(craft)

[Draft of Appendix to Alan Macfarlane, *The Culture of Capitalism* (Blackwells, 1987) pp.191-222.]

Individualism reconsidered, or the craft of the historian

The essays in this book have restated, elaborated and attempted to confirm the central argument of **Individualism**. We may wonder, however, to what extent the theories in that book have survived the critical scrutiny of the community of historians. For while most reviewers are agreed that if I was broadly right, then fundamental changes would have to be made to our view of the English past, many have argued strenuously that the thesis is either unproven, or erroneous. That thesis, namely that there was never a conventional peasant society in England and that the supposed revolution from peasant to individualist, or feudal to capitalist, did not occur in the way that many sociologists and historians have suggested, has been restated in chapter one, and in passing in many of the other chapters in this book. I will not restate it here but will look at some of the objections which have been brought against the argument.

More than fifty reviewers have dissected the book in over a hundred and twenty pages of published reviews.(note 1) I am grateful to them for many constructive criticisms. If my reply sounds somewhat defensive, this is because I have chosen to concentrate on the strongest criticisms and queries.(note 2) I am here concerned to answer criticism. What then remains of the central thesis and what modifications need to be made in the light of comments? It is a humbling exercise to re-examine a work that has been chewed over so carefully, and the tone of certain critics has not made the task easier. But readers may be interested to know what is left of the ship after the gales have blown and the counter-blasts fired.

The heart of the argument of **Individualism** concerns the growth of the concept of private, individual, property. The work is an attempt to refute Marx and those who follow him in believing that only through a legal revolution sometime after the end of the fifteenth century was the "modern" concept of private, individual, property introduced. Marx had argued that "the legal view...that the landowner can do with the land what every owner of commodities can do with his commodities...arises...in the modern world only with the development of capitalist production". Capitalism as a system "transforms" the "feudal landed property, clan property, small-peasant property" into modern, individualistic ownership.(note 3) Through an examination of the work of legal historians such as Maitland, through an analysis of early law books, and through a re-examination of the arguments of medievalists like Homans, who appeared to have distorted the court roll evidence, I attempted to show that property was highly individualized by the end of the thirteenth century, if not much earlier. It was held by individuals and not by larger groups; it could be bought and sold; children did not have automatic rights in land; there is no evidence of strong family attachment to a particular plot of land. Thus the core of the argument of the book is, as can be seen in a mild form in chapter one above, of a legal and technical kind. It is on this argument that the book's thesis ultimately stands or falls. The reactions of critics to this argument is revealing.

G.C.Homans reviewed the book and does not attempt to refute my criticisms of his work. He admitted that "The Common Law did indeed make the transfer of land between living persons relatively easy...and by the end of the thirteenth century there was an active market in land, even land held in villeinage", even though "in theory, transfers of such land required the consent of the lord of the manor". He merely re-asserts referring back to **English Villagers**, "especially in the common-field areas of England", in practice villagers "tended to leave their standard holdings intact to their heirs, usually their eldest sons" and to give their daughters dowries and to support other children who were unprepared to leave the holding. This is, in fact, far less than what was argued in the book. It is a weakened assertion which is much easier to accept. It is significant that Professor Homans should not have attempted to defend or comment on my detailed criticisms of his use of, and inferences from, medieval court roll materials.

Only two of the other critics of the book even begin to tackle this most central argument. By their silence, one has to presume that many of the rest either tacitly accept the argument, or feel incompetent to argue on the matter. Their failure to discuss the most central technical issue, let alone reject it, is the clearest indication that the central thesis of the book, its most important argument around which all the other arguments are ranged, has withstood the test of criticism. It is in the light of this tacit acceptance that criticisms directed at less essential arguments should be evaluated. The critics can hardly have been unaware of the centrality of this discussion. The two middle chapters of the book are devoted to "Ownership" from 1350-1750, and from 1200-1750, and comprise some fifty pages, or one quarter of the book. What are the arguments of the two reviewers who have attempted to challenge parts of this thesis?

Dr. Rosamond Faith agrees, in essence, with my argument that property was held by individuals and not by wider groups in medieval England. Indeed she suggests a more radical theory, namely that this feature may be present much earlier, in pre-Conquest England. She accepts that there was no restraint on alienation of property imposed by the lineage, that "the common law in the middle ages certainly exemplified a much more individualistic attitude to land than existed on the continent". Her major criticism is that medievalists, and probably even Marx, knew this all along. I have created a straw man which I then attack. If I have done so, I can only regret that certain writers did not make their views more explicit. Certainly a number of historians and sociologists, and not only myself, have been confused into thinking that they believed that there was some kind of group, family-based, peasant property system up to the fifteenth century, which was then radically transformed by a new capitalistic and individualistic framework.

One of my arguments was that where land is fully alienable and often bought and sold, it is unlikely that there will be that characteristic sentimental attachment to specific "family" plots found in many peasantries. There will not be a strong feeling that "the name should be kept on the land", that is the family name on specific inherited lands. Dr.Faith shows more ambivalence on this question. On the one hand she agrees that there was little attachment to specific pieces of land. But she is reluctant to give up the idea of a generalized high sentimental valuation of land. Thus while she agrees that land was bought and sold freely and frequently for the majority of the period after 1300 and probably before, she argues that since land was "virtually the only source of subsistence" for the major part of the population, it could not be described as a "commodity". Land, she believes, must have been emotionally important to medieval people, because it was so crucial in the economy.

Obviously no-one would deny that land was of enormous economic importance, even if it is also necessary to remember the many other sources of income from manufacture and trade in medieval England. But this is true of England up to the later eighteenth century and it is still true in farming regions today. Yet most historians would concede that in the later seventeenth and eighteenth centuries there was little sentimental attachment of a "peasant" kind. What this shows is that to say that something is economically important, in itself, tells us little about its social or symbolic importance. The other reviewer who has commented in any depth on this central argument is Professor Hilton, manifesting in a classic way that ("we knew it all already, but it is all wrong anyway") ambivalence to which Elton drew attention. Hilton alleges that I am "bemused" by my discovery "(well known to others)" of the **ante mortem** alienability of freehold tenures. While "surprised" would be a better adjective, the important fact is that Hilton agrees that in relation to freehold, I appear to be right. But this admission is then undermined in a curious way, namely by obliquely attacking F.W.Maitland.

Hilton alleges that my discussion of medieval property is based "almost entirely on F.W.Maitland". Although I do rely heavily on Maitland, readers of the book will see that this is a considerable exaggeration. Maitland is essential, but he is supported by many other cited sources. Having tied me to Maitland, Hilton is then in a position to undermine me by undermining Maitland. Firstly, it is implied that Maitland is somehow discredited for the period on which I was writing, namely from 1300 onwards. I am accused of ignoring S.C.Milsom's "gentle warning that Maitland 'sometimes places highly abstract notions of property too early'". One might answer that Milsom's views are as liable to be wrong as are Maitland's. But there is a better answer. Hilton must have read two sentences further on in Milsom's preface that "If Maitland's picture was not true to start with - and here the heretic is most disturbed by his own heresy - it came true". What is meant is made clearer in the same introduction by Milsom when he writes, "There can be no doubt that by the end of the period covered by his book, the world was as Maitland saw it."(note 4) Now the end of Maitland's period is 1307, the start of my period is 1300. To suggest on Milsom's authority that Maitland cannot be trusted for the early thirteenth century onwards is, to put it mildly, somewhat misleading. But this is not the end of the attempt to undermine Maitland as an authority.

Professor Hilton suggests that Maitland is "in any case mainly talking about freehold among the upper classes, such as military tenures". A non-medievalist reading the review, never having read Maitland, might accept this **obiter dicta** from a leading medievalist. If I have really depended "almost entirely" on this authority, and he is hardly concerned with other than freehold tenures, I must surely be on very weak and unsupported ground. But it is in fact a considerable distortion, as will be evident even from a glance at the table of contents which shows that that "Pollock and Maitland" devotes a number of sections and chapters to each of the different kinds of tenure. For instance, volume one, pages 356-383 is specifically devoted to "unfree tenure". To suggest that Maitland's writing is not directly relevant to all groups in medieval society is surely indefensible.

Having severed me from my one apparent authority, I am then open to destruction by the expert. Professor Hilton argues that I have misunderstood the nature of villein (that is, servile) tenure, "attempting to assimilate it to freehold as though it were equivalent to sixteenth-century copyhold". Hilton does not seek to defend the position that there was group or family ownership. But he argues that villeins were "severely restricted" in their rights "by the control exercised by lords". There was a concentration of power into the hands of nobility, gentry and clergy and hence it was ridiculous of me to talk about rights, individualism, and so on at this level of society in the medieval period. The same point is made by Lawrence Stone, who wrote that "it is symptomatic of Mr Macfarlane's myopia that he totally ignores the close communal control, through the manorial court, of almost every aspect of the use of property. Such courts could tell people when, where, and what to sow or reap..."

There is a half-truth in this argument. No-one would deny that from a very early period lasting up to at least the middle of the eighteenth century, there was a considerable concentration of property which put considerable pressures on "the small man". I have examined the restrictions imposed by manor courts in Earls Colne from l380, and in the more conservative north of England from the sixteenth century. Of course tenants were circumscribed and had to work within the rules. But my argument is in fact rather different. I argued two points. Firstly, that the **de jure** position, as shown by an examination of the legal sources, was that property, the right to alienate, was not held by the family but vested in individuals. This was present from very early on and is, cross comparatively, very unusual. This is not challenged by Hilton, Homans, Faith or any other reviewer. Secondly, I argued that the **de facto** position as shown by recent work on court rolls and other documents seems to have been that people exercized this right very widely. Land sales to non-kin were very frequent, even by those "villeins" whom historians for long thought were incapable of such activities. This fact also is not challenged. Having studied manorial courts in the fifteenth and sixteenth centuries and seen how they combined control and direction on the one hand, with a perfect ability to allow people to buy and sell land on the other, I see no particular incompatibility. How this was done needs to be documented in detail. What does not seem to have been shown is that in the central argument I was mistaken.

Since it might be thought that my pleading is partial, perhaps I may bring forward a relatively impartial authority. Dr. Paul Hyams is a medievalist specializing in these very subjects, namely the legal position of villeins and the nature of early medieval law. Any inappropriateness of my use of Maitland, any misuse of medieval legal concepts, any distortions concerning discussions of property would have been revealed by Dr. Hyams, who reviewed the book. In particular, any defects in my extended attempt to show where G.C.Homans' work had been misleading, a fairly technical subject, would have been pointed out. Dr. Hyams is aware of the seriousness of my challenge. He writes that Homans' views of the medieval peasant community has, for forty years "never faced real challenge. Now Alan Macfarlane offers that challenge..." While Dr Hyams has some criticisms concerning my model of peasantry and notes some sins of omission, to which I will return, there is significantly no defence of Homans, no criticism of the use of Maitland, no fundamental disagreement with the central thrust of the argument concerning the very early development of individualized concepts of property. He concludes by urging that "Specialist indignation of the righteous kind ought not to obscure the book's genuine achievement".

Although this advice has not been heeded very much, it is difficult to see that the central thesis of the book has been seriously challenged, let alone refuted, by counter-arguments. Until a serious and convincing challenge is mounted, I see no reason to alter the argument. Marx, Weber and those who have followed them were wrong. There was no revolutionary change from one, pre-capitalist, economic formation, to another, capitalist one, in England in the fourteenth, fifteenth, sixteenth or seventeenth centuries.

While the central argument appears to me to stand, critics have rightly pointed to a number of possible weaknesses in the deductions made from this argument, and in the ways in which it was presented. One cluster of criticisms we might label as sins of omission. Many critics would have liked to see the book investigating many other topics. The longest single list of omissions is provided by another medievalist, Dr. John Hatcher. He points out that "Rent, conditions of tenure and lordship are scarcely mentioned...the odd passing reference to such matters as villeinage, competitive tenures, estate management, farming methods, occupational structure, standards of life, consumption patterns, land-labour ratios, productivity, towns, trade, industry, technology, communications, finance and so on, would not have been superfluous". In fact, there are passing references to many of these, and Dr.Hatcher has overdone the irony. But he has a good point when showing that in a short book many of these topics were hardly developed in any detail. Other topics which reviewers have suggested that I should have devoted some, or more, space to include the following; external relations and world markets, the struggles of classes, the labouring classes and social class variations, violence and suffering, the State and state regulation, the role of the bourgeoisie, the historical preconditions of the common law, custom, the role of the Church and religion, the Black Death, enclosures, the open-field system, the traditional village and the manor, serfdom, law in relation to government and politics. Finally, several reviewers, for instance the anthropologist Dr. Rheubottom, drew attention to the omission of any extended treatment of "individualism as a cultural system". It is suggested that while I seem aware that "individualism is more obviously connected with morals, politics and general culture than with land-holding", as Dr.Harding writes, I tend to ignore these dimensions on the whole.

Two points can be made concerning these formidable omissions. The first is that those who drew up the lists do not suggest that if I had considered all, or some, of these topics it would have either undermined or even weakened my argument. Rather, they believe it would have added further weight to it. Thus, for instance Barbara Donagan writes that the absence of a discussion of the moral and sentimental dimensions "leaves us with only half a picture of these rediscovered English individualists". More harshly, Dr.Baker points out that I "foolishly" ignore "the role of cities, of industry and of commerce in medieval England which would have lent support", to my "main thesis". Thus the argument I put forward in the book, and which has been greeted as unproven and controversial, could be very greatly strengthened.

The critics are, of course, right. A full treatment of the origins of English individualism would include all those topics listed above, and many others which I could add, such as the role and nature of political power, the legal and administrative structure, language, literature, art, ideology and so on. My difficulty was that I wanted to write an interpretative essay, putting forward a simple hypothesis as clearly and succinctly as possible. I could have waited twenty years and assembled huge quantitities of material into a set of volumes which could scarcely have been less weighty than Braudel's massive three volume history of "Civilization and Capitalism". I hoped that by going to the heart of the argument, the relations of production, I would then be able to lay a renewed framework on which I and other could build. In particular, in relation to the moral order, I wrote in justification in the postscript , that the difficulties of using much more literary evidence, "combined with the desire to keep the argument relatively simple in a first presentation" led me to reserve the treatment of the moral order, sentiment, and so on to a later occasion. The essays in this book are a partial fulfilment of that promise, dealing with selected small parts of the political, moral and sentimental order. In these essays and subsequent research I have tried to answer some of the very sensible criticisms and in particular that of Donagan and others that one cannot understand the free-floating individualism described in the book without seeing how individuals were linked to the society and State. What would seem a fair assessment is that by Dr.Hyams. He points out that the argument needs refinements and finer gradations and further evidence. "Macfarlane recognizes the need. His scale and level of generality perhaps absolve him from the duty to satisfy it." It seemed to me worth the risk.

The sins of omission are also linked to another perceived weakness, namely that I ultimately failed to explain the origins of the supposed individualism of the English. A number of reviewers were disappointed that my conclusion was so enigmatic, namely an oblique reference to Montesquieu and the German forests. Having apparently destroyed alternative solutions to the problems of the origins of modern society, they hoped that I would provide something fully worked out and convincing to replace them with. Frankly, so did I. But the task is a very large one, not to be undertaken by one individual or in a few years. I decided to drop a hint, and then to think further about this and encourage others to speculate on these themes, particularly those more expert in the period before the twelfth century.

In the intervening years since **Individualism** I have indeed tried to push the argument further and attempted to provide at least a partial answer to the problems posed. My main two lines of thought are represented in the last chapter of my recent book on marriage and in chapter eight of this collection. In that chapter, I have taken up a remark which I made at the end of **Individualism** where I mentioned that I seriously doubted that English feudalism was like other feudalism. I have undertaken a preliminary examination of this, and of the related questions of lordship which Hyams and others have rightly commented on.

Another set of criticisms concerns my treatment of historical change. According to some of the reviewers, I denied the existence of change. Three examples, out of half a dozen along these lines, may be given. Barbara Harris states that ultimately my book "is a historical work that denies the existence of fundamental historical change for a period of over five centuries. Such a claim boggles the imagination..." She then lists eleven major changes that occurred during this period. It does indeed sound boggling. Or again, Dr.Hatcher finds my supposed hypothesis of "half a millenium of social stability culminating in the Industrial Revolution" very weak, arguing that this is a book "which seeks to persuade us that in essence society and economy did not change in the 500 years prior to industrialization..." Perhaps the most extreme statement of this criticism is by Professor Stone, who writes that I "seem to claim" that because there was not a revolution in property relations, this "indicates that any idea of significant progress - or even significant change from the thirteenth to the eighteenth century is a mirage". It is strongly implied that since I do not formally affirm my belief in the Renaissance, Reformation, agricultural revolution, commercial revolution, growth of London, spread of literacy and the invention of the printing press, the development of political theory, and other changes Stone lists, I do not believe they occurred or, if they did, had much effect.

It would appear from such reviews that I must be eccentric, if not insane. What was I doing during all those years at school and University studying English history if I managed to avoid noticing all these changes? The answer is that by exaggerating my position, these reviewers have managed to make it ridiculous. It is a well-known demolition technique. The fact that I do not mention or stress something, for instance the growth of London, or invention of printing, cannot be taken to mean that I do not believe it occurred, or do not know of its existence. If an author had to list every event that occurred during his period of study or be accused of being ignorant of it, books would become very long indeed, and exceedingly boring. Of course there were fundamental and important changes of all sorts and kinds. My thesis is, I hope, slightly less foolish than this, namely that the speed, nature and depth of change in England is remarkable.

In essence, I am arguing that there was no **revolutionary** change in this period from one order of society, which one could label "peasant", "medieval", "feudal", "pre-capitalist", or whatever one likes, to another, which is the total antithesis of this. I deny a sudden, dramatic and all-encompassing revolution located in a few decades, whether in the sixteenth, seventeenth or eighteenth centuries. This does not mean that there were not deep changes of many kinds and it would clearly be ridiculous to argue that nothing changed. As Professor Elton wrote, "One argument already being employed against Macfarlane misjudges him: he never denies that even large changes occurred in the six centuries before 1800, only that change was linked to major upheavals in the social substructure." The fact that some critics have drawn this conclusion and that Professor Elton feels he needs to come to my defence, does suggest that further discussion of the historian's problem of combining continuity with change is needed. Chapter seven of this collection is a more extended treatment of this problem, where I try to show what I mean by revolutionary change and how it is possible to believe in both continuity and change. I hope that this discussion will go some way to answering those critics who attack me from one flank as an old-fashioned Whig (Goldie, Pocock), on the other as a latter-day Idealist (Blok), and from a third as a nineteenth-century materialist (Goldie).

Another group of criticisms concern the nature of the historical evidence I have used and the way I have analysed it. The late Philip Abrams argued that I have brought the wrong sort of evidence forward to try to solve the question of whether there was, or was not, a traditional peasant society in medieval England. As he rightly points out, this is essentially an "analytical and qualitative", rather than an "empiricist and quantitative" problem. To find out how many people were or were not 'peasant' is not helpful, "the head count is simply not the relevant test". What we are interested in is structures and relationships. With this I entirely agree. Obviously the argument was not as clearly expressed as it should have been if Abrams thought that I was mainly interested in quantities, that my main evidence was "demographic", or that it was "essentially empiricist and quantitative" as he claims. My position clearly needs clarification.

Of course there is a good deal of discussion in the book of quantitative matters, the number of sales of land, the size of landholdings, the rates of geographical mobility, the proportions owned by various groups. In a discussion of peasantry, it seems difficult to avoid such matters. But the argument was meant, ultimately, to be analytical. This was indicated by setting out a theoretical model at the start which included many non-quantitative elements in it, such as attitudes towards land. The most detailed part of the discussion in the medieval sections has nothing to do with empirical facts or numbers, but is concerned with legal concepts. F.W.Maitland was not a noted empirical quantifier, nor are the authorities I quote in the chapter on 'England in Perspective' at all concerned with statistical or empirical facts. Indeed other reviewers have castigated me for not being sufficiently interested in such things, Rosamond Faith writing that my approach is "by and large...polemical and analytical", and Dr. Hatcher complaining that it is not sufficiently quantitative. Any misleading impression that I think that the questions of capitalism, individualism and peasantry are ultimately quantitative and empirical will, I hope, have been settled by the essays in this volume. Even the sharpest critic will find it difficult to accuse me of undue quantification or disinterest in analytic approaches in discussing such matters as violence, evil, nature, love or revolution.

A second, related, criticism is put more harshly. The medievalist Professor Herlihy believes that the book is "founded on faulty method" and propounds "a preposterous thesis". One way in which the method is faulty is in using the wrong documents. Herlihy claims that most of the records I have used "reflect the interests of landlords and princes. Both landlords and princes wished to assign responsibility for rents and taxes to single, easily identified, readily found individuals". They could not tax larger units. Thus the "individualism" that I find "tells us little about the interior life of the family, its values and its spirit". I am dismissed as having discovered "the individualism of the rent gatherer and tax collector", but I have not "gained access to the inner reaches of the peasant houshold".

There is, of course, a half-truth in this suggestion. It is indeed very difficult to go beyond the formal records for ordinary villagers before the sixteenth century. My own strategy was to do three things in order to probe behind the records. The first is to use such records as we do have as carefully as possible, usually for purposes for which they were not originally designed. As Marc Bloch put it, we have to interrogate the records; "from the momemnt we decide to force them to speak, even against their will, cross-examination becomes more necessary than ever. Indeed it is the prime necessity of well-conducted historical research".(note 5).

Secondly, we can place such formal records alongside other records in a period when these other sources do appear in quantity. This is one of the reasons why I worked backwards from the seventeenth and sixteenth centuries. During those centuries we can compare the impression from taxation and manorial records with other, more qualitative, records, particularly disputes in courts of law, wills, diaries and other sources. When we do this, as soon as the documentation is present, it is clear that similar taxation and manorial records to those for the medieval period do not merely reflect the individualism of rent and tax gatherers. They fit perfectly with the general mentality and the inner recesses of the family. This does not, of course, prove anything about the period before l500, but it is suggestive.

The third strategy is to work carefully back using the same procedure where we can. There are sources before l500 which allow one to probe behind stark lay subsidies or rentals. Ordinary records of the court leet and transfers in the court baron, ecclesiastical and civil court records, literary sources, textbooks of the time on law and morality, these can enrich our analysis. Combining all three approaches, I came to the conclusion that the "individualism" is not merely a surface phenomenon. Professor Herlihy seems convinced otherwise. He asserts, presumably as a matter of faith, or perhaps on the basis of some English medieval records of which I am not aware, that we know that there really was another world, those "inner reaches of the peasant household".

While this may well be true of the areas with which Professor Herlihy is familiar from his own research, namely southern France and northern Italy, where the opposition between the needs of the

State and landlords on the one hand, and the inner dynamics of peasant households may have been pronounced, it does not convince me as a helpful approach to the English evidence. The most that Professor Herlihy could logically maintain from such an argument is that my case is not proven. What he strongly implies is that I am patently wrong, "propounding a preposterous thesis". It is significant that even the fiercest of the reviewers who had worked on English medieval documents do not make a similar kind of criticism. Having worked extensively with English sources they are presumably aware that it is possible to go beyond a mere landlord's view of the world. If Professor Herlihy had analysed English documents and compared them with his North Italian ones he might perhaps have come to a rather different conclusion.

For those who have worked in southern Europe where medieval records are either very fragmentary, or are clearly entirely the work of those who are trying to extract money from a hostile peasantry, it is difficult to imagine that the English records are good enough to be able to test as complex a theory as whether the ordinary population were "individualist" or "familist". It would be equally impossible, for instance, to make such deductions in medieval India or China. Indeed, it is one of the characteristics of peasant society that the gap between the mainly oral and enclosed world of the peasantry, and the external world of State and landlords is so great that it is almost impossible to do much more than observe from outside and above. In such a situation, records cannot be trusted, they do indeed reflect the mentality of outsiders. Having spent some time working with a team which has been investigating the history of Portuguese social structure I have seen this at first hand. It is not surprising, therefore, that the only other critic to make a similar point, in a much more sophisticated way, has also worked on the records of southern Europe, and particularly Portugal.

Robert Rowland concedes that the English may well have been individualists, but points out that there is a danger that using a "Weberian/individualistic strategy for recovering the intrinsic meaning of social action (the meaning it has to the actor)", is only legitimate provided I have independent grounds for considering the society to be individualistic. I can proceed on the assumption that the society has such characteristics, but if I proceed in this way I should be careful not to argue in a circle and believe that I have proved what I originally had to assume. The way of escaping the circularity, Rowland suggests, is to consider alternative assumptions, of a non-individualistic, holistic, nature. If such an analysis confirmed a basic individualism in the English past, this would validate my argument. A wider range of sources would allow me to sketch out an analysis based on such alternative assumptions. I have only partly answered this objection. I have used a wider range of sources in this book which allow me to look at the political, legal and moral dimensions which are largely missing from that book. Using more qualitative sources, one would not have to impute intrinsic meanings to observed actions. The essays in this book have followed this suggestion. Most of them are concerned with qualitative matters where there are many contemporary statements, a sufficient amount of qualitative and independent evidence, which seem to confirm what was only a hypothesis in **Individualism**. Even on the basis of **Individualism** and my work on violence (see chapter 3), Rowland felt that if one stripped away all the suspect imputations "enough remains...to suggest that English society...was peculiar, and that this peculiarity appears to derive from the fact that the social system institutes the **individual** (and not the group or quasi-group) as the locus of production of the meaning of social action." In these essays I hope that I have begun to move towards that "greater solidity" which Rowland requests, and which allows me to argue that I had "independent (i.e. not methodologically individualist) grounds for arguing that England was an individualistic society and, assuming this to be the case, for attempting to describe the motivations and orientations of actors".

Another type of criticism concerns typicality and generalization from specific cases. While one historian, Dr. Hyams, has rightly pointed to the need for further refinement of my argument by looking at a wider range of examples to counter the "geographical sameness", the main criticisms do not come from the historians, but social scientists who are less familiar with the original materials. Dr. Pryor notes that if I am correct in my argument "then much of what we have learned about English economic history must be given up", but he wonders "how typical were the cases he cites?", rightly pointing out that "social and land tenure arrangements differed almost from hamlet to hamlet". The second, Philip Corrigan, does not merely raise a query, but is more confident as to the answer. He describes the two parishes which are the central examples, Earls Colne in Essex, and Kirkby Lonsdale in Westmoreland, as "extremely non-typical". They are non-typical because neither is in the large, open-field area of central England, and because they are both "heavily dominated by particular landowners and specific production markets from an early point in their development". There is a useful point here. Although I cite work from all over England, it is true that my own detailed work only takes examples from one area of the Highland and one of the Lowland region. It would be good also to have done a study in equal depth of the open-field areas and not to have had to rely on the extensive studies by W.G.Hoskins and his pupils. Yet the point cannot be pushed too far. It is rather curious to have to argue that East Anglia and the whole of the Highland region, of which these two parishes do not seem untypical, are "exceptional". It is also interesting that none of the historians who have critically reviewed the book, some of whom have worked in open-field areas, have tried to argue that the sample villages are "extremely non-typical". Of course there were very important regional differences, but no-one has yet suggested, let alone demonstrated, that evidence from Essex, Norfolk or Cambridgeshire is flatly at variance with that from the midlands.

A number of attempts have been made to impugn my scholarship by suggesting that I lack judgment, that I am unscrupulous in my citation of authorities, that I have failed to do any original research, that I am unprofessional in my use of unpublished research. Although all these are potentially damaging charges, often meant to prevent a serious discussion of the central argument of the book, I believe that they can all be answered satisfactorily. Since the argument is often very specific and technical, I have decided to deal with these attacks on my historical scholarship in an appendix for those who are interested to see some of the methods one's colleagues employ in defence of an historical paradigm. Readers can then decide for themselves whether scholarly standards were maintained or betrayed by either myself or certain reviewers.

In concluding this section, I do not feel that the criticisms on the historical and documentary front have seriously damaged the central arguments of the book. I have indicated that in a number of ways it could have been better done, more clearly expressed, and more fully documented. Yet, as a foundation on which to build the essays in this book and to undertake further work, it has proved a durable enough structure. The strongest charge that can be made is that I over-simplified the position of certain medievalists, but I hope and believe that in doing so I have managed to clarify their views somewhat, and revealed an ambivalence which not all of them were previously aware of.

The use of comparative models

We can now turn to the other central question of methodology, the one which drew by far the most criticism and which raises some interesting historiographical problems, the use of comparative models in general, and my use of a specific model of peasantry in particular. What I tried to do was as follows. As an anthropologist looking at English history, I thought it would be useful to bring out some of the essential features of English history over the centuries by using a comparative approach. I decided that it would both be most honest and most helpful if I made the comparison explicit, rather than implicit. The comparison I would make would be between English characteristics as revealed in the records and a model of the central characteristics of "peasant" society, along the lines of the analysis in chapter one above. I pointed out that it "is clear that attempting to compress the major characteristics of a society into part of a chapter, when whole books have been devoted to the subject, will not only mean leaving out a great deal, particularly concerning the religious and ideological level, but will lead to the creation of a very simplified 'ideal-type' model in Weber's sense. It is likely that no particular society will fit exactly, at any time, all the features to be enumerated."(note 1) At the end of the detailed analysis of the features of peasant society, I repeated this statement. "It should again be stressed that the description above is a model, a simplified abstraction from reality. As a result it would be absurd to expect any particular society to fit all the features exactly; nor would we expect any specific feature to be entirely 'pure'. There are always some who marry late, there is almost always some marketing, some cash, some wage labour, some geographical mobility." Nevertheless, I still believed that "it is useful to have a strong model of the basic socio-economic nature of peasantry with which to confront a particular historical reality."(note 2) When model and historical data are compared, we need not abandon the whole model if some of the features are absent. But if almost all of the characteristics are missing, then it becomes difficult to go on labelling the historical society as "peasant".

The necessity for historians to create explicit ideal-types of this kind is well known, but it is worth repeating Weber's advice on this. " Hundreds of words in the historian's vocabulary are ambiguous constructs created to meet the unconsciously felt need for adequate expression and the meaning of which is only concretely felt but not clearly thought out." Such words would include Christianity, capitalism, peasantry, individualism, patriarchalism and so on. If we do not provide explicit definitions of these terms, we will be imprisoned by our unanalyzed implicit assumptions. "If the historian...rejects an attempt to construct such ideal types as a 'theoretical construction,' i.e., as useless or dispensable for his concrete heuristic purposes, the inevitable consequence is either that he consciously or unconsciously uses other similar concepts, without formulating them verbally and elaborating them logically or that he remains stuck in the realm of the vaguely 'felt'."(note 2) It was partly for this reason that I attempted to define 'peasantry' so carefully.

The nature of an 'ideal type' in Weber's sense and in mine can also be formally stated. "An ideal type is formed by the one-sided **accentuation** of one or more points of view and by the synthesis of a great man diffuse, discrete, more or less present and occasionally absent **concrete individual** phenomena, which are arranged according to those one-sidedly emphasized viewpoints into a unified **analytical** construct(**Gedankenbild**).In its conceptual purity, this mental construct (**Gedankenbild**) cannot be found empirically anywhere in reality. It is a **utopia**. Historical research faces the task of determining in each individual case, the extent to which this ideal-typical construct approximates to or diverges from reality, to what extent for example, the economic structure of a certain city is to be classified as a 'city-economy'."(note 4) Such was the ideal type that I was trying to construct as an heuristic device in relation to understanding England.

In specifying how one should construct such ideal-types Weber pointed to various difficulties. The principal one was in the confusions that occur when using real materials to give substance and clothing to models. Since this is an area where I have come under particular attack and may indeed have failed to escape entirely from the difficulty it is worth elaborating more fully what Weber meant. A comparison of the ideal type and the "facts" is a procedure which "gives rise to no methodological doubts so long as we clearly keep in mind that ideal-typical developmental **constructs** and **history** are to be sharply distinguished from each other..." But this is not easy. "The maintenance of this distinction in all its rigour often becomes uncommonly difficult in practice due to a certain circumstance. In the interest of the concrete demonstration of an ideal type or of an ideal-typical developmental sequence, one seeks to **make it clear** by the use of concrete illustrative material drawn from empirical-historical reality. The danger of this procedure which in itself is entirely legitimate lies in the fact that historical knowledge here appears as a **servant** of theory instead of the opposite role. It is a great temptation for the theorist to regard this relationship either as the normal one or, far worse, to mix theory with history and indeed to confuse them with each other."(note 5). The example Weber cites of such a confusion is in Marxism, where ideal-types of Marxian theory are sometimes used to manipulate evidence.

With such an ideal-type in mind I constructed a model of peasantry. I first pointed out that "peasant" has two meanings, one common-sense and another precise and technical, as described in chapter one above. Often historians use the word in the common-sense way, and we cannot object to this. What I was interested in was to alert readers to the fact that certain analysts were either consciously, or unconsciously, assuming that the English had once been peasants in the stronger, more technical, sense.

I then surveyed a good deal of the general literature on peasantry. The classic analyses and surveys of authorities like Redfield, Wolf, Thorner, Nash, Sahlins, Shanin, Galeski and others were used to bring out some of the essential features of peasantry. I explained that the model I was creating was based on these general accounts and on reading I had undertaken on peasantries in the Mediterranean (eight studies were cited), Asia (twelve books were cited) and northern Europe (three studies were cited). At this point I could have stopped in the creation of my model. Indeed, in my first formulations of the argument, for instance in chapter one above, I did stop here and the analysis was not slanted towards any specific peasantry. The general argument would not have been altered by doing so and, in view of the inordinate amount of criticism that has been levelled at my next step, it might have been wise to do so.

As it was, it seemed to me that I should clothe the model in some specific detail, rather than leaving it very bare and abstract. In order to bring out some of the consequences of being a "peasant" society, I felt it would be helpful to see how such societies worked in one particular area of the world, rather than leaving a broad general description of the lowest common denominator of peasantry throughout the world. This was not done in order to characterize peasantry. That had already been done. It was to give readers, most of whom would not have a strong feeling of what a "real" peasant society was like, a solid alternative picture to place alongside the English evidence. To create such alternative possible worlds by the use of comparative models seems a useful procedure on occasions, without it one is trapped into believing that what did happen is all that could have happened.

I decided to take Eastern Europe as the illustrative area for comparative purposes. I gave four reasons for this. Firstly, the earliest classic studies of peasant society and economy had been undertaken there in the l920's onwards. Secondly, some of the very best studies had been undertaken in this region, models of their kind. Thirdly, I felt that in order to provide a sufficiently strong sense of 'otherness' it would be helpful to move outside western Europe. I did not want to go too far away as it would be easy to ridicule attempts to compare England with India or China, for instance. Finally, it seemed useful to look at this area because it began to emerge that implicit analogies with East European peasantries had already strongly influenced many of those who had written on medieval England, namely Kosminsky, Vinogradoff, Homans, Postan, Titow and others. By making the comparisons more explicit, it would be possible to see how far such analogies were helpful.

I then tried to draw out the central features of East European peasantry based on the works of Galeski, of Thomas and Znaniecki, on Poland, of Shanin on Russia, and others such as Czap and Hammel. During this discussion, I also made constant comparisons with peasantries in other parts of the world, India, China, Mexico, Turkey and so on, in the work of Nash, Redfield, Wolf, Hajnal, Marriott, Stirling and others. From this it seemed to me that although Eastern Europe, as described by the authorities I relied on, was an extreme case, there were many features which could be found in other agrarian civilizations which comparative analysts termed "peasant". At the end of the discussion, I briefly compared this to western Europe and pointed out that the peasantries of western Europe were probably very different from what I called this "classical" peasantry. In terms of the "presence of cash and markets, land sales, rural specialization, age at marriage and all the other features, ethnographic accounts suggest that west European peasantries had moved a long way from the 'classical' peasantry described above."(note 6) I showed that this was true also in relation to the concept of ownership and its overlap with the family.

I then proceeded to look to see to what extent sociologists, anthropologists and historians had believed that England had once been "peasant" in the technical and "classical" sense. In a chapter on this subject, I showed that there was a widely held view that England had moved from a situation approaching a classical peasantry, to an individualistic and capitalist society, through a great transformation or revolution which occurred in the sixteenth to eighteenth centuries. Often the authorities were using the word "peasant" in the common-sense way, as in the title of a book by Dr.Thirsk on 'English Peasant Farming'. But often they really believed that there had been a different socio-economic formation before the sixteenth century along the lines suggested by Marx and Weber. The major rupture was supposed to have occurred in the sixteenth or seventeenth centuries.

Roughly up to the sixteenth century there were "peasants" throughout Europe. After that they disappeared in England, while elsewhere in Europe they lingered on in a muted form, disappearing in France, for instance only in the second half of the nineteenth and early twentieth centuries (note 7). The rest of the book set out to test this general theory, using the model I had created as a benchmark, a set of indices, against which to judge the historical evidence starting in the seventeenth century and gradually working back to the thirteenth.

It may sound strange, but when I began research on this, having set up a preliminary model and being curious as to when the peasants had "disappeared", I genuinely believed that I would find real peasants up to about the fifteenth century. I believed this on the basis of reading accounts of the great transformation by certain medieval and early modern historians. But when I looked at their accounts more closely, and incorporated my own work on documents with the results of recent research, particularly by Dr.Richard Smith, it began to dawn on me that this was a huge myth. The explosion in my mind, surprise and amazement that the data did not fit the predictions of a model which I had unconsciously accepted over the years, is what is captured in the book. The method may be rather a curious one, but in my own case, and for a number of others, it has forced a re-thinking of stereotypes. Since the method led to the conclusion that one of the central orthodoxies of modern historical and sociological thought was wrong, and hence directly challenged the work of Marx, Weber and many recent historians, it not unnaturally attracted considerable critical attention. We may briefly look at some of the objections that have been brought.

One set of criticisms focused on the procedure, that is on the choice of models, what was included in them, how they were constructed. A first argument was that the model of peasantry was based on too few instances. Dr.Harris alleged that it was "constructed from three works on peasants in Poland and Russia", and Faith, Hilton, Dyer, Hyams and Herlihy, also stated that the model was basically Slavonic, or East European. As I hope to have shown in the summary above, this is a distortion. The model was based on several civilization and on many of the leading accounts of the basic nature of peasantry throughout the world. It is only a part of the model that is based on the concrete East European case. I do not believe that the model can be dismissed in this easy way. In fact, none of the reviewers, with the exception of Hirst and Tribe, challenged the wider work of Wolf, Sahlins, Dalton, Redfield, Nash, Thorner and the rest which is just as important to my argument as the books on Eastern Europe.

Having asserted that my model was based on only three cases, Harris goes on to ask whether any meaningful model can be made from so few cases, and whether the "abstracting an ideal type from a particular context (in this case Eastern Europe) and using it to create a general category of thought (i.e. the peasantry)" is a valid method. It is probably not valid, but it is equally not what I did, as chapter one above shows. Rather curiously, the same author then suggests that my comparison is undermined because while my material on classical peasantry is "constructed from three works of the most general and synthetic sort", my English material is based on specific studies of particular villages, manors and so on, and hence I am not comparing like with like. In answer to this, we might note that this does not seem to be a correct characterization of the work by Thomas and Znaniecki. More importantly, however, my model, as I make clear, is also based on several dozen field monographs, anthropological studies of particular villages and communities.

Harris' next criticism is one which is also made by Faith, Herlihy and Rowland. It is basically that rather than using Eastern Europe as my central concrete comparison, I should have used a model based on western Europe, perhaps located in France, Italy or elsewhere. This is a reasonable point. I have given my reasons for not doing so, but these critics, and Dr. Baker, may have a point that by taking a very pure concrete case, I have pushed the argument to its extremes. It would have been safer to have chosen somewhere nearer to England, and this would indeed have brought out some of the subtler similarities and differences within western Europe. But I am not sure that as a first effort it would have revealed as much. Like Jack Goody and John Hajnal (note 8), I find it refreshing to look from outside western Europe in order to see some of the peculiarities. The work of making the finer discriminations within Europe I saw as a second stage. It was begun in the last two chapters of the book where I compared England to other parts of Europe, using some of the insights gained from this wider perspective. Yet there is no doubt that Robert Rowland has a strong argument when he points out that I need to show why England, France, Italy all "differed in **different ways** from the ideal type."

Philip Corrigan seemed to find my use of models confusing in two ways. Firstly, he finds that taking definitions of peasantry "developed since l945 to describe peasants defined within a capitalist world market" and applying such definitions to 13th to 17th England, not finding such peasants there, and then arguing that there was no peasantry at all, an odd procedure. I would agree that there are dangers in using modern categories and foisting them on the past. In some ways this was my very point. What I was arguing was that this was exactly what some historians were implicitly doing. They were taking unexamined assumptions of what "peasants" are like from their experience of modern societies, and then making medieval man look like them. Perhaps it would clear up this misunderstanding if I stated that basically I am not interested in whether medieval or modern English people were or were not "peasants". My interest was in whether the numerous features that nowadays we associate with peasantry, that is the absence of individual rights in property, constraints on the extensive penetration of money and markets down to the village level, low geographical mobility, a symbolic attachment to the land, a certain type of household formation, and other features, were present in England in past centuries or not. I do not really see what the capitalist world market or l945 is really relevant to these ultimately empirical questions.

A second criticism by the same reviewer is that in some places, apparently, I see that I am checking one 'representation' (or model) against another, in other places I suggest that I am "comparing two comparable realities". No specific instances are cited, and without these I find it difficult to answer this criticism. In principle and generally, both seem to be valid procedures. At times I would like to compare observer's models of classical peasantries against historian's models of medieval England. At other times, I would like to compare what I consider to be the "reality" of peasant societies in the present against the "models" of certain historians. At other times it is present "reality" against past "reality", or past "reality" against past "model". As long as I am clear in the book as to which of these things I am doing, it seems to me legitimate to do them all, always acknowledging that the relationships between "reality" and "representation" is a much more complex one than is implied in the brief preceding remarks. It may be that I was not always sufficiently clear about what I was doing, however, since one perceptive critic, Robert Rowland, suggests that I "appear to confuse the ideal type with a simplified empirical model of a (any) peasant society".

Dr. Anton Blok, an anthropologist, has some other methodological criticisms. He sees my model as "at once ideal-type, stereotype, and description of 'real' peasantries". He dos not object to this in itself, but finds that the model I have created is too simple. He does not like the way it lumps together the peasants of Russia, India and the Mediterranean, ironing out very important differences in the process. In general, this is a fair criticism. As I acknowledge, the model is very simple and reality is very complicated. I had to rely on generalizations made by the leading experts, against whom, presumably, the same criticisms would be levelled when they wrote books or articles on "peasants" at a global level.

Dr Blok then goes on to pursue a particular argument which I find less helpful. He states that I prefer "to sacrifice cultural diversity on the altar of sterile nominalism", and that I apparently feel "uneasy about concepts which do not have sharp boundaries". These are harsh words, and I can only leave readers to judge whether they are true. I would say, however, that to believe that for heuristic and analytic purposes it is better to have a clear, unambiguous and simple, if over-simplified model, than to have a more realistic, but probably highly qualified and complex model, does not necessarily mean that I feel "uneasy" in general about concepts without sharp boundaries. I just happen, on this occasion, to have found them less useful.

My "misunderstanding", Blok writes, is "closely connected with the belief that the only aim of the comparative method is to look for similarities". Unless I have misunderstood Blok's meaning, this is rubbish. While it may be true that I ironed out certain differences as between peasantries elsewhere, the two central themes of my book are precisely concentrated on differences and not similarities. My use of the comparative method was precisely to show the differences between medieval England and classical peasantries, and to show the differences between England and much of the Continent. Indeed, it has been for stressing these differences that I have been roundly condemned. It is not helpful to be admonished, particularly with an allusion to Marc Bloch, for only being interested in similarities. Bloch, Blok and I are in agreement that the purpose of the comparative method, of which this book was meant to be a prime example, is to look for both similarities and differences.

Another criticism is that the model is incomplete in certain ways. Dr.Faith argues that I have created a model of "a peasantry" rather than of a "total society in which the peasantry is a subordinate class". The evidence for this charge is that "the landlord hardly makes an appearance in this book". In England, in Poland, Russia and so on, peasants were not autonomous but were "subordinate to a ruling class". Apart from the anachronistic and loaded use of the phrase "ruling class", Dr.Faith has a point here. Although my model did in fact incorporate quite strongly the relations between lord and peasant, this was an area to which, as I acknowledged earlier, I should have given more attention when dealing with medieval England. It does not undermine either model or treatment, but it is one of the threads which would have strengthened the argument still further.

Dr. Faith's second criticism is that the model "does not contribute very much to our understanding of peasant production as an economic system". This is not surprising, she says, because it is "primarily the creation of anthropologists and sociologists, whose interests lay elsewhere". Dr.Faith is not convinced that I am very familiar with what peasants actually do. By this she appears to mean ox-team cultivation, open-field agriculture, and other features of agricultural techniques and organization. Now it is true that I do not devote much of the book to these matters, but it is a slur on people like Wolf, Thorner, Sahlins, Nash and others to say that they ignored such matters because their "interests lay elsewhere". It is also the case that having lived for fifteen months in what might be described as a "peasant" village and written a book which analyses the production methods of this society in extreme detail, I do have some familiarity with these subjects.(note 9) It is a field which I ought, of course, to know more about, but I have read many of the classic accounts of agricultural production methods. I did not deal with these topics in more detail in the brief constraints of the book because they did not seem central to my particular argument. I do not happen to believe that open-field agriculture, ploughs, the physical aspects of production throw much light on whether people are, or are not, peasants. Some may disagree, and argue for some kind of technological determinism where the means of production inevitably lead to certain relations of production. It would be interesting to hear the arguments. My knowledge of agricultural societies throughout the world does not support such a crude association and I do not feel that this is a large gap in the model.

Further evidence that I am not very familiar with what peasants do according to Dr. Faith is the fact that "in a rather odd footnote" I write that I decided "not to include any discussion of peasant production or of Chayanov's contribution to our knowledge of it(p.15, note 31)." This seems damning. I seem to have admitted that I will include no discussion of peasant production. In fact, if one turns to the note, there is no mention in it of peasant production at all. What I do say is that a number of Russian scholars have contributed very significantly to the "general discussion of the domestic economy of the peasantry", a different subject, and I have decided not to go into their work in detail.

The footnote arose from discussions with Professor Shanin, an expert in this field. I had written a certain amount about Chayanov and others in an earlier draft, but he pointed out that this was a very large and complicated field of debate which I should either enter more fully, or not at all. I therefore decided to use Chayanov's important work indirectly, through his well-known and extensive influence on almost all of my main authorities on peasants and the domestic mode of production, including Shanin himself, Sahlins and Wolf. I thought that by writing that "I have decided to omit any direct reference to their work", joined to the fact that I presumed that most people knew that I was heavily drawing on Chayanov's insights through the wider literature, this would be sufficient. But this footnote has given several reviewers, as well as Faith, the chance to strike a wider blow.

Paul Hirst has raised some interesting theoretical points, claiming that ultimately I cannot avoid the Populist versus Marxist arguments in this way. At the widest level he is probably right and I will return to his arguments. But in the context of the book, I still feel that to have spent an extra ten pages or so at the start of the work going over these very specialist debates would have alienated many readers. Professor Herlihy is less helpful. He claims that Macfarlane "dismisses the work of the foremost theorist of a peasant economy, the Russian scholar A.V.Chaianov, for fear of 'complicating the argument'". As a stark accusation, this sounds bad. I certainly do not "dismiss" Chayanov's work; my book is very heavily indebted to it and I respect it greatly. It is merely that it seemed possible to obtain his major insights without getting bogged down in a detailed treatment of those Russian writers of whom he is only one.

Two final criticisms of the general methodology are particularly interesting. Barbara Donagan suggests that the "careful sociological-anthropological definition of peasantry" is "in a sense a red herring". She admits that "it allows demonstration of the weakness of medievalists' arguments by analogy to other peasant societies when their English evidence fails" and, since this was part of my purpose, has some justification. It is a red herring because my belief in a classical peasantry has a "theological element", suggesting that I am a "believer" in something that is merely a matter of faith. Since matters of faith are disputable and unprovable, presumably Dr.Donagan feels they should be kept out of the argument.

After the sometimes rancorous responses to this use of the peasant model, I would like to feel that the argument could indeed have been made without any reference to a comparative model of peasantry. Unfortunately, I am not sure how, in practice, I could have done so. If I had listed a set of criteria which I asserted were interlinked, and then merely shown how they were not present in medieval or early modern England, it is likely that many would have replied that they did not exist anywhere, rightly asking me to make explicit and document my comparative model. Furthermore, I am certain that without making it explicit to myself, I would not have been in a position to question much of the growing orthodoxy of the supposed capitalist revolution. I suppose I could have used an explicit model for my own purposes, and then disguised it. This might have been a safer procedure, but I believe it would have been a less honest one. Honesty, an attempt to share one's procedures of thought, may not charm all the critics, but I still believe that historians and others should make their comparative models explicit.

These appear to be the main methodological criticisms of the nature of my comparative model building. Some, we have seen, are possibly right, others absolutely right, others are exaggerations, misrepresentations or based on misunderstanding. In sum, they do not convince me that my procedure was basically flawed in either conception or execution, though of course it could have been done better. I leave others to judge whether I have answered the criticisms.

There is no doubt that I could have improved the contents of the model. Here I come to a second class of criticism, that basically the ingredients of the comparative model are wrong. There are two main criticisms. The first is that peasants are much more diverse, less "pure" and uniform than my model allows. I had already tried to anticipate this, as my quotations above show. If one considers a model as a kind of ruler or bench-mark, then on all the different indices specific societies will be at different positions. I particularly stressed that this was the case in western Europe. Thus it is useful to be reminded by Dr.Blok that peasants in southern Italy are not always "familistic", that they are often market-oriented, selling and buying land, and sometimes geographically mobile, even in non-European peasantries. This is, of course, true. But I do not see that it invalidates the discussion; it merely adds nuances to that very difficult problem of creating a lifelike model.

The severest criticism, however, was reserved for my treatment of East European peasantries. We have seen that certain reviewers convinced themselves that my whole model of peasantry was based on East Europe, and that my whole account of Eastern Europe was based on three synthetic accounts. The next stage was to show that these three accounts were inaccurate. I was pushed out onto the gang plank, step by step and then given the final push to the sharks. I have refused to accept the first two steps in the argument; the book, as chapter one above shows, could easily have been written without mention of Eastern Europe. But supposing I was now at the end of the plank and the question was put, "has he based his whole model on faulty reporting", what would be the answer?

The first thing to say is that there has been little direct challenge to the authority of Galeski on Poland, of Thomas and Znaniecki's massive work, or of Shanin on Russia. The approach is usually more indirect. Dr. Faith argues that there is doubt as to whether the model is accurate; Kula's work on Polish feudalism "would certainly cast doubt on this rigid conception of a 'natural' economy by showing that both seigneurial and peasant economies were alike both monetized **and** 'natural', and that from very early on peasants in Poland were anxious for access to the market. Likewise, Chayanov's work would not support the view that there was no land-market and a virtual absence of wage-labour. I expect Dr. Faith is right. As I wrote, in practice no situation is pure, indeed peasant society is bound to have some of these elements since the very definition of peasantry makes it a "part-society", a part in relation to a greater whole which includes a State, a market, towns and so on. If there were none of these, it would not be peasant, but something else.

The same point is made in another form by Professor Hilton. On the cited evidence of R.E.F.Smith's work, we are told that medieval Russian peasants were much more mobile, lived in nuclear families, did not have a sentimental attachment to a particular piece of land, bought and sold land and so on. Quite so. I was not trying to paint a picture of a fossilized East European "peasantry" that had been unchanging since time began. It does not discomfort me to hear that medieval Russian peasants also do not fit the stereotypes of certain English medievalists. But I do not see that this has a great bearing on the model. Are Galeski, Shanin, Thomas, Znaniecki wrong about the time and groups they were describing? If so, this should be shown, rather than implied by innuendos such as "Macfarlane has been badly let down by his advisers".

Two reviewers constructively discuss this matter. Keith Tribe alleges that Shanin's **Awkward Class** "relies on a combination of development sociology and Slavophilism to present an image of the Russian peasantry as a cohesive and natural order". Tribe then proceeds to make several other criticisms of Shanin's work. I am not competent to go far in this debate, but the most that is alleged is that Shanin, one of my three major authorities on Eastern peasantry, has made the Russian peasantry of the early twentieth century more communal-minded, more stable and family-based than they really were. If this criticism is correct, then my model needs some adjustment. Shanin's peasants may not have been as close to one end of the bench as I had thought.

Paul Hirst makes some interesting comments on this problem. He rightly points out that the theoretical literature I draw on is "strongly influenced by the work of...Chayanov, notably Shanin, Thorner and Sahlins". He then reminds us that "this model of peasant economy is by no means uncontroversial". He writes that I cannot avoid this controversy, that is between the Populists and Marxists. Hirst claims that my model of peasantry is so extreme that it may not fit the Russian case, in other words Shanin is possibly wrong. What is his support for this claim? "It is arguable that Russia (certainly after 1861)" was characterised "by individualistic and highly monetized agricultural producers, family members having few rights in relation to the head, the communal or clan regulation of landholding playing a marginal role, money lending, sale and rent of land, wage labour, etc. playing by contrast a prominent role." He then proceeds with further detailed points which suggest that, if he is right, my picture of Russia is too simple.

If it turns out that the model I have created of East European peasantry is too extreme, or even that it is a model in the minds of Shanin, Galeski and the rest, and not a very accurate representation of even the period they were studying, this would not be a crippling blow to my book. As we have seen in the discussion of Weberian models, this is precisely what models are for. Indeed, it would be somewhat refreshing if it were the case that while I had thought my work would have its main implications for English history, in a boomerang way it made people question peasant stereotypes for Eastern Europe. Naturally it would be more convenient if the debate were resolved in a way that showed that model and reality in Eastern Europe matched in the way that I had assumed. But my main interest was in the use of a model, a thought-experiment, to expose three things. Firstly, what goes on in the minds of historians and sociologists when they try to study the development of English history. Secondly, some interesting questions we could ask of the historical evidence. Thirdly, as a kind of predictive system.

When treating historical materials, and particularly when analysing periods like the thirteenth and fourteenth centuries when the documents are often scanty, it is necessary to have a hunch about what would be interesting questions, in other words to have a thesis to test. The creation of a simplified model of peasantry gave me an interlocking set of predictions about how the society might have been. I was not trying merely to show "at some length" that English villagers "seem always to have differed from the inhabitants of nineteenth-century Poland and Russia, or twentieth-century Turkey or Mexico" as John Hatcher facetiously put it. If this had been so, it would clearly be a ridiculous activity. What I was trying to show was that there is a strong, and recently heightened, tendency to lump the histories of all societies together. It is easy to slip into thinking that all societies progress through a uniform "peasant" stage into the "modern" world. It is tempting then to conclude that the only difference in the case of England was that it did so earlier than anywhere else (with the possible exceptions of Holland and Denmark). My use of an abstract model was to help myself, and hopefully others, in their escape from such a narrowing approach.

We may ask what is left of the arguments put forward in **Individualism**. Have they survived, or have they been destroyed? Here I have to leave the decision to others. Certainly it was recognized that I was sailing for large prizes. As Lawrence Stone wrote, "It is not often that a book appears which challenges the whole corpus of conventional wisdom about the evolution of the modern world". If I should happen to be right in my facts and deductions from them, then he agrees that I would be saying "something very important". Indeed, it is so important that if am roughly correct, then I could well be, Stone suggests "the Einstein of history".

Certainly others have now sailed along the same route and have found a land which was until very recently denied to exist. One flotilla is described in chapter six above, where it will be noticed that most of the books published up to 1978 argued strongly in one direction, while those published in the wake of **Individualism** took an entirely different view. A great deal of subsequent historical research, very ably summarized by Dr.Clark, has now been published which basically supports the thesis of continuity. (note 10) But whether the account I have given of the battles that have been fought, or the further evidence presented in this volume, will convince others that I have succeeded in destroying, in Stone's words "the grand theories about England propounded by all those foreigners, Marx, Weber, Durkheim, Tocqueville, Rostow, and the rest", is up to the reader. Stone argues that "this...is an unlikely consequence of an implausible hypothesis based on a far-fetched connection with one still unproven fact of limited general significance", and one could not be more damning than that. Others are more encouraging.

Personally I would accept that the argument is faulty in several respects and I would no doubt modify it a little if writing the book now. In particular, I would make more explicit some of the remarks which, in a naive way, I had thought would be treated charitably and sensibly, rather than being ripped out of context and held up to ridicule in order to discredit the book. Furthermore, in the light of the almost universal statement of the medievalist reviewers that they never believed in real peasants, family property and so on, I would not now present their findings in quite the same way. It appears that we agree more than I thought. Yet the ambivalence of the reactions, often fiercely trying, by some fairly piratical means, to sink the book, while loudly proclaiming that they were really friends and why had I ever thought otherwise, does suggest that there was something in what I wrote.

LIST OF MAJOR REVIEWS OF THE ORIGINS OF ENGLISH INDIVIDUALISM REFERRED TO

This list includes 39 of the 52 reviews known to me. All reviewers mentioned in the Postscript and the Appendix are included. A few short or very general reviews have been omitted.

Abrams. Philip Abrams, in Historical Sociology (1982), 3 22-6.

Blok. Anton Blok, 'In Search of the English Peasantry', Theoretische Geschiedenis (Sept. 1980).

Charlesworth. A. Charlesworth, in Environment and Planning.*A, 13 : 2 (1981), 254.

Corrigan. Philip Corrigan, in Sociological Review 28 (May 1980), 465-70.

Delbos. Genevieve Delbos, in L’Homme, xx : 3 (1980), 162-5.

Donagan. Barbara Donagan, in Ethics 91 (Oct. 1980), 168-70.

Dyer. Christopher Dyer, in Economic History Review 3 2 (Nov. 1979), 600-1.

Elton. Geoffrey Elton, in Times Literary Supplement 23 (Nov. 1979), 27.

Faith. Rosamond Faith, in Journal of Peasant Studies 7 (April 1980), 3 84-9.

Goldie. Mark Goldie, 'Old Whiggery, New Bottle: Macfarlane's English Individualism', Cambridge Review, CI : 2255, (1978) 111-4.

Harding. Alan Harding, in British Book News (March 1979), 250.

Harris. Barbara J. Harris, in Journal of Social History, 14 : 1, 1980, 169-172.

Hatcher. John Hatcher, in The Historical Journal, 22 : 3 (1979), 765-8.

Herlihy. David Herlihy, in Journal of Family History, 5, (Summer 1980),235-6.

Hilton. Rodney Hilton, 'Individualism and the English Peasantry', New Left Review, 120 (March-April 1980), 109-11.

Hirst. Paul Hirst, in Cambridge Anthropology, 5 : 1, (1979).

Homans. George C. Homans, in Contemporary Sociology: A Journal of Reviews, 9: 2 (March 1980), 262-3.

Hyams. Paul R. Hyams, in English Historical Review 96 July 1981), 605-7.

Keates. Jonathan Keates, 'In the Woods', New Statesman (5 Jan. 1979) 21.

Kurtz. Lester R. Kurtz, in American Journal of Sociology, 86 (1980) 430-7.

Laslett. Peter Laslett, 'Always Individualist', New Society, (14 Dec. 1978), 649-50.

Levine. David Levine, in Journal of Interdisciplinary History, XI : 4 (Spring 1981), 669-76.

Mount. Ferdinand Mount, 'Goodbye to the Peasants', Spectator, (17 Feb. 1979),4.

McCloskey. Donald N. McCloskey, in Journal of Political Economy, 89: 4 (August 1981), 839-40.

Pocock. J. G. A. Pocock, in History and Theory, XIX : I (1980), 100-5.

Pryor. Frederic L. Pryor, in Journal of Economic Literature, XVIII (March 1980), 133-5.

Raftis. J. A. Raftis, in Journal of European Economic History, 11 : 1 (1982), 242-4.

Rheubottom. D. B. Rheubottom, in Man, 15 : 3 (September 1980), 5 74-5.

Rogers. Alan Rogers, in The Local Historian (May 1980), 105-6.

Rowland. Robert Rowland, 'Robinson por computador, Alan Macfarlane e as origens do individualismo inglese, Ler Historia, 5 (1985), 83-104.

Ryan. Alan Ryan, 'Yeomen, not peasants', The Listener, (14 Dec. 1978), 790-1.

Smith (1). Richard Smith, 'Some Thoughts on "Hereditary" and "Proprietary" Rights in Land under Customary Law in Thirteenth and Early Fourteenth- Century England', Law and History Review, I (1983).

Smith (2). Richard Smith, ' "Modernization" and the Corporate Medieval Village Community in England: Some Sceptical Reflections', in A. R. H. Baker and D. Gregory (eds), Explorations in Historical Geography (1986).

Stone. Lawrence Stone, 'Goodbye to Nearly All That', New York Review of Books (19 April 1979), 40-1.

Timm. Lenora A. Timm, in American Anthropologist, 82: 3 (Sept. 1980), 679-80.

Todd. Emmanuel Todd, 'Hypothese revolutionnaire d'un Britannique', Le Monde (9 November 1979).

Tribe. Keith Tribe, in Social History, 4: 3 (October 1979), 520-2.

Trumbach. Randolph Trumbach, 'Kinship and Marriage in Early Modem France and England: Four Books', Annals of Scholarship, 2: 4 (1981), 113-28.

Waugh. Scott L. Waugh, in Journal of Economic History, 39 (Sept. 1979), 770-2.

Worsley. Peter Worsley, in Journal of Development Studies, 16 January 1980), 263-4.

Wrightson. Keith Wrightson, in History, 65 (February 1980), 87.