of southern Tunisia was a senior administrator. If so, we can imagine him to have been, like Frontinus and

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172 There were eleven per legion (Sherk 1974: 549).

many others, highly mobile[173]. This is why it could be suggested above *(6.3)* that the apparently identical *modus operandi* evident in both Béziers 'A' and the hypothetical colonial cadastre of Lincoln is the same, they may both have been designed within a few years of each other by the same man.

Given new territories to be surveyed, and the general existence of trained bodies of military surveyors, coupled with the necessary senior management, it is hard to believe that Tiberius, Claudius and Nero would allow them to stand idle. There was a pause in the cadastration of the province of Africa for about thirty years, but there is no reason to see this as a general phenomenon. Rather, it can be suggested that developments in Africa came to a temporary halt because the necessary directors of operations were not available. The senior surveyors were needed elsewhere.

Britain stands out as the most likely next focus of their attentions, since it was potentially the greatest prize among the territories incorporated into the empire at this time[174]. It was to that latter province that the energies of the administration are most likely to have been directed. And we should anticipate that, subject to the local political situation, British cadastres would be likely to follow precedents set in Africa, particularly with regard to scale and physical impact (or lack of impact) on the landscape. If the next phase of cadastration took place in Britain then we should expect to find systems on a grand scale. We should also expect such systems to have had a large conceptual component, which could make them difficult to perceive among contemporary, but physically discordant, agrarian structures.

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[173] In fact Balbus, who was (temporarily) surveying on the Dacian limes shortly after 100 AD, refers to "an annual rotation" which allowed him to return to his studies (Sherk 1988: 156). One could also ask, only half jokingly, why a senior administrator and technical expert would want to settle down on the southern fringe of the empire.

[174] Apart from Britain, the following four provinces were permanently added to the Roman empire: Mauretania (AD 42), Lycia (AD 43), Judaea (AD 44) and Thrace (AD 46) (Wells and Barrow 1950: 91).

7.2 A scientific approach?

Even if we accept the historical possibilities presented here, we must consider the scientific validity of our approach. In the areas considered, and particularly in South Norfolk, there are rival theories which appear to be based upon well-founded principles and scientifically assembled facts. The question is whether or not our alternative interpretations are equally valid.

7.2.1 Theory and observation: the South Norfolk 'A' cadastre in the Scole-Dickleburgh area

We have seen that part of the landscape of south Norfolk may reveal a Roman cadastre consistent in many ways with similar systems elsewhere in the empire, but we must acknowledge that this view is at odds with another interpretation (Williamson 1986; Williamson 1987) which has gained wide acceptance (Edwards and Wade-Martins 1987; Fleming 1987). This has been expressed in its most extreme form by Muir and Muir (1989).

Tom Williamson's interpretation is simple. Clearly a cadastre, or part of a cadastre, existed in the Scole-Dickleburgh area of South Norfolk. Clearly a Roman road is oblique to its general orientation. Thus, he argues, we have the same pattern which we see when some modern linear feature, such as a new road, cuts across the pre-existing network of field boundaries. So, if we accept the unstated assumption that this is the only way in which such a pattern can arise, the road is later than the cadastre.

Some people accept this argument as logical and subject to "proof" by verification. So Muir and Muir (1989: 29) say that "The logic of the argument is quite clear, but readers can check its validity by looking at the relationships between field-patterns and younger railways or new main roads or motorways." And they are not alone since Oliver Rackham (1986: 160) argues in the same way in a similar context by telling us that "Mr Paul Drury has neatly proved that parts of the system [of south-east Essex] are Roman or earlier

by showing that they are cut across by Roman roads (e.g. around Braintree) as well as by railways and motorways."

It is hence quite easy for them to say that "Dr Williamson had in fact rediscovered a prehistoric 'coaxial' field system ..." (Muir and Muir 1989: 31), and they put this together with several supposedly prehistoric systems, including Stephen Bassett's 'Goltho' system *(6.3)*, to create a vision of prehistoric landscape populated by cadastres "many square miles in area", even in lowland Britain. According to them "the emerging evidence is consistent and open to no other interpretation". This conviction is shared by Williamson hiself, at least as far as the Scole-Dickleburgh system is concerned. He regards it as an "obvious observation" that it "must predate the establishment of the Roman Pye Road" (Williamson 1987: 427). It is Williamson himself who will not admit any other explanation.

Hardly any enquiry can be open-minded, for reasons which will be given below, but this extreme example of dogmatism seems to have had a most remarkable consequence. Williamson and others, having become convinced that this is essentially a pre- or proto-historic cadastre, show us the data in a way which, by its rather significant but unconscious departure from the empirical evidence, supports this theory and only this theory.

Maps are by far the most important way in which a view of the Scole-Dickleburgh system is conveyed. It is clear that in Tom Williamson's hands, and more so in the hands of at least one secondary author, these suffer some deformation.

Three examples may demonstrate this. Each compares a particular feature as depicted on the second series six inch scale Norfolk county maps[175], on the left, with the equivalent feature of the

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175 The second series maps are used rather than the first series because they have less detail, such as trees, to obscure the boundaries, which are the features of interest. The second series date from the first decade of this century and the first series are about 15 years earlier. As far as the boundaries are concerned there does not appear to be any significant difference between

Scole-Dickleburgh "prehistoric coaxial field system" as presented to us in Peter Wade-Martin's introduction to *Norfolk from the Air* (Edwards and Wade-Martins 1987: 12). Each map fragment was scanned in the same way using the Applescan package, and resized to obtain pairs at as near the same scale as possible. This allows direct comparison between cartography of the early twentieth century and Wade-Martin's representation of Williamson's view of the system.

The first illustration *(figure 7.1)* compares the two representations of Vaunces (or Vances) Lane and its northern extension. This formed part of the Dickleburgh-Shimpling parish boundary until the parishes were amalgamated. As we have seen, it may be a *quintarius* of the proposed Roman cadastre.

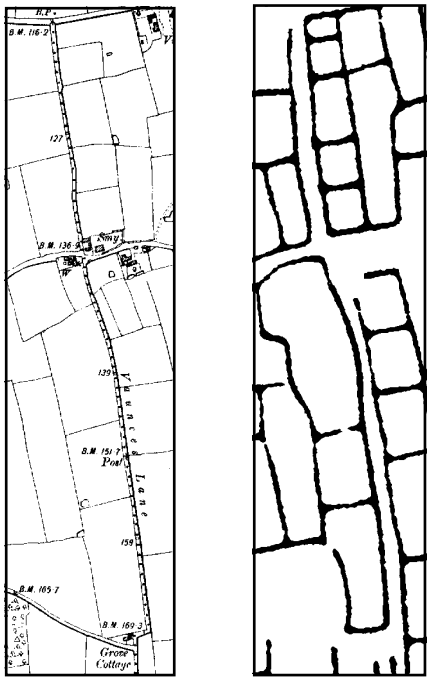

Figure 7.1 Vaunces Lane.

The Williamson-derived version was originally drawn to a scale which was smaller than six inches to the mile. This has created some distortion. The lane has necessarily been drawn wider than its scale width and this has not been done in a consistent fashion. The result is that the feature, which appeared only slightly distorted from a straight line in the original, now has a marked sinuosity.

The second example *(figure 7.2)* shows the two representations of the lane that is now called Burston Road, Dickleburgh. Again, the overscale drawing of this road has lead to a distortion of the profile.

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them in this area. There is, of course, an earlier series of maps, those of the tithe apportionments of the first half of the nineteenth century, but these were not available for scanning. However, they have been consulted and they show that no change took place in the three features presented here during the latter part of the century.

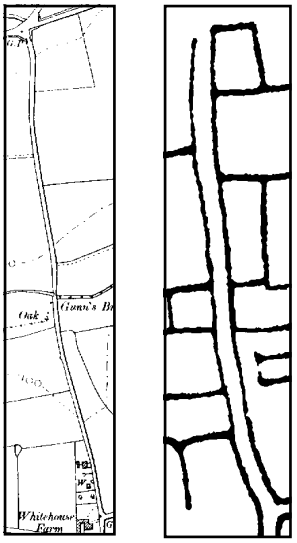

Figure 7.2

The end result is that the road, which, in the original, could have been interpreted as a straight line with small random deviations, now looks banana shaped.

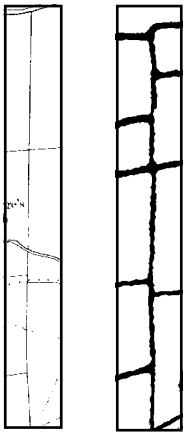

Fig. 7.3

The third example *(figure 7.3)* refers to a field boundary rather than a road. The boundary lies to the east of Shimpling church and part of it forms the western boundary of the field in which the fossil ditch was excavated *(5.2.3)*. As already remarked, this hedge appears now to be very straight, and this is true for all cartographic representations of it, including the tithe map. Yet the alternative representation makes it appear markedly sinuous.

Williamson's own view of this particular boundary varies. One drawing (Williamson 1986: fig 2) depicts the line as if it were sinuous, possibly even more so than in the drawing shown here. Another series of drawings (Williamson 1987: figs 2 and 3) generally show it as it is - i.e. straight. These latter drawings are more accurate and hence give a more rectilinear appearance to the landscape. This rectilinearity is exploited in the drawing of major elements and topography (Williamson 1987: fig 4)[176], which is used to emphasise the planned nature of the system. The boundary under discussion is depicted as a "major relict landscape feature". It is straight, and so are many of the other features shown.

However, this latter view is not the one which has persisted. At present the "sinuous" vision is paramount. An explanation for this could be that Williamson and others have committed themselves to the idea that the system predates the Roman road; for them it is essentially prehistoric, and hence it is expected to appear so. Slight

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176 In commending these drawings, which are Williamson's most accurate representation of the landscape features, it must be observed that the heights indicated in this last figure are incorrect. For metres read feet.

errors in draftmanship which support this vision by emphasising sinuousness go unnoticed and uncorrected. Alain Gallay (1986: 16) has said "On ne voit que ce que l'on est préparé à voir, ... ". Conversely one does not see those details which, according to the theory by which the observations are made, have no importance and which, in fact, one is not prepared to see.[177]

Apart from these minor, but significant, distortions of the cartography, the draftsman of the illustration from *Norfolk from the Air* has made another important error. This is in the scale.

Using the scale of the illustration, we can establish that two identifiable points in it are 2.88km apart. The indicated equivalent distance on six inch sheet 106 SW, using its scale, is 2.63km. The former scale is thus in error by nearly 10% over this particular interval, an approximation which can be extrapolated to the whole figure.

The scale is also a feature which we would look at quite differently according to our prior belief about the nature of the cadastre. If we thought that the layout could be Roman then such an error could not have occurred, since the accurate measurement of the distance between possible *limites* is important evidence for the reality, and even the likely date, of the system. Accurate measurement could only be made on an accurately scaled map.

On the other hand, an error of 10% can be unnoticed in the theoretical context of a prehistoric cadastre, where the accurate measurement of intervals between boundaries has not so far been very important.

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[177] Since in English "not prepared to" can mean "not willing to", it should be made clear that the intended meaning here is "not set up to". We are suggesting that it is the vision that has not been prepared. A way of seeing is not adopted as an act of will, but results from the adoption of a particular theoretical orientation.

So, although no-one would suggest that these modifications of the data are deliberate, it is evident that they are linked to beliefs in the minds of their authors. They are theory-dependent.

This is a general phenomenon. Stephen Jay Gould gives us another example in his account of a revolution in Paleontology, *Wonderful Life*. He describes the growth of a new vision of the development of life. It is a fascinating story in itself, but for Gould there is a deeper message. He says:

> "I have laboured through the details ... because I know no finer illustration of the most important message taught by the history of science: the subtle and inevitable hold that theory exerts upon data and observation. Reality does not speak to us objectively, and no scientist can be free from the constraints of psyche and society. The greatest impediment to scientific innovation is usually a conceptual lock, not a factual lack." (Gould 1990: 276)

Similarly in our case the "prehistoric" theory, once it has become fact, influences the observation of the cartographic traces and scale, and this changes both the data and the way in which it is presented to others.

This demonstrates the extreme difficulty, or even the impossibility, of conducting a totally open-minded enquiry. The data must always be selected, collected and presented in the context of a theory. In the process they are modified. The facts and theories cannot be separated from the people that work with them. As David Clarke (1978: 14) has observed "Archaeological facts or data change in the changing light of what the archaeologist deems 'significant attributes' ".

7.2.2 Positivism, realism and the science of cadastres

How then can an historical science proceed when faced with such potential lack of objectivity? To answer this question it may be helpful to look at some of Archaeology's closest scientific cousins, such as Paleontology and Geology. For the former Gould demonstrates that there was a perceptual problem, but this did not prevent the science from developing. Similarly in Geology the long debate on continental drift, now the authodoxy and re-christened "plate tectonics" (Hallam 1973; Le Grand 1988), terminated in the revision of a number of outmoded facts. This has not diminished the economic importance of the discipline or its status as a science.

Such shifts of viewpoint are probably inevitable in most disciplines (Kuhn 1977). They do little damage, except to the idea that "out there" there is some ultimate truth waiting to be found. Nevertheless this lack of secure foundation clearly worries some people, who react in several ways.

One response is to appeal to convention and the status quo. Thus we have John Maddox, editor of *Nature* (BBC Radio 4, 30 October 1981):

> "The conventional scientific view, which I think is entirely proper, is that there is no particular point in inventing theories which in themselves require a tremendous feat of imagination and constitute an assault on what we know about the physical world as it stands, when there is at least a good chance, in my opinion, that conventional theories will in due course provide an explanation." (Sheldrake 1985: 229) .

David Clarke pursued a similar line:

> "One answer to this criticism [by those who would deny the existence of any fundamental entities in archaeological data] is that the function of the archaeologist is not to doubt well-tested propositions accepted by common sense, but to provide analysis or elucidations of them - the function of the modern philosopher since G. E. Moore expounded 'that it is futile to doubt, or to pretend to doubt, common-sense propositions.' " (Clarke 1978: 22).

Another reaction, that of the "New Archaeology", was to propose to reform the discipline by introducing what Clarke (and many others) called "the scientific method"[178].

Binford wants to see Archaeology become "a more objective, scientific" discipline. According to him, we cannot justify "returning to prescientific forms of debate." We should be guided by the logical positivists and make theory testing essential. This is one of the reasons why, as we have seen *(2.2)*, Binford rejects Kuhn's theory of paradigm change.

Alain Gallay rejects Kuhn in the same way:

> "Change in attitudes can only be progressive. Revolutions in the development of objective knowledge have been rare. If the history of science and techniques sometimes gives this illusion, it is because it concentrates on particular men and ideas, and ignores the context of emergence of discoveries and the multiple interactions which link, at all periods, the members of a scientific community." (Gallay 1986: 279) (author's translation).

Clearly, Gallay regards the development of *objective* knowledge as not only a possibility, but the function of science. According to him, this objective knowledge develops gradually.

A third reaction goes beyond the idea of providing a "scientific" basis for "objective" knowledge. It recognises the social nature of the activity and wants to form a discipline, that is to establish an institution. Thus we have Clarke:

> "It is time for archaeology to move from the status of an intuitively acquired craft towards that of an explicit discipline .... In an attempt to

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178 Note the singular article. By definition, for these scholars, there is only one scientific method. Thus, according to Clarke (1978: 24): "The cycle of the scientific method - systematic observations, preliminary hypothesis, testing experiments and tentative theory, then the application of the model to reality - these techniques were implicit in the works of Galileo (1564-1642)". However there are other, perhaps more accurate, views of Galileo's methods (Biagioli 1990; Feyerabend 1978).

> remedy this situation [the struggles of an embryo discipline] we have spent some time developing general models .... Part of this procedure has involved a conception of sociocultural systems as elaborate behavioural information systems." (Clarke 1978: 149).

And, while not doubting the valuable contribution of the natural sciences, Gallay wants to see Archaeology as an independent scientific discipline:

> "It seems however more urgent to seek to establish, for archaeology, *its own means of acquiring knowledge* which would simultaneously take into account the objective of the discipline - which sees itself comprising both ethnology and the history of man - and the constraints associated with the available material. This position is in some ways a reaction against *interdisciplinary methods of study* which have flourished during the last few years in the human sciences." (Gallay 1986: 278) (author's translation).

However, as far as interdisciplinarity is concerned, Clarke was ambivalent. For him a variety of aims and interpretations can be a strength

> "Indeed we should encourage the analysis of archaeological problems from as many differently based approaches as possible and integrate the overall consensus."

Or it can be a weakness,

> "isolating archaeology in an undisciplined fever from the calm status of a coherent discipline." (Clarke 1978: 19).

These attitudes appear to be reactions to insecurity. They seem to be responses to the uncomfortable proposition that in Archaeology, despite an apparently sound empirical basis, very little is certain. Such reactions are understandable, but unhelpful.

Positivism[179], which seems to offer safety in following the 'rules' of scientific procedure, is still under repeated attack. As Courbin says

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179 Tainter and Lucas (1983: 712-4) provide a particularly clear critique of the empiricist-positivist position.

in the preface to the English translation of his book *Qu'est-ce que l'archéology* (Courbin 1982):

> "As far as methodology is concerned, hypothetico-deductive reasoning (which was already in a bad way, though less so than laws) has suffered a new attack: Hodder (in *Antiquity*, 1984) noticed that the famous "validation of hypotheses" couldn't base itself on new "facts" since the latter are themselves theories; archaeology couldn't be a science!" (Courbin 1988).[180]

Perhaps we would do better to adopt a realist approach, as advocated by Victoria Lawson and Lynn Staehali (1990). They face up to the criticism that "realist conclusions cannot be disproven because the researchers do not specify counterfactuals by which findings can be checked. Accordingly it is impossible to determine whether contrary findings represent mere contingencies or fundamental flaws in the theory." This is precisely the problem that the investigator of a possible cadastre must face. Real cadastral systems cannot be assumed to be based upon simple deterministic rules. If they were, then we could use the (pseudo) scientific approach. We would postulate the existence of a system over a certain area, and then follow Popper (1972) in attempting to disprove the hypothesis by studying reports of archaeological investigations at points where, according to the oversimplified model, physical traces should appear. Such a search for counterfactuals is, in reality, unproductive.

Lawson and Staehali argue for their sort of realism, despite their remaining feelings that the positivist criticism (that theories cannot be categorically disproved by counterfactuals) "... is troubling even to realists, ..." (1990: 18). In their view realists want to understand how elements of a system are causally related, but they

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[180] In Courbin's words, "If I say that the proper procedure of archaeology is the establishment of facts, this is precisely because they *have to be established*. [emphasis in the original] Like everyone, probably, I think that they are never "given", that they are, as the word indicates, made, fabricated, at the most elementary stage of observation on which all the rest depends." (Courbin 1988: xii).

conceptualise causality "in terms of mechanisms (which are tendencies, not laws) whose operation depends on the context in which they are found".[181]

Their realism has three essential features, which are that
1. Many realists combine several bodies of theory
2. Realists try to clarify the distinction between observable phenomena and processes that create them
3. They believe that "the plausibility of knowledge must be evaluated in a given social context, not in the light of some 'objective truth'".

While this may not be the royal road to all knowledge, it fits the study of Roman cadastres perfectly. Reality resides at a conceptual level, as expressed in the *forma*. This reality is expressed in a rich assortment of observable phenomena, whose appearance depends upon the social and geographical context in which they appear. This richness of outcome justifies an eclecticism which uses several theoretical approaches. Lawson and Staehali "view the pluralism inherent in realist research as a strength". One can only agree that there are indeed "many ways to know the past."[182]

Such a set of realist epistemological and methodological beliefs are appropriate to this sort of study, and they open up a perspective which is quite different to the unreality which can be conveyed by a positivist view. We can see the investigation in a broader context. Perhaps we may compare it to a judicial enquiry. A crime has been committed; data are collected, which may be subjective accounts of witnesses or the more "objective" information provided by forensic science; this may narrow the search for suspects; a case may be

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181 This view probably raises sympathetic echoes in the minds of most students of cadastres. Tendencies can be seen in the landscape, but certainty can rarely be achieved in a particular case. As Chouquer and DeKlijn (1989: 284) say, "... on ne peut se prononcer valablement sur chacune des lignes que la carte enregistre par milliers et dire si elle est ancienne ou non."

182 For this last phrase the author is indebted to Monique Clavel-Lévêque.

constructed against a person or persons; the case is tried; the verdict is delivered.

All of these things may happen in our own investigations, and they are all potentially subject to human failing. In a trial even the scientific expert witnesses can err, although perhaps, as Alistair Brownlie says (1984: 39), we should not ask the question "is the scientist right or wrong", because it presumes that science is capable of a fixed and definite answer which will always remain true.

Alan Chalmers is probably right in saying that

> "... what renders a scientific claim acceptable or utilizable is the extent to which it offers objective opportunities for future research or practical application, that is, the extent to which avenues for future investigation or exploitation present themselves, given the existing theoretical and technological resources.
>
> What matters is not the "truth" of the claim or its value in the joint opinion of a group of scientists, but what is done with it." (Chalmers 1990: 77).

On this basis, this present research is "science" to those who find its techniques useful and see its results as a stimulus to further research or a confirmation of their own findings. Conversely, for those who adhere to the "prehistoric" view of the South Norfolk 'A' cadastre, and for whom the theories presented here may have a potentially destructive effect[183], it cannot be so called.

So we need to recognise and value inevitable uncertainty. As an eminent scientist said, "It seems to be a common defect of human

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[183] This is particularly true when institutional commitment has been made to the "prehistoric" theory. A case in point is the award by the Prehistoric Society to Tom Williamson of its Baguley Prize in May 1988 for his article (Williamson 1987) in the previous year's proceedings. It would be asking a lot of human nature to expect those who gave the prize to welcome an attempt to show that the system described in the article is not prehistoric.

minds that they tend to crave for complete certainty of belief or disbelief in anything" (Lyttleton 1977: 14)) He also urged us to resist this craving.

If we accept this point, we are not necessarily left in a state of complete anarchy. Theories can still be proposed, and their likely consequences can still be tested by any conceivable and available means. These can be as objective as possible[184], but can also attempt to avoid the pitfalls of positivism and dogmatism. Self-examination is needed in order to recognise how those taken-for-granted theories on the border of our sub-conscious may influence our view of the world.

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184 Note that there is a distinction between objective knowledge (truths which are independent of what we think) and more or less objective methods. The author finds it possible to doubt the existence of the former while trying to achieve the latter. Such objective methods include the use of data collected in different theoretical contexts by independent observers, and quantification.