

in other areas through fieldwork, even with the support of Tithe and Enclosure maps.

At a time when the Sites and Monuments Records (SMR) for each county are being computerised and updated, often as part of MSC funded programmes, there is surely a pressing need for a reasoned assessment of the manner by which such data have been collected. To this end Nigel Holman's contribution is a welcome one. As he states in his paper, a substantial portion of the SMR for northwest Norfolk derives from data collected by individual, non-professional fieldwalkers. Holman, using straightforward procedures, attempts to establish the extent to which site distributions based on the SMR of his region accurately depict past settlement patterns, and how much of its structure is merely a reflection of the pattern of fieldwork. The results indicate a blend of both fieldwork bias and real distributions, but the value of the exercise lies also in the use to which such knowledge is put, particularly towards future amateur and professional cooperation. Many amateurs spend significant portions of their leisure hours tramping fields, following the metaphorical footsteps of several worthy predecessors. Through such activity they help to keep alive the spirit of archaeology and a public awareness of the past. The discipline has a duty to acknowledge their commitment and enthusiasm, to foster their contributions, and not merely dismiss them lightly. Moreover, in a time of financial stringency, professional archaeologists may in the long run benefit by broadening public involvement in, and support for, archaeology. While pandering to market forces and the interests of the leisure industries may reap short term financial gains, listening to peoples' views and according recognition to the way they see the world and their past, is likely to be a more fruitful course to follow.

It is this search for a balance between financial constraints and ethical concerns that preoccupy our contributors. Many of the authors emphasise the personal aspects of fieldwork, the sense of place that comes from a growing familiarity and understanding of the landscape and its human presence. Their enthusiasm for the subject and the tasks in hand is clearly communicated, and, I would surmise, enhances purpose and meaning in what might otherwise be a sterile and certainly tedious exercise. While each adopts a different stance, all acknowledge the truth in the adage "what you see is what you get". It behoves us then to eschew dogma, and to allow for changing the perspective with which we seek to perceive the traces of past human endeavour.

Common abbreviations used in this issue:

DoE	Department of the Environment.
HBMC(E)	The Historic Buildings and Monuments Commission for England.
MAFF	Ministry of Agriculture, Fisheries and Food.
MSC	Manpower Services Commission; a Government funded youth employment scheme.
RCHM	Royal Commission on Historical Monuments (England).
SMR	Sites and Monuments Record; County based archive.

DYKE SURVEY: AN IMPERFECT APPROACH TO THE INVISIBLE

Francis Pryor

Introduction

This paper is in essence a post hoc rationalisation or justification for the research method and design that grew out of fieldwork, largely, it seemed at the time, by itself. It would be nice to pretend, as I have done from time to time in research seminars and the like, that we approached our problems from the top-down: unfortunately things did not work out that way, and it would be dishonest to pretend otherwise. I used to worry about the fact that my research designs, even my research objectives, invariably changed beyond recognition once put into practice, as this clearly went against the principal canon of modern Anglo-American archaeological law; to make matters worse I had convinced myself that the fault was mine, that my work lacked discipline, and conceptual rigour. However, as time has elapsed, I have slowly come to the realisation that this is not so: our work has a discipline of its own, even if we do not broadcast the fact explicitly.

We are funded by a Cultural Resource Management (CRM) agency -- English Heritage -- and we are, I feel, expected to produce tangible results that will mean something to the public at large. Accordingly we cannot ride our own hobby horses -- at least not to the point of saddle sores -- and tend to stay clear of Negative Evidence (the last resort of an archaeological scoundrel), whenever we can. This system of funding, in essence 'payment by results', imposes a very real discipline on our work and forces us to make numerous, sometimes quite drastic, tactical decisions, whatever their effect might be on the original research design. I hope this paper will show that a flexible approach to the subject is both structured and disciplined. We can see little point in pursuing previously stated research objectives once these have been shown to be unattainable or irrelevant (or both). It is our belief that a properly thought-out, self-assessing and well-executed project produces results, as it goes, that justify its existence, whatever its initial aims and objectives. I see this approach as being complementary to the more conventional 'scientific' approach of hypothesis, then test, then rejection, modification or validation; this is a method better suited to shorter, perhaps less intensive projects. Our approach, which is more 'organic' in its organisation, is well adapted to long-term projects within the CRM sphere.

We believe, and it is for others to judge whether we are right, that this flexible approach frees CRM work from unnecessary self-imposed restriction and allows the unexpected -- even the unexpectable -- to be examined. Finally, our highest-level aim is to break the inherent

(Archaeological Review from Cambridge 4:1 [1985])

circularity of current archaeological reasoning -- a circularity imposed by its own methodology. I need hardly add that our effect on the way the subject is practised will be minimal in absolute terms; nonetheless, the effort is probably worth making.

I have approached our project from a historical standpoint in the hope that by doing so, readers will be able to appreciate what we mean by 'flexible'. I freely acknowledge that our procedure has many imperfections -- but it has produced some splendid, and thoroughly unexpected, material. It also has the added advantage that it does not pretend to be perfect, either in methodology or conception. I will close the paper with some thoughts on how I would like to see the work develop in the future. I make no apologies for the (intentional) narrative style.

Origins and Development of the SW Fen-edge Survey

The idea for a large-scale investigation of Fen dykes came about sometime towards the end of 1980. David Hall, Fenland Field Officer to the then Fenland Research Committee, was producing an important discussion document on the archaeological potential of the region. This (unpublished) document provided the major stimulus for the latter, and much larger, Fenland Project whose base, and HQ, is in the Department of Archaeology at Cambridge (Committee Chairman, Professor John Coles). In those days, David and I seemed to have more time to think and talk about the Fens, in the Fens; and there can be no substitute for being surrounded by one's research problems, as a tangible, physical entity. David's brief was to undertake a rapid, field-by-field, surface survey of the Cambridgeshire Fens, working mainly in the winter months, when the earth was bare, at a staggering annual rate of 25,000 acres. The early years of the survey produced extraordinary results, of which the most exciting were the buried barrowfields (Hall, forthcoming).

One day in the autumn of 1980, David arranged for myself and the team (then excavating the henge monument at Maxey, a Fen-edge site eight miles north of Peterborough) to clean up the sides of a drainage dyke (ditch) where it cut through one of the barrows of the Haddenham (near Ely) barrowfield (Pryor 1982, Figure 11). As I recall, it took six of us a long (and very strenuous) afternoon to clean both sides of the ditch, for a length of c. 40 metres. At the time, I remember thinking how much easier it would be if we could do the cleaning with machines, (as on a surface site), except that finds' locations on a steeply sloping ditch side are unlikely to have the same spatial significance that they possess in the plough-zone. In short, it seemed a good idea.

In 1980 I moved house, away from Peterborough (a city on the Fen-edge) to the true, deep Fen near Wisbech, just 15 miles from the Wash coast. This move was important, as I was able to observe, on a daily basis, the different seasonal jobs of the modern Fen farmer. It soon became apparent that the autumn and winter months were the times when farmers and drainage authorities recut (to use the archaeological term)

their ditches. Sometimes this work merely involved 'slubbing-out', in which the lower third or quarter of the ditch was scooped free of 'rapid-silt', to use yet another archaeological term. In general, unless one knew the locality particularly well, it was difficult to decipher the very partial stratigraphy revealed in the 'slubbed-out' sections, and we have usually stayed clear of them.

The Maxey report-writing ended its traumatic phase in 1981 and I was able to invent some excuses to escape the office and think about what ought to be done next. I had made the connection (painfully slowly, I should add) between monuments, such as barrows, and freshly cleaned dykes, and lost no opportunity to look for samples of the two. Unhappily, they rarely seemed to coincide. It was discouraging and very frustrating, until we had some remarkable luck.

Excavations at Maxey had ceased for some time, but we still maintained a close relationship with the quarrymen and staff there. When we learned that the pit was to be expanded eastwards we were particularly interested in case topsoil removal operations clipped a part of the ditch of a probable causewayed enclosure. The site in question -- now known as Etton to distinguish it from the Maxey complex -- was very faint indeed on air photos, and its very existence was in some doubt. This poor showing on air photos was a result of the thick deposits of stiff clay alluvium which blanketed the better-drained gravel subsoil at this point. The quarry kindly loaned us a mechanical excavator for a weekend, and after we had removed a metre or so of solid clay, there, to our considerable delight, was the buried ditch of the causewayed enclosure. Our subsequent pilot excavation proved it to be both middle Neolithic, and partially waterlogged to boot (Pryor and Kinnes 1982).

Etton was particularly interesting in that it was completely buried: the only way it could be revealed from the surface was by plant roots, and then only in very dry seasons. The depth of clay has defeated the usual surface geophysical techniques and of course the collection of surface finds is impossible. However, the site was cut, along its southern edge, by a drainage dyke which penetrated below the clay near the centre of the enclosure, where the ground below rose in a natural knoll. Eventually, we put two and two together, and it became apparent that we could use dykes as a means of prospection.

Development of the Technique (before fieldwork)

By the time that the dyke survey idea had taken root, David Hall had produced his interim assessment of the Cambridgeshire Fens for the Fenland Committee. His results had an extraordinary impact, even on those of us who had been able to witness their accumulation. We all realised that the buried landscapes beneath the Fen were just as important as the more traditional truly wetland sites (i.e. crannogs, trackways, fish weirs and the like), which we also expected to find, in this rapidly drying-out and intensively farmed region. Clearly the

region had hidden potential without rival anywhere in England.

So to sum up the early work, the main aims of the dyke survey, as understood in 1981, were to prospect for buried sites and landscapes. However, shortly after these ideas began to take hold, it was arranged that the team who would be supervising the proposed dig at the Etton causewayed enclosure would visit the Amsterdam Institute's excavations at the Assendelver Polders. We reasoned that the problems faced by Dutch archaeologists would closely mirror those we were about to tackle. I do not have the space here to list all the lessons we learned from that remarkably well-run and well thought-out project, but one point was very clear: even superbly preserved sites lose much of their meaning unless they can be placed in their environmental and cultural contexts. The former can be done by borehole sampling and similar techniques, and generally presents few major practical problems, provided that is, that suitable deposits exist close to the site in question. The latter, however, is far less straightforward.

The Amsterdam project carried out their site prospection (and thereby helped to provide cultural contexts) by means of dyke and bore-hole survey. However, although superficially similar (i.e. flat, low-lying and near the North Sea), the Dutch and British Fenlands are very different today. The British Fens have been used for growing crops on a large scale since the last War; before that, drainage (since the early 17th century) had led to much peat 'shrinkage'; however, despite this, the erosion of the past 40 or so years has been far more severe. Water levels are maintained several feet below the surface (this varies from one drainage authority to another, and from season to season), and the desiccated peats and other fine-grained soils simply oxidise or blow away. In the months of spring it is common to see the sky turn brown as peat is transported in a 'fen blow'; when digging at Fengate I was surprised, on two or three occasions, to discover my stripped site surface covered with spring barley seed and nitrate granules.

The Dutch Fen landscape has a different recent history (de Bakker and van den Berg 1982). It was realised, some time ago, that arable agriculture was eroding the peats, and accordingly ploughing was discouraged. Water levels are kept a few inches below the surface, and far from ploughing the land during the war, the Dutch actually flooded large parts of the lowlands, as a resistance measure. These major post-depositional differences between the two countries have had a direct bearing on techniques of archaeological prospection. Large-scale mechanical bore-hole survey is possible in Holland, where grazing can be returned and where standing crops are rare. In East Anglia, on the other hand, the seed drills have been known to follow the combines within hours. This means that when we do use boreholes, they are hand-driven and small-scale (being very exhausting to operate). We generally used them to define the extent of sites revealed in the dykesides.

If it is easier to sink boreholes in Holland, it is harder to view dykelines, for the simple reason that they are filled with water, and

black water at that. Accordingly the dykes are surveyed by inspecting the upcast dumped along the brinks, after mechanical recutting or 'slubbing-out'. Although a useful procedure, and one that has found many sites, it is necessarily less precise than an inspection of the cleaned ditch sides in situ, as we are able to do in East Anglia. I suppose the main lesson we learned from our Dutch experience was that dykeside/borehole survey deserves to be taken just as seriously as excavation itself. It cannot be satisfactorily executed on a part-time ad hoc basis.

We had been pondering the whys and wherefores of dyke survey for about a year and a half before we actually received funding to do it, in the autumn of 1982. Looking back on it, I am glad there was a delay, because our ideas were still half-formed (and in hindsight, half-cock); even if, when expressed on paper they looked plausible enough. At all events, our initial intention, as we set off along our first dyke in September '82, seemed clear enough: we were to concentrate on buried soils and were to make much use of in-the-field geophysical and geochemical techniques, such as the measurement of magnetic susceptibility enhancement and soil phosphate concentration, both of which had been used successfully on our previous excavations. This approach, moreover, gained added support from our visit to the Netherlands.

Finally, before we launched ourselves into the field we realised that our survey was more than just a search for buried sites and landscapes. The Fens are drying-out and 'shrinking' at an alarming rate and we determined to attempt to monitor this process, both by recording water levels and land surface levels, especially in the vicinity of archaeological monuments, such as barrows. Every profile we recorded along every dyke was to be levelled in at the top and bottom of each observed layer. So far we have managed to keep to this standard of recording, but there have been times when we have been sorely tempted to let it slide...

Development of the Technique (during fieldwork)

We divided into two teams of three people. Each team carried a dumpy level, tripod and staff, a 50m tape, range poles, two cameras and film, a spade, a portable magnetic susceptibility meter, a supply of bags for samples, record sheets and clipboard, a photographic record board, 1:2500 maps, field-glasses (for wide dykes) and of course sandwiches and a vacuum flask, for each person. Many of the dykes were very long and we were often unable to return to the Land Rover before the end of the day -- add to this mud which accumulated on feet as 'moonboots', and it will be apparent that we were carrying too much. Sometimes the teams were reduced to just two people, and then life became impossible, and multiple visits to the same stretch of dyke became necessary.

We had originally decided to record dyke profiles at 30m intervals in order to allow comparison with the Amsterdam project, but we soon found that this was hopelessly over-elaborate, as many of our dykes were

belated realisation (and it took about a month for this particular penny to drop) that there was more to dyke survey than buried soils alone, has made our task very much more difficult: in practical terms, it has forced us to broaden our horizons; we can no longer concentrate our attention on a single, relatively narrow band of sediment and when one is trying to to walk at an angle of 45 degrees on a dykeside, breadth of vision presents real problems of balance and concentration. Further, the harder the task becomes, the fewer the people we can rely on to do it adequately (and as dykes are available perhaps once every 10 to 20 years, we cannot afford to waste our opportunities).

Any competent fieldwalker is aware that he or she must be able to recognise flint or pottery; in most cases animal bone can be ignored (due to modern contamination etc.). We expect the dykeside surveyor to be able to recognise (a) a buried palaeosol, (b) a significant arrangement of roundwood or brushwood -- e.g. a track or footpath, (c) the various types of split or worked timber, (d) organic artefacts -- in theory at least, (e) archaeological features such as post-holes, in oblique section, (f) flint, pot and bone, both *in situ* and on the surface upcast. It is a tall order for most people and we find it difficult to make good use of students or amateur volunteers. One result is that dyke survey, like other branches of wetland archaeology, is not particularly cheap to perform. That does not mean, of course, that it is not cost effective.

The Present and Future Plans

We have recently prepared a paper describing the rationale behind the dyke survey and the way in which our recording system works (Crowther *et al.*, forthcoming), so there is no need for a long description here. Suffice it to say that profiles are recorded by our soil scientist on proforma sheets and sampled as necessary, by him. The different horizons are levelled-in and the profile location recorded on 1:2500 maps. The basic record is provided by 35mm colour slides, of which at least four (two distant from both directions and two close-ups) are taken at each profile. We are aware that slides do not last for ever, but monochrome photos of soils are generally useless (especially in the peatlands), and video tape has an even more limited life.

I have already noted that our project has experienced growing pains, but we are now confident that those problems are largely behind us; next we must consider where to go from here. I would very much like to see a larger area of land covered, in order to compare different regions within it, but resources -- especially experienced people -- are in short supply, and there is always the danger of attempting to run before one can walk. We have yet to come to grips with the problems posed by off-site archaeology and this is regrettable, as our data is uniquely suited to this type of enquiry. Off- (or non-) site archaeology is a topic we made much of in our original research design, as submitted to the DoE; however, in the event, sites have tended to dominate our work. Apart from anything else, sites like Flag Fen mean

something to the taxpayer -- who ultimately funds our work -- whereas off-site archaeology is a somewhat recondite concept. However, now that we have a good supply of sites under our belt, I hope we will be able to consider the significance of the huge tracts of buried soils that link (or separate) them.

Our basic and, I hope, objective record is provided by colour slides, a medium with serious physical drawbacks, and we are currently giving serious consideration to the setting up of a centralised Fenland 'soil bank', where consolidated, but intact, blocks of soil could be kept for reference and study purposes. Ideally this 'bank' would eventually serve the whole of Fenland. Although subject to the same biases as an observed sample, a preserved sample must be an improvement on a mere photograph, albeit augmented with drawings.

We will re-assess our project in a more formal manner -- as opposed to the continuous modification discussed here -- in three years time, when we should have a sufficient body of data available. I cannot predict what that re-assessment will decide, but I have a strong feeling that dyke survey, or a modified version of it, is the most cost-effective way to examine large tracts of buried landscape fast and thoroughly. True, it is haphazard, rather than random in the statistical sampling sense, but given the complex and still unpredictable nature of the subject-matter, I can live with this imperfection; as we still do not understand the complexities of the buried palimpsest of human or natural landscapes, it is hard to see how they could be sampled to produce useful information free from bias. In the interim, I would rather accept a known bias towards wet places, as that is where dykes are located and where the most interesting sites are to be found.

Conclusions

I hope that this discussion, by pointing out how a project can change as it matures, will also illustrate the advisability -- I would even say the necessity -- of keeping both research objectives, design, and *modus operandi* flexible. By saying this, I do not wish to criticise the hypothesis-testing and to all intents and purposes 'scientific' methodology of the once New Archaeology. I believe that the rigour brought to our subject by the New Archaeologists was essential. What I am trying to say in this paper -- in a deliberately oblique, circumlocutious fashion -- is that there is more than one way to skin a cat, dig a site, have an idea, or do good archaeology. I suppose I am making a plea for academic tolerance; for example, I have found by experience that pondering problems while doing something quite different often produces useful insights. It does not always pay to approach a topic head-on: i.e. define, test, then accept or reject. Sometimes (but by no means always) this approach leads to the definition of problems that are too far removed from the data to be testable; alternatively we may find that elegant, but hopelessly inappropriate tests have been used to tackle straightforward issues.

If this narrative account of how our dyke survey developed has any message at all, it is that research designs can develop slowly, and should be encouraged to grow, to change and improve. A small team working in a given area (the boundaries of which should change as the research changes), for a long period of time, often becomes totally immersed in the area's past and present. One's interest of necessity becomes diachronic and one trades the discipline of geographical restriction for the greatest freedom of all, that only archaeology can provide, namely, the license to move through great expanses of time. Provided that the research team is able to keep abreast of modern archaeological developments (and this is not always easy), the process of immersion somehow filters out archaeological irrelevance. Real problems, i.e. those that stand some chance of being examined against real archaeological data, come to the fore seemingly of their own accord. I cannot explain this process and do not intend to try; but it happens.

In short, parish-level archaeology need not be parochial, although sadly it often is. As we pass along our dykes, do we progress from the particular to the general, deducing, as we go, successive law-like generalisations? I suppose we do, after a fashion, but I would rather not think about it too closely. After all, when something is thought to be explained, it loses much of its mystery and thereby its fascination. Perhaps this is why, on reflection, I found New Archaeology (with its obsessive concern with explicating everything) most worthy and commendable (in a very self-conscious way), but intrinsically tedious. However, having said that, it did force one to re-examine accepted explanations -- the conventional wisdom -- from the data upwards, and thereby provided the stimulus that led me to the Fens. And I have never been bored since.

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TOPSOIL AND THE ARCHAEOLOGY OF THE SOLWAY PLAIN 1981-84

R.H. Bewley

ARC Volume 1:2 published a short article on the approach to the study of the archaeology of the Solway Plain, Cumbria (Bewley 1982). Three years later, with the research finished, the author is now able to offer a portion of that work and show how it relates to 'topsoil studies'.

There are different ways in which archaeologists can use the topsoil to obtain information about the occupation of a site or the settlement of a region. For my research I divided the fieldwork into site specific and regional studies (Bewley 1984, 100-133). In this article I will report only on the topsoil aspect of the work, as a full report of the fieldwork and excavations will be published in 1986 (probably in the Transactions of Cumberland and Westmorland Antiquarian and Archaeological Society, hereafter T.C.W.A.A.S.).

Site Specific Fieldwork

Three sites were chosen for this work, mainly because of their 'availability'; although many more were fieldwalked by a local team of volunteers (see T.C.W.A.A.S. 1985). The site specific approach can be summarised most effectively by a diagram, see Figure 1. In this instance Aerial Survey was the starting point, but obviously this is not always the case. The important part of the process is the Fieldwalking-Geophysical-Soil survey stage, and it is this aspect which I will explain here and the results achieved by this process.

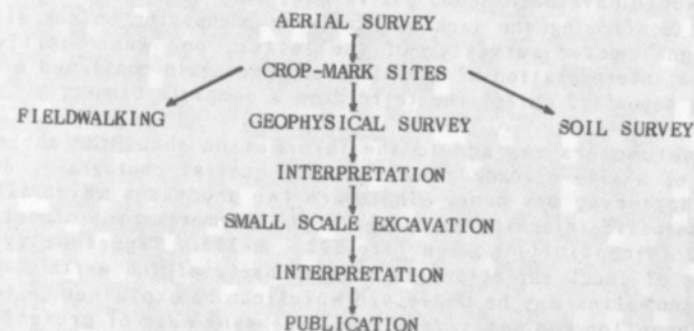


Figure 1: An approach to site specific fieldwork.