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SCATTERING IN OPEN FIELDS: A COMMENT

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sceptical of the power of a distaste for risk to explain scattering, for in the later papers I deliver the goods that were only hinted at in the early one. His conclusion on this score makes ironic reading in view of what is now known. Says he, "it seems unlikely that the limited area of the typical community's arable land offered sufficient variation in conditions to justify separate parcels in such numbers". I offer in the paper published in 1976, some fifty pages of evidence that, unlikely though it may seem, it is true.

My purpose here, however, is not merely self-advertisement and the correction of an incomplete picture of the state of play in the field. Mazur's piece, for all its lucidity in expression and penetration in economic reasoning, illustrates some alarming features of the application over the last eight years or so of economics to medieval history. I would like to use his contribution, unfairly no doubt, to raise the alarm. If the flame is not quenched soon it will leave in ruins whatever small chance economics had of influencing medieval historiography.

There are three points to be made. The first and the most narrow is that Mazur, in common with many other economists venturing into medieval history, supposes that medieval villages did not know private property in land. "A village community", Mazur asserts, "would have had little incentive to preserve throughout the fallow year the land divisions which were established during the period of cultivation".⁴ Compare the similar if more guarded assertions of B.D. Baack and R.P. Thomas in this *Journal* in 1974 or of J.S. Cohen and M.L. Weitzman in 1975.⁵ The alleged lack of privateness is the justification for applying the so-called "fisheries" model to open fields. That manorial courts were often virtual land registries for petty transfers of land or that rents were high and variable or that the only non-exclusive exploitation of the soil was (sometimes) the grazing of animals has not stopped these economists from seizing on the word "common" in common fields and making it into a tale of a primitive communism that never was.⁶

³ *Op. cit.*, p. 471.

⁴ *Ibid.*, p. 465.

⁵ B. D. BAACK and R. P. THOMAS, *The Enclosure Movement and the Supply of Labour During the Industrial Revolution*, "Journal of European Economic History", 3, no. 2 (Fall 1974), p. 419 ff.; J. S. COHEN and M. L. WEITZMAN, *Enclosure and Depopulation: A Marxian Analysis*, in Parker and Jones, eds., *op. cit.*, pp. 165-169. It is a tribute to the ideological blindness of economic analysis as practised in the United States that the same argument enchants both self-declared Marxists and self-declared capitalists.

⁶ Cf. S. FENOALTEA, *On a Marxian Model of Enclosure*, "Journal of Development Economics", 3 (1976), pp. 195-198, which criticizes Cohen and Weitzman on the same grounds. See J. S. COHEN and M. L. WEITZMAN, *A Marxian Model of Enclosures*, "Journal of Development Economics", 1 (1975), pp. 287-336. A devastating critique of the notion of primitive communism is contained in ALAN MAC FARLANE, *The Origins of English Individualism* (Cambridge, Cambridge University Press, 1978), pp. 80-130.

The second peculiarity of this literature is most characteristic of historians, not economists: but the economists have caught the bug. Following medical terminology we may call it "megaloetiologism", that is, the enlargement to excess of the study of origins. Mazur is well aware that his "is an explanation of the origin of scattered holdings and is not an explanation of their persistence into the post-medieval era",⁷ although he is less aware that the supposed origin is located in any place in the ninth or tenth century, leaving five centuries of middle ages to fit into his "post-medieval era". The main point is that studying the origin of open fields or of anything else is antiquarianism unless it is accompanied by evidence connecting the origin to the persistence. Coarctation, partible inheritance, clearance of waste, risk aversion, land hunger, or whatever could begin an open field in 890 AD, causing it to survive one year to 891, without contributing at all to the more important fact of its persistence from 890 to 1790. The point is especially clear in another branch of the literature, the modestly titled *A Theory of Economic History* by J.R. Hicks and *The Rise of the Western World* by D.C. North and R.P. Thomas, in both of which the manorial system is alleged to have originated in a mutually advantageous exchange between serf and lord of tribute for protection.⁸ Suppose this was so. We now have an origin. But an origin without persistence is merely a terminated pregnancy. The question is, how was the manorial system (or the open fields or the rise of cities or whatever) sustained? When applied to the origin the cheerful picture of exchange between serf and lord looks merely dubious; when applied to the continuation it verges on comical. As Alexander Gerschenkron has put the matter, "the main, if not the only, danger against which the peasant very frequently was in need of protection was the very lord".⁹

Megaloetiologism may lend support to a political opinion, as when an original accumulation from the slave trade damns capitalism or when an interpretation of early Christianity damns the modern church. Or it may satisfy a naïve though powerful need for a creation myth, a story of Genesis to begin the book, as when the medieval manor is said to spring from a social contract or medieval agricultural arrangements from the accidents of ploughing. But it must not be allowed to grow into the whole historical enterprise. Considering the evidential void that nearly always surrounds origins, megaloetiology is fatal to serious history.

⁷ MAZUR, *op. cit.*, p. 471.

⁸ J. R. HICKS, *A Theory of Economic History* (Oxford: Clarendon Press, 1969), p. 102; D. C. NORTH and R. P. THOMAS, *The Rise of the Western World* (Cambridge: Cambridge University Press, 1973).

⁹ *Mercator Gloriosus*, "Economic History Review", n.s. XXIV, no. 4 (November 1971), p. 655 and (again) the work of S. Fenoaltea, this time *The Rise and Fall of a Theoretical Model: The Manorial System*, "Journal of Economic History", XXXV, no. 2 (June 1975), pp. 386-390; both criticize North and Thomas on the same grounds.

The evidential void leads to the third and oddest peculiarity of the literature of economics applied to medieval history, most characteristic of economists but not confined to them. The peculiarity is the writing of history in the subjunctive mood, that is, replacing evidence of what did happen and what people did do with what "must" have happened or what people "would" have done. Mazur is not the worst offender but merely the most candid when he writes that "*it would be likely that on the first day of ploughing all the plough teams of the village would go to the best area*", or that "*there are enough advantages to a system of impermanent land allocation to render its existence highly credible*".¹⁰ Mazur does not offer evidence that either of these non-facts crucial to his argument is true, or that any of the two dozen or so similar statements in a short article are true, and the procedure is typical of such work. It embodies the belief that a theoretical presumption (usually on second thought an ambiguous presumption, but there is little time for second thoughts) has the same status as evidence as does a fact. In suggesting still another explanation of scattering, for example, Stefano Fenoaltea argues that without scattering across types of soil "each peasant *would* have to work his land largely at suboptimal times, and output *would* suffer accordingly"; to meet the objection that peasants traded labour during harvest and planting he has another presumption in the subjunctive mood, that "all the problems associated with working for others *would* significantly reduce the effective input of labor".¹¹ Fenoaltea offers no evidence from any age that either of these effects was potent. So it goes.¹²

The cumulative effect of reading the literature is at first elation that a technique has been discovered for writing history from an easy chair, followed by depression when the technique turns out to be the chimera of schoolmen ancient and modern that the facts of the world may be ascertained by unassisted reasoning.¹³

¹⁰ MAZUR, *op. cit.*, pp. 464, 466, my italics.

¹¹ S. FENOALTEA, *Risk, Transaction Costs, and the Organization of Medieval Agriculture*, "Explorations in Economic History", 13, no. 2 (April 1976), pp. 142, 143s my italics. See my *Comment* on Fenoaltea's essay cited in note 2 above.

¹² Other examples are EVSEY DOMAR, *The Causes of Slavery or Serfdom: A Hypothesis*, "Journal of Economic History", 20, no. 1 (March 1970), pp. 18-32; and CARL J. DAHLMAN, *The Open Field System and Beyond: A Property Rights Analysis of an Economic Institution* (Cambridge University Press, forthcoming), in many ways a brilliant book but wedded to the subjunctive mood. See also CLYDE G. REED and TERRY L. ANDERSON, *An Economic Explanation of English Agricultural Organization in the Twelfth and Thirteenth Centuries*, "Economic History Review", n.s. 26 (February 1973).

¹³ This should not, by the way, be construed as still another of the tiresome assaults on the method of counterfactuals. It is one thing to imagine in a way disciplined by facts an alternative world in order to understand the real world better; it is quite

The usual defences to the charge of theory-spinning are two; both are revealing. The first is that anyone who doubts the spinning must not "believe in economic theory", for "theory tells us" that a common field is overexploited or that men do not work as hard for others as for themselves. This restatement of the metaphysical doctrine governing the work is stunning. One might suppose two centuries after David Hume that modern scepticism would have penetrated so far, but one would be wrong. A second reply, indeed, betrays a commendable self-doubt among the theory-spinners, for the spinning is defended by being woven into a mere 'hypothesis' for which we need "further research", presumably research by the dolts who work the libraries instead of the bright lads at the blackboards. These magic words "hypothesis" and "further research" absolve the writer from responsibility to provide evidence. But with no evidential barriers to the fair field of Science the field is crowded with graziers of hypotheses. The social results are not only predictable from the model of the fishery, but — what is more to the point than any alleged theoretical prediction — do actually happen: there has been a proliferation of untested hypotheses. The fishery model, in particular, has been overfished (or overgrazed, to keep the medieval flavour), but it is not the only case. As I have explained elsewhere, for example, all that is necessary to generate a novel explanation of scattering in open fields is to posit a failure in a market that no one so far has claimed failed.¹⁴ The coaration hypothesis (of Mazur) supposes contrary to fact that plough teams could not be rented; the scheduling hypothesis (of Fenoaltea) supposes contrary to fact that labour could not be hired; the communal grazing hypothesis (of Dahlman) supposes contrary to fact that grazing rights could not be bought; the fishery hypothesis (of Cohen, Weitzman, Baack, and Thomas) supposes contrary to fact that rights to the soil could not be bought; the risk hypothesis (of McCloskey) supposes in accord with fact that insurance could not be bought. And these novelties are in addition to the older hypotheses, of immemorial custom, land-clearing, partible inheritance, village egalitarianism, and land-hunger, all of which entail a supposition (contrary to fact) that a market in land did not exist. Surely it is time to stop supposing and to start testing.¹⁵

another to imagine with no discipline of facts what the real world must have been in order to avoid a trip to the library or, still more wearisome, to the archive. The difference is that between a physicist performing an experiment to determine the speed of light and a physicist sitting in his arm chair trying to infer the speed of light from the laws of (Newtonian) physics.

¹⁴ See McCLOSKEY, *The Persistence of English Common Fields*, *loc. cit.*, pp. 93-102, 113, 116-117.

¹⁵ A similar point applies to the new economic history of manorialism (Hicks, North and Thomas). They too are based on unsubstantiated suppositions that markets did not exist.

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Virtue is easier to applaud than to practise. Few could maintain that they had never swung across an evidential void on a rope of supposition, but the task should be to keep the swings short. Likewise, megaloeitologism can be reduced to mild originitis and the fisheries model can be applied where it is strictly applicable. I commend these practices to my economist colleagues. Without some adjustments of our practices medievalists will do well, in the face of economic reasoning detached from fact, to maintain their embarrassed silence. Better, even, to have what the medievalists too often give, fact detached from reasoning.