

[This is an unpublished piece, written in 1986 at the request of my publishers. It answers criticisms in relation to 'The Origins of English Individualism' (1978). The other part of my reply to my critics was published as the Postscript to *The Culture of Capitalism* (1987) and is on this web-site. This section was finally not published because it was thought to be too defensive and personal. I include it now for the historical record; many of those to whom I reply are retired or dead.]

## **Scholarly standards; judgment, accuracy and honesty.**

Some of those who reacted harshly to my book on **Individualism** made a number of charges of scholarly inadequacy which need to be answered. If such charges are in the main correct, it could be argued, as some have done, that the book is so "unsound" that it need not be taken seriously. This is a form of pre-emptive defence which makes discussion of more substantive, and perhaps uncomfortable subjects, appear unnecessary.. Reviewers have a responsibility as great as authors. Their criticisms deserve to be weighed carefully.

The damaging accusation has been made that I am not to be trusted as a historian. Several critics have suggested that my use of evidence is so selective, biased and unprofessional that my argument can be ignored. If a historian's use of sources is deeply flawed in the way suggested, his credibility is gone and his reputation may be damaged for good. So it is worth examining this charge. The main proponent of this view is Professor Rodney Hilton, perhaps the senior English mediaevalist now active. Hilton concludes that "if the book had been a serious historical investigation, it would have called for a lengthy reply". One of the reasons it is not serious is because of my mis-use of evidence. Let us examine the basis for this charge.

Professor Hilton starts by accusing me of being "entirely unreliable" in my estimates of wage labour in the middle ages, which is "characteristic" of my "unscrupulous selection of evidence". Unscrupulous is a strong word and this is a grave charge. What is the evidence to support it? Only one matter is brought up, the treatment of Hilton's own work on wage labour. I am charged with making misleading use of the fact that "between 50 to 70% of males in East Anglian villages were employees designated as servants and labourers". What is the offence? Not that I am telling a lie, not that I have misquoted, not that I have concealed my sources, but rather that I should have stated that this was "mainly in centres of textile production" as my sources make clear.

It would be possible to answer that I had assumed that many would know that East Anglia in the middle ages was famed as a centre of cloth production, or that it makes no difference to the argument if it was, or was not, a cloth area. But there are also other answers. The charge implies, even though the use of "mainly" admits some doubt, that wage labour was exceptionally confined to cloth areas in East Anglia. In the context of Hilton's review, it also implies that it is an exceptional piece of evidence and that if I had only given the background, it could largely have been discounted. It is perhaps true that if I had only used this one figure to base my whole analysis of wage labour, then I should indeed have made the background clearer. What Hilton does not mention in the review is that I also cited evidence from the work of Kosminsky, other evidence from Gloucestershire, and some of Postan's work on the very same topic. Furthermore, I cite studies by Ritchie, Raftis and De Windt also bearing on the point I was trying to make, namely that there was a large amount of wage labour in medieval England. Was I in each case to explain the sort of economy that seems to have produced very high levels of wage labour in many parts of thirteenth and fourteenth century England? Nor do I see that Hilton's next point, the fact that "proportions of hired labour varied considerably from village to village as well as from household to household" and that "many peasant households had no servants, very few had more than one", makes my evidence either unreliable or unscrupulous. In the end, however, there is no real ground for dispute since Hilton believes that medievalists can absorb widespread and documented wage labour easily enough into their models.

Professor Hilton then goes on to suggest that the whole book can be dismissed as unprofessional,

that "the quotations from principal sources do not give confidence in the author's judgments". To lack "judgment" is a very serious failing for an historian. What is the basis of this grave charge? The first example is rather curious; "Marc Bloch is quoted both to support the thesis of English individualism and as one of the deluded believers in the primacy of the group in medieval social organization". In general, it does not seem a totally odd thing to believe that a historian, even as great as Bloch, can have written things with which one agrees, and others which appear to be misguided. It is rather a curious view of history which believes that people must be always right or always wrong. In this specific case, in a number of places, and particularly towards the end of the book, I approvingly quote Bloch who recognized that there were basic differences between the development of England and France in relation to concepts of property. Bloch was comparing these two countries and I felt that he was one of the most perceptive observers. On one occasion, however, I quote him as saying that "early societies were made up of groups rather than individuals. A man on his own accounted for very little"(Individualism,52). I pointed out that his much more general remark falls into line with certain evolutionary thinkers such as Marx. While Bloch's view may have a general truth, I believed that it is misleading in relation to the period of English history covered in my book. In fact, it is likely that Bloch would have agreed, and it is probably true that I should not have brought him into the argument here. But this is not the accusation made by Hilton, which is altogether simpler, if not naive.

The second ground for doubting my judgment is that I have mis-used Weber. This is because "Weber is mainly cited at second hand from a biography or from the small selection from his writings entitled **Theory of Social and Economic Organization**". This accusation implies two things. Firstly, that I have somehow misrepresented, misunderstood, or misled readers in some way because I have relied on "second hand" and partial sources. Secondly, that I lack the scholarly application to read Weber's own writings in a serious way. Since one of the central conclusions of the book is that Weber misdated, indeed partially invented, the transition from peasant to individualist, from feudal to capitalist, in England, this charge is a serious one. I was myself taken aback when accused in this way; my use of Weber sounded so shallow and unreliable. Then I checked the book. A few points need to be made in reply.

Firstly, "from a biography", makes the work quoted sound a lightweight one. Only those who have read the 522 pages of the "intellectual portrait" of Max Weber by Reinhard Bendix will realize that this is a totally misleading description of one of the greatest of analytic portraits of a major thinker. From this I quote fourteen times. The "small selection" entitled "Theory...", also manages to make that work seem lightweight. In fact this is a book which, in my edition, is 436 pages long and is probably rightly described on the cover as "the most extensive general exposition of Max Weber's sociological theory and its applications to the broad empirical problems of historical structure and change". It is only relatively "small" because Weber wrote so much. In fact, though, I only use this work on five cited occasions. The book of Weber's which I cite most often, some 32 times, is his **General Economic History**. Another book, cited as often as Bendix's intellectual portrait is **The Protestant Ethic and the Spirit of Capitalism**. Neither of these works is mentioned by Hilton.

As for misrepresenting Weber, it is curious that none of the other reviewers, a number of them experts on Weber and sociological theory, have disputed or questioned my use of Weber, or suggested that it is based on too small or secondary a base of reading. The only specific misrepresentation Hilton alleges is that in a quotation which he says is from the **Theory...** (but which, as the footnote on p.51,no.135 shows is from **The General Economic History**). I have cited Weber's description of women in 'ancient' England, as being his view of women's position in medieval England. Hilton may be right, though his belief that Weber's views are "probably based on the Anglo-Saxon law codes" is just a guess. If he does turn out to be right, I gladly concede the point. It would hardly, after all, constitute a major error of judgment.

Professor Hilton then goes on to accuse me of four misuses of medieval historian's work. The first, citing what I agree with but not what I disagree with from Dr. Razi's unpublished thesis I explain below. The second accusation is as follows. "C.C.Dyer is cited as being unable to find more than one example

of long lasting peasant families, whereas in fact the tables in the quoted article provide many such examples." As I made clear in my book (p.98), what I was looking for was evidence that a family stayed for three generations or so on one landholding. I was therefore dismayed to read that there are "many such examples" in the tables. Here is a very specific issue concerning the interpretation of medieval documents where the outsider, trespassing outside his "period" appears to have been unable to see clearly. Since there is only one such table in the published version of the article (ed.Smith,284), which corresponds closely to the unpublished version, it is obviously here that the "many examples" have been found by Professor Hilton. Turning to table 6:1 of the article by Dyer, what is revealed?

The table gives columns concerned with the number of land transactions, returns to the lord, taking land from the lord, **inter-vivos** transfers (non-family), and finally "Transactions within family", as titled in the unpublished version of the article I used. Having constructed similar tables for fifteenth and sixteenth century manors in Essex and Westmorland, it is clear to me that such transactions can be of numerous kind. They may be from husband to wife, wife to herself and husband, from brother to brother and so on. They can also be of very varying amounts, from the passing of a small part of an estate, to the whole of an estate. They can be for a few years, or for several lives. They can indicate the transfer from a father to a daughter who immediately afterwards sells the property, and so on. I would judge that it is impossible to use the very crude aggregate figures in the table to deduce anything about whether there are "long lasting peasant families" in a manor, one way or the other. It is not even certain that the families lived in the manor; some of them were probably outsiders holding property there. I find it disturbing that Professor Hilton can find evidence for such a proposition from such figures.

The next criticism concerns my use of Professor Hilton's own work in relation to the composition of the family. Since this is a direct confrontation, all I can ask is that readers who feel I have misrepresented Professor Hilton by suggesting that he believes in multi-generational peasant families read my account of his views (p.136) and check them against the cited texts. It is worth noting that of the three quotations from three separate works by Hilton, two of them very directly on this point, and one more indirect, Hilton only challenges the one which I myself pointed out is the least direct.

Hilton's last evidence that my judgment cannot be trusted is that I have mistakenly stated that three quarters of lord's income on Suffolk manors was derived from fines paid by tenants on transfers of land. What I should have written was that three-quarters of the income from the manorial court perquisites was from this source. To this I plead guilty. I should not have let that misleading statement pass, being obviously aware from my work on the way in which the Earls of Oxford and the Harlakendens ran their manors from the fifteenth century onwards that profits of court are only one source of income.

Having surveyed all the evidence brought forward by Professor Hilton for his serious accusations, I leave it to the reader to decide whose judgment is called into question by the "quotations from principal sources".

Another medievalist who, in a milder way, accuses me of misrepresentation of printed work is Professor J.A.Raftis. He complains that I have "misrepresented" his work, which he claims actually supports my argument, rather than being one of those who mistakenly believes in a medieval family-based economy. I am sorry if I did misrepresent Professor Raftis, though I believe that readers who compare what I wrote with his books and with his attempt to make such an allegation may find it hard to see what the misrepresentation was. But I am delighted to hear that he agrees with my interpretation, which he claims is what he was trying to say all the time. I am also glad to hear that Postan and Hilton, according to Raftis, also basically laid the groundwork for my approach. I am delighted to hear that we are all indisputably on the same side, though by citing their work in my support on numerous occasions I had tried to show that their evidence was indeed important. What I find more difficult to understand is why, if we all agree so fully, there should have been such a furious reaction from a number of historians.

The bitterness of the reaction is well illustrated in a third accusation of serious misrepresentation, this

time by another medievalist, Dr. Rosamond Faith. In making such an accusation, she refers back to an earlier criticism in her review in which she writes that "It is easy to demolish views that are misrepresented", at the end of a paragraph about family ownership. It is indeed, so what are the "misrepresentations"? While Hilton accused me implicitly of misrepresenting Weber, Dr. Faith explicitly criticizes me for distorting Marx. She cannot find "any evidence that Marx regarded the household in this first society (i.e. 'Germanic') as necessarily the unit of **ownership**". She writes that she cannot find any evidence for Marx holding such a view in my quotations from Marx, which she cites as on pp.32-45 of my book. In fact, I do not start quoting Marx until page 37, and the key quotations are on the bottom of pages 39 and on page 40. Since it would be tedious to re-quote all these passages again to support my interpretation, I will quote only one. Mine does not seem an entirely bizarre interpretation of the following single quotation from Marx, namely that "in the Germanic world, the totality is the individual residence, which itself appears as only a small dot on the land belonging to it, and which is not a concentration of many proprietors, but the family as independent unit".(Grundrisse,484). How else would one interpret "proprietor", "family as independent unit", "land belonging to it", and so on, other than by assuming that Marx was talking about the family as the unit of ownership?

Dr. Faith then alleges that I have misrepresented Tawney by ascribing to him a view of family ownership, whereas the quotation I have given only speaks "of a household economy"(Individualism,54). In fact, what I wrote was that Tawney "believed that society was originally based on the household as the basic unit of production and consumption". I supported this assertion by giving a long quotation from Tawney. I wrote nothing about Tawney's views on family ownership at all.

I am then accused of misrepresenting Postan, whom I apparently cite as believing in family ownership but who, according to Faith, speaks of such "family holdings only in the sense of holdings that supported a family". In fact, in the place cited, I do not go beyond giving a long quotation and then state that this implies that Postan believed, as he indeed says, that medieval England was a 'peasant' society. I do not say, or imply, anything about his use of the phrase "most villagers possessed family holdings of modest size". Though one might interpret this to mean family ownership, I refrained from such an interpretation because it was obvious that the word "possess" is open to various readings.

Finally, I am charged with having misrepresented Hilton and Christopher Hill in the same way, on pages 55-6, apparently suggesting that they too believe in 'family holdings'. Readers who check these two pages will see that I say nothing whatsoever about family ownership in relation to these two authors. Dr. Faith is indeed right when she says that it is "easy to demolish views that are misrepresented".

Another kind of criticism is of a rather different kind, lying on the borderland between unscholarly and unethical behaviour. A serious charge of unprofessional behaviour is made which is worth debating. It is raised in three different forms, but all concern the same matter. One of my major problems in writing the book was that in questioning the stereotype of medieval society upheld in the published work of some senior medievalists, it was necessary to go beyond and behind their work. Some use could be made of what had been published, showing the inconsistencies and questionable assumptions. This I did, particularly in relation to the work of Homans and Hilton. I could also use some published village studies, late medieval records which I myself had studied at first hand for the parish of Earls Colne from 1380 onwards, and certain literary and published legal sources. I did use all these, but in order to be really convincing, it seemed sensible to draw on the very latest and most detailed research, that is the material which had been submitted, examined and passed as a doctoral thesis. A great deal of time and money is spent on such research and it is usually of a very high standard. I had always assumed that, with the author's written permission, such doctoral theses were legitimate sources for historical writing. A thesis is deposited in a University Library after having been examined, and I had assumed that, with appropriate permission and acknowledgement, it was then in the public domain. Why else examine it or have it so deposited? Of course one should preserve the usual scholarly conventions in dealing with such materials, but I had never realized that it should not be used. Yet such seems to be the argument of several medievalists.

The matter is formally raised by Dr.Faith who wrote that "It is, of course, perfectly fair, indeed useful, to draw the reader's attention to the existence of such work. It is quite another thing, surely, to use its conclusions, without presenting the evidence in full, in order to support one's own arguments. It is highly dubious to use it to support conclusions with which the authors themselves may not agree." If one accepted this view, it would in effect mean that doctoral theses were often not usable. To quote "the evidence in full" for each argument would usually be impossible, both because of space restrictions, and also because the author would rightly object to a very large amount of their unpublished research being lifted into another work. Moreover, if one were not allowed to come to conclusions different from the thesis-writer, or even those with which the writer "may not agree", there would be other difficulties. When citing material from a thesis, just as when citing from any other long book or source, it is not implied that the author endorses all the other conclusions in my book. Nor is it implied that I agree or accept all his conclusions. Clearly the cited authors have the right to publicly disclaim any association with my views or use of their materials if they wish.

The same matter is also raised by Dr.Dyer who believes that "there is surely a fair and reasonable convention that unpublished works should not be used extensively until we all have an opportunity to read them and make our own judgments". Given the state of thesis publishing, that may be a very long time indeed. Is such a convention also to be applied to other "unpublished works", namely the original sources which are the heart of historical research? Should one not use materials in Record Offices and other archives until they are made available, in published form, and we have all had an opportunity to read them?

A related point, in a somewhat less polite manner, is made by Professor Hilton. He wrote that the book is based on "printed works....unpublished books and articles" which apparently "unsuspecting authors allowed Macfarlane to see before publication..." It is implied that if such authors had realized that I was going to openly question the orthodoxy of certain senior medievalists, they would have forbidden me to use their work. This throws an interesting sidelight on medieval studies. One of these unpublished works that I am accused of unscrupulously using is the thesis of Dr.Razi on medieval Halesowen. Here I am accused by Professor Hilton of citing Razi when it suits my argument, but not when it does not do so, namely on age at marriage. In general, as earlier argued, I do not see it as an obligation of a historian to cite all of an author's conclusions if he cites one. In this case I did not do so for the following reason. I had been in a lengthy correspondence with Dr Razi in which I explained to him why I found his work on age at marriage based on totally unconvincing methods. I found that he was unable to satisfy me that ages could be calculated from manorial records. I had tried his methods on my Earls Colne material and found that they were circular, and untrustworthy. At one time I thought of putting these detailed criticisms in my book, but I was advised that it would not be entirely appropriate to attack the methods of an unpublished doctoral thesis in a published book. I agreed with this view. My restraint in only using doctoral research in a positive and constructive way, rather than negatively, may not have been known to Professor Hilton, even though he was Razi's doctoral supervisor. I continue to hold to the view that I should not have cited unpublished conclusions with which I disagreed and then been forced to criticize them.

The unpublished work which was most vital to the medieval section was that of Dr. Richard Smith. It was to a considerable extent the meeting of our minds, looking at similar problems but from the perspectives of a medievalist and early modern historian, that made the book possible. I had many discussions with Dr. Smith and he generously read much of the book and lent me much of his unpublished materials. My use of his work was always fully cited and a very full acknowledgement given in the preface, where I wrote that "this book is, particularly in relation to the medieval section, largely the book which he could and might have written, though his conclusions might have been different from mine". Dr. Smith was always supportive and never expressed anything but pleasure that his work was being used. Yet this also has offended medievalists.

Dr.Alan Baker wrote that "the original contribution of the book lies in its **unduly** heavy reliance...on the unpublished findings of Dr.Richard Smith...while that work clearly supports the thesis that English

medieval societies and economies were open rather than closed systems, this hardly excuses the **excessive** use which Dr Macfarlane **apologetically** makes of it."(my italics). Professor Raftis complains that "more especially has Macfarlane **exploited** the confidences of his colleague, Richard Smith...It is a pity to have a promising young scholar's work presented in such a fashion that, by the end of the volume, one begins to expect references to 'poor Richard's almanack'"(my italics). This laboured joke is presumably meant to further discredit the venture.

It is worth noting that the complaints of unethical use of unpublished research should come from medievalists whose views on medieval peasantry are most directly challenged. With the exception of Dr.Dyer, none of those whose work has been so used has ever in private or public complained to me. It is not claimed that on the whole my use has been inaccurate or mistaken. There is just a claim that I have somehow unfairly relied on others. Perhaps one can sympathize a little. In the best of all possible worlds I would have been a full-time medievalist myself, as well as an expert on early modern England and an anthropologist. Even if I had been, it might have been difficult, single-handed, to have studied court rolls all over England in considerable depth. Or again, I might have waited for some years to see whether the results in doctoral theses were published. In fact, the majority of these doctoral theses are still unpublished ten years on. As it is, I tried to act as honourably and carefully as possible, giving full acknowledgement and obtaining full permission. I leave it to the reader to draw conclusions about unprofessional behaviour and the degree to which one is allowed to question the published pronouncements of professionals.

The next criticism of the scholarly reliability fits rather oddly with the last. It is the allegation by Dr. Baker that my views are "not based on any new corpus of original research" and hence my account is "obsolete". Having been castigated for trying to make use of the very latest empirical and original research of medievalists, it is somewhat strange to be told that the work is not based on any "original research". Presumably, the criticism is that I have not done any "original research" myself, merely plagiarizing the work of others. If this were true, it would indeed be a serious criticism. To successfully convict a fellow historian of not basing his conclusions on "original research" is a damning criticism in the eyes of many. How could it be possible for me to successfully challenge a whole corpus of received wisdom about the past if I had not undertaken such research? Without using primary sources of a new kind, or analysing and integrating them in a new way, it would indeed be difficult to challenge those who had spent many years in documentary research. It is thus important to answer this accusation, so lightly made by a reviewer, and yet seeming to undermine my work.

**Individualism** lists the manuscript sources which formed the background of many of my arguments, and references in the text and footnotes give a few examples of where I have drawn on these materials. But since Dr. Baker, and perhaps others, do not seem to take this as evidence of "original research", perhaps I can elucidate and expand the statements a little. In five years of research for a doctoral thesis on witchcraft, I did a great deal of "original research", working through many kinds of original documents, particularly court records and village sources. I then worked on another class of materials, namely autobiographical sources and particularly the diary of Ralph Josselin which I edited for the British Academy and analysed in a book. Building on this early work, with the help of a team of helpers, I spent, as explained in the preface of **Individualism**, some fourteen years reconstructing the history of two parishes, one in Essex, one in Westmorland. This involved locating, understanding, transcribing, indexing by hand, over twelve thousand (when typed) pages of original documents. By the time that I wrote **Individualism**, much of the material had already been transcribed, indexed and was being fed into a computer. An original way of bringing the records together was described in **Reconstructing**, and all the records were finally published on microfiche in 1981.

These published records are the largest set of original documents ever published for a single parish. Over seven thousand pages of wills, manorial records, Chancery records, hearth taxes, parish registers, ecclesiastical court records and many other sources were transcribed and indexed. Every family in the parish was reconstituted, every landholding and house traced in its history from the early fifteenth to the late eighteenth centuries. The application of computer systems, and particularly an advanced database

management and enquiry system to this material was entirely original. An almost equally large quantity of material for Westmorland was transcribed and partly indexed. Other parishes in Essex and Westmorland were also partially investigated.

It is important to state unambiguously that without this very deep immersion as part of a research team in these original documents, it is quite inconceivable that **Individualism** could or would have been written. No other historian, to my knowledge, has ever undertaken such a detailed analysis of a single parish and then set his or her findings against the current historical orthodoxies. It was the clash between what the "original research" into documents appeared to show, and the interpretations of a number of early modern and medieval historians which forced me to rethink the framework of the supposed revolutions of English history. I find it extremely strange that Dr. Baker should make such an error in his review, but I am happy to reassure him that I would never have had the temerity to challenge authorities like Homans, Hilton, Postan and others on the interpretation of medieval court rolls, for instance, if I had not helped to transcribe and understand many hundreds of pages of such documents at a period when they can be compared to other sources.

A final criticism of the scholarship is of its detachment. Professor Hilton feels able to discern an ulterior motive in my work, a form of xenophobia, if not racism. He hints in several places that I am some kind of 'little Englander', that I despise foreigners, that perhaps I believe in the racial superiority of the English. This is not the most attractive image for someone who teaches anthropology and most of whose teaching and research now concerns the Third World. It is therefore worth bringing what is embedded in asides and hints out into the open, in order to attempt to answer it. In three revealing asides, Professor Hilton implies that the tone of my argument is summarized by writing that "the English, as distinct from less fortunate peoples living in Europe...", "conceptions of property are what differentiates the English from lesser breeds", and, concerning family structure "Perhaps this suggestion is suspect because it implies that the English had habits like the continental Europeans". Now it is true that I argue that the English do seem to have been peculiar in many ways from early on. I fail to see how Professor Hilton can turn this into a thesis of English superiority. I am surprised that a distinguished historian should have felt able to attribute such attitudes to a colleague. These are obviously imputations which I thoroughly reject.

I leave it to the judgment of readers to decide whose scholarly standards have been damaged most by this debate. It seemed important to examine the criticisms because it is all too easy as a reviewer to assert things without really thinking through the imputations. In general the seriousness of one's responsibility in this role is not normally reinforced by a detailed answer to one's criticisms. In the conclusion of the book I have tried to show how interesting and constructive many of the criticisms were. Here I have pointed out how there is a danger that over-zealous commitment to a certain historical paradigm can lead one to attack a threat with the wrong weapons and in the wrong manner. There are sufficient weaknesses in **Individualism** as an argument to make it unnecessary to invent faults in order to sink the book before it can even leave port.