
A Reply to Professor Charles Wilson

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Dear Professor Wilson:

How pleasant to have your letter from Fiesole! My mother used to live across the valley, on the other hill as it were, and I have always had a warm spot for Florence.

How pleasant in another way, too, for you have replied very fully. Let me respond in order to the points you raise:

p. 194: The hypothesis that people avoid the risk of a bad harvest does not require them to feel themselves miserable or to be able to imagine much larger yields. I cannot locate where I left this impression, but it is quite unnecessary anyway. What is necessary, at least at the margin, is some awareness of the alternative of consolidating one's land. There is ample evidence for that awareness; the most conclusive evidence, I suppose, is the actual consolidation of open fields, as in parts of Kent. And of course all open fields had bits (sometimes more than bits) of consolidated land within and around them. The peasant would not need a degree in economics (or, more to the point, agronomy) to see, if it were true, that John seemed to be getting better yields than he was. The "consciousness of inefficiencies" need only be very vague.

Economists, I must remark, often have this problem in speaking with human beings. There is a widespread opinion that economic rationality requires close calculation of a sort that one seldom observes in modern businesses, much less in medieval villages. The opinion comes, I think, from the words economists use — especially that word "efficiency", with its ring of... uh... efficiency about it.

p. 194: Yes, we agree that open fields must be explained in terms of the ordinary needs of the peasants, not communalism. But it will not suffice

to simply assert that open fields came from needs and leave it at that. I am probably being uncharitable, but it seems to me that the Orwins do precisely that. They leave the "explanation" at a very high level of generality, the sort of thing that has been called an "explanation sketch".

p. 194: Yes, we have no evidence of peasant farming. The distinguished company and I will sleep soundly, however, until it is shown how the gap in evidence affects the arguments made using the substitute evidence.

p. 195: Monocausalism. As an initial strategy of research I would say that nothing beats it and that we all do it. After all, it is possible, is it not, that the open fields *were* caused by one thing.

p. 195, bottom: Yes, I should have more evidence on the detailed source of the hazards. I worked on a number of leads for a while, trying to find out how spotty is the occurrence of mould, for example, or how wide and common are thunderstorms (I cracked that last). I came to two serious problems that discouraged me from spending more time on that line of research. First, the truly detailed evidence is necessarily anachronistic: plant varieties change, moulds mutate, weather cycles. Second, unless the evidence comes from an agronomic experiment with very nearly the same interests as I have it does not tell me the crucial fact: how much are *yields* affected by such and such a plant disease or late frost or whatever. The point is similar to one I confronted in examining soil maps of English villages. It is not difficult to find astonishingly detailed maps prepared recently, with three dozen different types of land marked on them. But after one has looked at these for a while it becomes clear that they are irrelevant to the main purpose unless it is known *how much* the variety of soils contributes to the variety of yields (and indeed, how: is a very sandy soil *significantly* better drained than a moderately sandy soil? That's the question, a question of setting the soils on a scale).

p. 195: You say "By and large, I suspect that disaster struck by areas very much larger than half an acre — or even thirty acres". I am puzzled by this suspicion, for I was under the impression that my evidence carried the discussion beyond suspicions and into fact. Certainly, this is the central fact (if it is a fact) that I need to establish, that yields did not move in lockstep over even small areas. I do not pretend that pp. 145 (really, 146)-150 of my 1976 article settle the issue decisively, but surely it is now beyond a matter of "suspicion".

Allow me another aside. Your "suspicion" reminds me of the "theoretical presumptions" of my economist colleagues: economists believe in their hearts that matters of fact are determinable by standing at a blackboard.

p. 195: Yes, I wish I could figure out which demesnes were consolidated and which intermixed with tenant lands. I do not claim familiarity with the original documents on this score, so I can't say whether or not the question is for some reason unanswerable. I agree that it is not usually answered. But I don't see that it matters much to my case, which is built chiefly on the evidence

of neighbouring demesnes (for which it matters little whether or not each was consolidated) and on modern experimental evidence.

p. 196: Yes, again I agree that we lack evidence on peasant farming (though not on peasant attitudes: the court rolls and the popular literature of the time speak to this). But again I do not see its special relevance to my argument. It might be worth noting that many of the demesne farms were little — not 10 acres, to be sure, but not always 150 or 300 either. It's not clear to me why there should be any difference in practice on little demesnes and large peasant farms, which clears part of the ambiguity.

p. 196: Now I see why you raised the question of consolidation of the demesne. Well, the consolidation of large farms (the demesnes among them) is in fact evidence for my view: a big farmer needed less insurance. But I accept, as I've said, your implication that we in fact do not know very much about the state of the large farms (including the demesne).

p. 197: I don't really suppose that at every instant all options were open. Have you read my essay on enclosure?³ The entire point of that essay is to show clogged and clotted were the routes out of open fields, and how the obstacle were shifted about.

p. 197: The cake of custom. Sure, 'tis true. But what of it? I myself am bound by customs of language, scholarly discourse, mechanical knowledge and habit, and so forth in typing this page. In a sense I am tied hand and foot by the custom of the academy. My father (who was also an academic) used a mechanical typewriter; if I do so is it evidence of my veneration for the traditions of my craft or is it merely evidence of my penury that keeps me from buying an electric one? We must not suppose that peasants were tradition-bound merely because the traditions by which they were bound are different from our own, or because the conditions of technology and resources under which they operated changed slower than do ours. What I am saying is that the stickiness of the system is a matter for difficult and subtle test; it is not a look-see matter. Havinden's work on open-field Oxfordshire is a case in point: we expect the open field to be bound by traditions in cropping, but on close examination it's not so.

p. 198: Yes, as I say, my essay on the enclosure movement talks about these matters.

p. 198, top: The cat, as they say, is out of the bag: "the original joint ploughing by which it had been created". I thought that explanation (Seeböhm and, with obscuring emendation, the Orwins) was dead, but I have been proved wrong several times recently. How do you propose to handle the well-known objections to Seeböhm's theory? That, e.g., open fields occurred in areas with cheap ploughs: that they were created anew in areas in which each peasant had

³ John Parker-Jones, as cited, 1975, pp. 123-160.

a team (such a statement demands qualification: each peasant of substantial size, say 30 acres); that ploughs and teams could be and were rented; that there is in fact no apparent reason why common ploughing should result in scattered holdings (think it through: I think you'll agree with me that there is a step missing, a crucial one, that connects common ploughing and the share each contributor demanded of its fruits with a scattered holding); and above all that scattering persisted centuries beyond the time it is possible (but only possible: the direct evidence of common ploughing is very weak) that ploughing was communal.

p. 199: The Crunch. Well, yes: I have searched and I have not found. What you mean by "direct evidence" is contemporary journalism (so to speak) attesting to the power of risk aversion to explain the open fields. Right. Case conceded. *Nolo contendere*.

But wait. Other, more learned students of the institution — the Orwins, Joan Thirsk, Ault, Tate, Seeböhm, Vinogradoff, Beresford, Gray, Homans, Maitland (I write them out in a verse) — have searched for years without discovering a single piece of direct evidence either. I would be delighted to be corrected in this, but as I understand the historiography there is not a single statement of the sort you demand supporting *any* theory of the open fields. Not one. This is a strange affair, so strange that I am inclined to doubt my perception of it. But perhaps not so strange after all, for you would be hard put to find a single piece of direct evidence from a single modern Englishman, farmer, priest, government bureaucrat, businessman, poet, or bishop explaining any number of institutions — planning permission, for example (to take a reasonably close analogy); or public schools; or government provision of roads. These are all assumptions of the society, little studied and little commented on. That does not mean they are unmoveable, by the way, whether by collective or individual action. They are simply not controversial. By the XVth century, when open fields do become objects of controversy, it suffices for the controversialists' purpose to label them mere custom.

p. 199: What contemporary evidence of the effects of joint ploughing of the open fields do you have in mind? Does it really meet the standard of "direct evidence" you would apply to risk aversion (or to partible inheritance, the main modern contender)?

p. 199: Farmers grumble, they do not worry. But I do not require them to worry, merely to take reasonable precautions. That modern farmers will often grow many different crops is testimony to their caution on this score: they diversify (the crops have other functions, too, sometimes, but the declared purpose of buying equipment for planting soybeans in the American Midwest, for example, is precisely to insure against a wet spring that will make planting corn or wheat impossible).

p. 200: I agree that there might be room for unideological equality. The best example I can think of is the sharing out of the meadow land (in which

Jack would have 10 lots, John only 5 but the actual location was determined by chance); another is the common practice (of which I speak in the paper) in a bitter division of an inheritance of dividing the patrimony by lot. What I am writing against, however, are precisely the ghosts of bearded Victorian prophets. They are surprisingly active in haunting the minds of educated people (even, I may say, educated historians, if agricultural history has not been part of their education),

p. 200: The psychology of the peasant that I use is not, I think, so strange: surely, to keep alive, and to be as rational as he could manage (amidst the irrational incantations and signs that he also made use of) in pursuit of that goal.

I thank you for your close attention, and am glad that you now believe the hypothesis of risk aversion deserves perhaps more than a sentence. Thanks, too, for the opportunity to exercise my own thinking on the matter. I am immersed in university business that takes me far afield (so to speak), and it is good to return to real work.

Sincerely,
Donald N. McCloskey

