DEBATES

A Rejoinder

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Since my name has been mentioned twice in recent issues of the *Journal of European Economic History* I feel obliged to make a brief reply. Is it necessary, you may ask? Words come and go it is true without leaving much imprint on the reader's mind, yet there are occasions when it pays to destroy certain ungainly *canards* before they take wing since they often grow up not as Hans Christian Andersen's fine swans but as ugly lammermeyers.

Of my two critics the first, J.L. Goldsmith (J.E.E.H., 1984, 1, pp. 174-199) is perfectly polite whereas the second, J. Mokyr (J.E.E.H. 1983, 3, pp. 671-673) plunges about like a crazy Pluto. In his article *The Agrarian History of Pre-industrial France. Where do we go from here?* J.L. Goldsmith argues in support of my position and even claims that I have succeeded in establishing more accurately than anyone else the real problems need to be tackled in an analysis of the development of French agriculture in the XVIIIth century. Nonetheless, he still reports many of the slights levelled at my book, and in particular the reference to the supposed inaccuracies (or some would say inconsistencies) in some of my arguments.

My reply must start with this since I am well familiar with the nature of these assertions which are also refuted in detail in the thesis by S.L. Kaplan. But historians should not remain in any doubt about the character and the origins of these claims. Many of my colleagues who felt themselves threatened by the criticism that I levelled against their calculations, their methods and their sources deemed it wise to divert attention away from their own shortcomings by launching a concerted attack on Morineau. A wise but scarcely an honest tactic, and while those with greater integrity have now abandoned their prejudices the others still persist in their campaign of denigration while carefully avoiding any open debate or confrontation. Such unproven mutterings should be totally repudiated, since jealousy and spite have no place in serious scientific discourse.

But J.L. Goldsmith's claim that ultimately I left my colleagues still hungry because I failed to offer an alternative 'comprehensive methodology' is much more serious, and I must reply to it at greater length. My book (*Les Faux-*

Semblants d'un Demarrage Economique) was conceived as a limited exercise — much like an experiment in physics or chemistry — in which I was concerned to reveal within a specific context the presence or absence of one specific factor by identifying and eliminating all the variables that otherwise distorted the situation. In this sense the experiment proved successful, although I was naturally keen to draw as much significance as possible from the results as is quite proper within the context of a scientific discipline. But after 1968 I was no longer content to rest my case on the examples provided by the 'great cereal plains' (despite claims to the contrary by French colleagues who had only read — and in general misread — my article in the *Revue Historique*). I broadened the analysis to embrace other sectors of French agriculture (first potatoes, then livestock) while still arriving at similar conclusions. I also conducted a number of comparative analyses and suggested that even in the case of England the term 'agricultural revolution' was an exaggeration and demonstrated that the agriculture of the Far East had done just as well in supporting a population which was growing at the same, if not at a faster rate in the same period[1]. There could be only one legitimate conclusion: everything that had been said to explain how a precocious agricultural revolution had brought about the imbalance in the economic development of the West and the East (not to mention the South) needed to be thrown out, and the whole question needed to be reopened from the beginning.

As can be seen, methodological narrowness is not exactly my line but one should still make it quite explicit that all scientific verification necessitates a full reassessment of the general set of methodological issues involved and a persistent refusal to seek refuge in approximation. So where does that leave us with France and its development in the XVIIIth century? I must confess that I have not been able to devote as much time to this question as I would have liked.

Nonetheless, the problem is still quite straight-forward. Once the scene has been cleared of a mythical agricultural revolution which came from no-one quite knows where, the way is open to analyse economic transformations in the context of time and space and to confront real situations. The research programme takes its own shape, because this approach means that you must first define the principal units that made up agricultural France, then reconstruct the social structures of these units from every angle — the distribution of property, the size of farms, the types of farming (family farms, farms using ancillary labour etc.), specific yields (in relation to area, manpower etc.), revenues (in cash), the overall regional balance (since each region contains many different types of rural economies), the influence exerted by a town or towns if they are close by, as well as the nature of the contacts and links with the wider world (competition, complementarity, ease of communication, transport etc.).

[1] Readers interested in my work after the publication of the *Faux Semblants* down to 1984 will find a collection of my essays in *Pour Une Histoire Economique Vraie* (Lille, Presses Universitaires, 1985).

There is nothing inherently original in this, and in large part the model can be traced back to the traditional methods of French geographers. This was a tradition which the founders of the *Annales* embraced with open arms, but which too many historians have distorted if not completely obliterated by forcing it into the Procrustean beds of their chosen chronologies: let me study the *Lozère atlantique* under the July Monarchy or the *Jura mediterranéen* under the Second Empire etc., and completely neglect the overall problems they pose in relation to the development from primitive exploitation to super-national organizations. In other cases they fall back on pure impressionism, which has become fashionable once again especially when practised by colleagues from across the ocean or by those who can vaunt prestigious academic affiliations. Yet the sources are there for those who want to attempt an exhaustive study (exhaustive in so far as that is possible in any historical research), and once the necessary work has been done there is no reason at all why the flight from the countryside, or the distinctions that divided rural France into (relatively) poor and extremely rich regions, or the process by which we see the 'peasant' transformed into a farmer seeking to make a profit out of his enterprise and trying to come to terms with his needs for equipment, the repayment dues on credit loans, the risks of selling produce at a loss, the need for a holiday and the never ending problem of looking after his animals should remain mysteries any longer.

The line adopted by Joel Mokyr I find much less comprehensible. I am prepared to agree with him that it is matter of some curiosity that the proceedings of a conference on 'Dutch capitalism and world capitalism' held in 1976 were not published until 1982. Unfortunately this is far from the only case of a totally unjustifiable delay in publication, and although the reasons for this are frequently quite mundane the scholarly community should be aware that as a result those who are naive enough to present the findings of *original research* at such meetings are offered no protection against improper imitation. There is no way of ensuring that the ideas and evidence presented at such meetings are not simply appropriated by the skilled hacks of the academic circuit, so that the rights of copyright that are automatically accorded to any novelist, film-maker or comic strip author are effectively denied to the historian.

I also agree with J. Mokyr's criticism of the thesis advanced by Immanuel Wallerstein, and if my reasons are different this is of no real importance. Yet it still seems to me eminently desirable that these criticisms should be formulated in such a way as to develop an open debate around Wallerstein's arguments, so that both he and his critics can have their say so that the issue does not simply lead to a form of trench warfare. In a word, a debate which by identifying the criteria on which that interpretation is based contains some possibility of moving beyond them. Despite certain efforts in this direction, the 1976 conference never succeded in developing such a debate.

But why then, after these general remarks with which I can in large part concur, does J. Mokyr turn his guns against almost the only contribution that shared his own views? By presenting at the Conference a detailed reconstruction of Amsterdam trade within a specified period (1667-1668) my objective was quite precisely to pierce through the mass of generalization. Yet my calculations are dismissed simply as 'some interesting but essentially irrelevant back of the envelope calculations'. I could almost believe that I was dreaming. Either J. Mokyr paid too much attention to one of the speakers at the conference who challenged the validity of the figures on the pretence that the movement of ships abroad had not been properly registered *in the port* or else he was momentarily infuriated by a fly walking across the screen of his word-processor. The data which I used are the only ones that exist for the XVIIth century and are based on the weighted prices calculated by N.W. Posthumus and on the calculations of Johan de Vries. My exercise consisted in quantifying both volumes and value in relation to prices on the Amsterdam Bourse, and where these were missing by extrapolation from major neighbouring exchanges. So much for the envelope: it would have been pretty fully covered since the data of the calculations take up more than 40 sides of A4!.

But J. Mokyr delivers the final blow in his conclusions. I argued, he claims, that the cause of Amsterdam's fortune must be sought first of all in the interaction between the opportunities that existed and the men who perceived and exploited those opportunities. '*Whatever that means*': he concludes contemptuously. But it means exactly what I said, which is no different from what I had said on an earlier occasion at the Leningrad Conference in 1970, and which remains in direct contradiction with the still dominant view that capitalism, capitalists and capital created growth and prosperity *ex nihilo*. In support of this interpretation I would cite the largely unheeded words of J.G. Van Dillen who remained unknown until English historians rediscovered the Great Depression of the late XIXth century. Van Dillen pointed out that it was absurd to attribute every success to the 'magnificent brains' of the entrepreneurs, and then at the same time blame the inadequacies of their successors for the subsequent decline. In any process of expansion there are always external opportunities, which are then realized depending on the flair, the vision, and the energy of those who chose to exploit them. This is still valid today (whether one thinks of the hiccups produced by capitalism left to its own devices or to the collapse of some of the most highly quoted companies on the finance markets: in either case there is something that is missing, some coefficent that does not work). It is as simple as saying hello, as the egg of Cristopher Columbus or Archimedes' principle. But I shall spare J. Mokyr from crying: Eureka. After all, what he had to say had a certain interest, especially when he avoided lapsing into irrational guesswork and closed his ears to the sound of the sirens.