Scattering in Open Fields: Reply

Michael P. Mazur
Dartmouth College

In his comments on my essay, Donald McCloskey ranges widely into criticisms of a number of writings on medieval economic history. I shall try to limit my response to those criticisms that apply specifically to my own piece.

McCloskey asserts that the "coaration hypothesis (of Mazur) supposed contrary to fact that plough teams could not be rented". Of course, the possibility of renting did not eliminate the practice of coaration, whose existence is well established.[1] The fundamental point is that my hypothesis does not rest on coaration at all. Indeed, the starting point for my hypothesis was the observation that coaration was neither necessary nor sufficient to explain the prevailing dispersion of holdings.[2] This observation is applicable whether or not plough teams were rented, so that my criticism of the Seebohm-Orwin coaration explanation is more general than McCloskey's.

What my hypothesis does have in common with the Seebohm-Orwin theory is not coaration, but the assumption of an association between the ploughing process and the division of strips. This assumption, while once taken as fact by many, must be considered an open question.[3] If it were to be refuted, that

---

[1] ALAN R. H. BAKER and R. A. BUTLIN, eds., *Studies of Field Systems in the British Isles* (Cambridge: Cambridge University Press, 1973), pp. 635-36.

[2] MICHAEL P. MAZUR, "The Dispersion of Holdings in the Open Fields: an Interpretation in Terms of Property Rights," *Journal of European Economic History*, 6 (fall 1977), p. 463, last paragraph.

[3] For a succinct survey of the still-inconclusive evidence against the assumption see ROBERT A. DODGSHON, "The Landholding Foundations of the Open-Field System," *Past & Present*, 67 (May 1975), pp. 4-9.

would be evidence against both my own and the Seebohm-Orwin hypotheses.

Also like the Seebohm-Orwin hypothesis, my theory assumes that scattered strips originated during a period of opening new lands — specifically, new lands made cultivable by the introduction of the mould-board plough. This probably would date the beginning of the process early in the Anglo-Saxon period. McCloskey dates the origin of scattered holdings considerably later — in the ninth or tenth century. The fact is that we simply do not now know the time of origination; it could have been at the time of the Anglo-Saxon conquest or as late as the tenth century.[4] If the timing can be determined, it will allow us to reject several of the competing hypotheses.

McCloskey's criticisms of an excessive interest in origins are thought-provoking. Yet the demise of one institution is simply the origin of whatever succeeds it. The enclosure movement, for example, may be equally well viewed as the demise of the open field system or the origin of a system of enclosed fields. Admittedly, the origin of a phenomenon precedes its demise, and generally the earlier we go in history the less evidence we have. But if that is the thrust of McCloskey's attack, it hardly seems to justify his strong language.

It is on the question of origins that McCloskey's own risk-aversion explanation for scattering, which is put forward as a complete explanation, is at its weakest. If he believes scattering originated in the ninth or tenth century, it would increase the appeal of his hypothesis if he suggested plausible reasons why not until then did risk-aversion make scattering advantageous.

One of McCloskey's principal criticisms of my piece is that he believes that his own empirical work [5] has firmly established that the motive of risk-aversion is sufficient to explain *all* the scattering of strips in the English open fields. McCloskey's empirical study is a path-breaking work of impressive scholarship and imagination. However, I am not yet convinced that he has proved his case. Stefano Fenoaltea [6] has already put forward several thoughtful criticisms, which suggest to me not so much that risk-aversion played *no* role in dispersion, but that it does not account for *all* the dispersion that existed, as McCloskey's empirical study purports to show. I would like to add here two further considerations, which also dissuade me from full acceptance of McCloskey's conclusions.

In his empirical study McCloskey attempts to show that the actual average

---

[4] *Ibid.*, p. 27.

[5] DONALD N. MCCLOSKEY, "English Open Fields as Behavior Towards Risk," in PAUL UHELDING, ed., *Research in Economic History: An Annual Compilation of Research*, Vol. 1 (Greenwich, Conn.: JAI Press, 1976).

[6] "Risk, Transaction Costs, and the Organization of Medieval Agriculture," *Explorations in Economic History*, 13 (April 1976), 129-51, and especially "Fenoaltea on Open Fields: A Reply," *Explorations in Economic History*, 14 (October 1977), 405-410.

number of plots equalled the number he predicts would be chosen by a risk-averse peasant holder. One might think that such an estimation would require empirical knowledge of the degree of risk-aversion of the average holder. In fact, McCloskey's estimation relies heavily on a quite restrictive assumption about peasant risk attitudes: that peasant holders sought to minimize the probability of "disaster" regardless of the cost in terms of expected (average) return. This is a particularly restrictive form of the "safety-first" models sometimes used in agricultural economics. Other, more sophisticated, versions of safety-models have sometimes been used.[7] More important, it remains a questionable proposition whether safety-first models of any kind are good characterizations of peasant attitudes toward risk.[8]

In his effort to compare actual dispersion with the level predicted by risk aversion, McCloskey defines the actual number of scattered strips in a holding that he must explain with his model by a curious assumption: two strips of one person's holding are not defined as "scattered" if they are separated by a single strip held by another, but are so defined if separated by two or more foreign strips.[9] Yet for some of the most important costs of scattering (e.g., the danger of a neighbour "stealing a furrow" or the spillover effect of a neighbour's poor cultivation), either the cost is the same regardless of the number of intervening foreign strips or it rises much less than in proportion to their number. This suggests that the best definition is the common-sense one that two strips are scattered if any number of foreign strips intervenes.

McCloskey's definition is convenient, for it means that his model explains *all* the scattering which prevailed. If he had defined scattering by its common-sense meaning, it appears that he would have explained only two-thirds of the prevailing dispersion.[10]

In conclusion, McCloskey's empirical study provides impressive evidence of the importance of risk-aversion as a motive for the scattering of strips. However, it still seems probable that risk-aversion cannot account for *all* the scat-

---

[7] See JAMES A. ROUMASSET, *Rice and Risk: Decision-Making Among Low-Income Farmers* (Amsterdam: North-Holland, 1976), especially pp. 37-47.

[8] The preliminary results of recent experimental research in rural India do not support safety-first models of risk behaviour. The unique aspect of these experiments is that they used wagers involving substantial monetary payoffs to elicit the risk attitudes of the participants. See HANS P. BINSWANGER, "Attitudes Toward Risk: Theoretical Implications of an Experiment in Rural India," [New Haven: Economic Growth Center, Yale University], revised December 1978 (mimeo).

[9] MCCLOSKEY, *op. cit.*, pp. 156-57. More precisely, "a collection of plots is to be counted as one when no piece is separated from another by more than one other owner and no part of any piece is outside a radius of 150 yards from the center of the effective plot, ..." (p. 157).

[10] *Ibid.*, table 17, p. 157.

tering which took place and that risk-aversion must have been only one of several factors motivating dispersion.

Finally, McCloskey conjures two responses to his charge of "theory-spinning" against myself and others. One he causes to be "stunning in its naïveté". With the second reply I would identify myself, but certainly not with the pejorative connotations about empirical research that he imagines. Indeed, in this field, where theoretical models generally must be kept quite simple, the value of good empirical work to my mind generally exceeds that of most theoretical contributions, my own included. But from that I am not led to conclude that the utility of proposing a tentative new hypotheiss is zero or negative. McCloskey evidently sees such a theoretical work as an attempt to substitute theory for empirical work, whereas I see the relationship as complementary.