

TransactionNumber: 263279



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Location: Warehouse

Article Information

Journal Title: Journal of historical geography

Volume: 5 Issue: 4

Month/Year: 1979 Pages: 426-

Article Author: McCloskey,

Article Title: Another way of observing the open fields: a reply to ARH Baker

Loan Information

Loan Title:

Loan Author:

Publisher:

Place:

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Imprint:

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by the process as well as by a vast array of circumstantial data. In fact, one need only set the large number of settlements throughout Britain which bear prefixes like East, West, Nether, Upper, etc., beside the unambiguous evidence which links such prefixes to early procedures of land division to appreciate that we can no longer glibly assume that such settlements manifest parent-daughter relationships as traditional opinion would have us believe.^[1] I may be guilty of overstatement but I would have thought my case was as securely based as that offered by archaeologists like C. Taylor and P. Wade-Martins for the greater nucleation of settlement over the ninth and tenth centuries. For this reason, I suggest the way forward is not to ask whose argument is wrong, but how they can be brought together within a single framework of interpretation. After all, there is no clash of chronology for I suggested that the main phase of splitting was probably after 1086. One approach is to keep in mind the fact that in many instances the splitting of settlements was linked to the splitting of estates, especially the earliest examples. Might we not envisage an initial phase when the proprietary fission of estates acted on large multiple estate-type units and served to re-group settlements, followed after about the tenth or eleventh century by a phase when units of estate-holding became focused on individual settlements so that their proprietary fission led to the process of township splitting.^[2] Such a proposal may not allay Baker's suspicions entirely but it embodies the sort of reasoning which ultimately may yield a satisfactory answer.

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Another way of observing the open fields: a reply to A. R. H. Baker

Donald N. McCloskey

In open fields as in intellectual fields a farmer views trespass as a serious offence. An economist trespassing on the lands so lovingly planted, cultivated and harvested by historical geographers over the past twenty years or so can expect to get an ill-tempered round or two of buckshot in his posterior. He can only retreat hastily to the bordering woods, and commence picking out the shot while explaining (at a safe distance) his business in the field. Most apparent cases of trespass are mutual misunderstanding, not malice, and the present case is no exception. My friend Alan Baker and I admire each other's business but do not entirely understand it, which is not surprising. Nor do we let our internal convictions of our own ignorance—for we are both wise men—stay for even a moment our

[1] I have discussed township splitting more fully in chapter 5 of a book entitled *The origin of British field systems: an interpretation* (London, forthcoming)

[2] Relevant comment on the changing size of estate units can be found in T. Aston, *The origins of the manor in Britain*, pp. 11–35 of W. Minchinton (Ed.), *Essays in agrarian history*, I esp. 29 (Newton Abbot 1968); W. Davies, *An early Welsh microcosm. Studies in the Llandaff charters*. Royal Historical Society's *Studies in history* series no. 9 (London 1978) 32–42

trigger fingers in academic dispute, which is surprising only to a naively optimistic observer of academic nature. Most academic disputes turn not on carefully considered matters of fact or logic but on oversights, misreadings and misunderstandings that we would not tolerate in the weekly essays of our first-year undergraduates.

So it is here. Baker commends me for "exposing the underlying logic" of scattered plots in open fields as insurance, but criticizes me for producing "no new empirical support for such a thesis", for "erroneously" asserting that among historians only Marc Bloch understood the idea, and for "unnecessarily" claiming that it is a "primary process" in causing open fields. In brief, McCloskey seems a bright fellow but his stuff is dead wrong, and furthermore we already knew it was true anyway.

The first criticism, for not offering new evidence, is hard to understand. To be sure, my early essay in the Jones and Parker volume, which Baker cites repeatedly, was merely critical and theoretical, offering the hypothesis of risk aversion only at its end, diffidently. But my later essay, which he cites only once, is full to the point of death by surfeit with new facts. It uses anthropological reports to show that risk aversion is a motive for scattering nowadays; manorial accounts to show that the level of risk faced by a peasant was high, and how high; quotations of prices to show that price fluctuations did not offset the risk; contemporary chronicles to show that peasants were closer to disaster, and how close; modern field experiments and medieval manorial accounts to show that a diverse holding was safer, and how much; manorial surveys and glebe terriers to show that the actual number of plots was close to the number the motive of insurance alone would predict; and a variety of sources to show, tentatively, that alternative insurance was poor. To describe the ratio of fact to its forty pages a reader would probably use the word "full", if not "crammed" or "gorged". Yet Baker says that it is a contribution to logic alone, "empirically inconclusive", with "limited empirical verification". Perhaps he did not read it. Or perhaps he has higher standards of conclusiveness than most of us do. Or perhaps he has let his methodological conviction that "observing process" (whatever exactly that may mean) is the only observation worth making obstruct his seeing the facts. I invite the reader to try out these various hypotheses at my expense: allow me to send you a copy. In the meantime, I submit that he has overlooked the facts in his fascination with the theory.

The second criticism—that the idea of open fields as insurance is not original with me—is a criticism of my reading of the literature. By Baker's method of reading there is no new idea under the sun, for one can always claim that the idea was "implicit" (Baker uses this word or a variant each time) in earlier writers on open fields, not to speak of Adam Smith or Aquinas or Aristotle. Regardless of their plain words, by retrospective humpty-dumptyism one can claim that what they all really meant was that plots were scattered to give a diversified portfolio of types of land. It should be noted that Baker and I do not disagree on the texts to be interpreted: he quotes in French the passages from Bloch I quoted in English translation, and then gives again the citations I gave to other texts. Baker's contribution is merely to insist that these historians believed that village-wide egalitarianism was meant to serve individual insurance. Such a reading misses the one persistent theme in the literature on open fields from Paul Vinogradoff to Marc Bloch and beyond, the theme (as Maitland put it) of sacrificing efficiency at the shrine of equality: "the communal principle with its equalising tendency remained

still as the efficient force regulating the whole";^[1] it was "the strength of the collective idea", not private and selfish motives of convenience, that "found its expression in open-fields with long furlongs".^[2] Come now. There is no insurance in this, simply redistributive fervour. When they do not merely revive the old, illogical, and mistaken idea of Frederic Seebohm that common ploughing did the trick, all the anticipations of insurance Baker detects are in fact plain statements of the alleged importance of fairness in the medieval village (the same village so cruelly unfair in everything else, it should be noted). The authentic anticipations of the idea are in the anthropological and geographical literatures, not the historical. With the exception of the recent stream of historical geography to which Baker himself has so brilliantly contributed, the historical literature on open fields is sunk in *non sequitur* and romance. Marc Bloch alone rose above it, mentioning insurance briefly and (truly) inconclusively on the way to a celebration of the "rudimentary communism" that was "the very reason for [the] existence" of medieval agriculture. I urge the reader to read again. In the meantime, I submit that Baker has misconstrued the position of insurance in the literature of open fields.

The third criticism—that I "unwisely" tried to make insurance explain everything, instead of giving equality, inheritance, joint ploughing, and so forth their due—is correct if shorn of the "unwisely". I really must insist that Baker read at least pp. 154–161 of the second, empirical article, for in those pages I claim to demonstrate that insurance does explain the observed amount of scattering. His bald counter-claim that it is "unnecessary" to make insurance a "primary process" looks very strange when put alongside an empirical demonstration to the contrary. I say at the outset that "it would be irresponsible to offer risk aversion as a large part of the explanation of scattering, urging that it replace inheritance customs or communal plowing in the usual tales, if risk aversion could not explain some significant part of the scattering actually observed". The responsibility to show that one's theory has the power to explain some notable fraction of what is observed is not commonly accepted by the historian of open fields. As does Baker, he proceeds instead by listing factors without assigning weights to each; or at best by assigning "primary" and "secondary" roles in auditions closed to the public and press. I demonstrated even in the article Baker clearly did read that partible inheritance, for example, could not perform the role assigned to it. If partible inheritance, joint ploughing, and egalitarianism contradict the social and technological evidence we have (they do), and are not able in a quantitative sense to explain open fields (they are not), there is little reason to retain their services. One can imagine "an elegantly theoretical justification in economic terms", of course, why they need not be dismissed: they can hold the coat of the principal actor, or move the scenery. But Baker and I run businesses of fact, not charities for superannuated theories. I again urge the reader to judge for himself whether the facts leave any option but ("unnecessary") de-emphasis of the older theories. And in the meantime, I submit that Baker's eyes have passed along the lines of my papers but have not seen.

I try to be sympathetic with Baker's intent, and to be clearer than he is in supporting his case. He would want chronological evidence for insurance, preferably some sequence of communal decisions or individual trades exhibiting its progress over time. My evidence that insurance is consistent with the state of open fields at

[1] P. Vinogradoff, *Villainage in England* (Oxford 1892) 237–8

[2] M. Bloch, *French rural history* (1931, English trans. 1966) 47–8

some given time leaves him cold, nor is he persuaded that the chronology of enclosure is evidence for insurance. He sees hints of the insurance motive in earlier theories of scattering, and is annoyed that I claim to have thought up so simple an idea. He wants to keep insurance in the position he believes it holds in the literature, as a supplement to the stronger forces of egalitarianism.

But being as sympathetic towards it as possible it is nonetheless a hastily constructed argument, based on a misreading of the literature and a lack of understanding of the work by me he cites. In the attention he gives my work I am flattered and pleased. But I am dismayed that even he adopts the method of Senior Common Room history, of testing facts (as Alexander Gerschenkron once put it) against vague preconceptions instead of the other way around. The "fair feeld ful of folk" deserves a more serious attempt at understanding.

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