

## Growth in a Traditional Society: The French Countryside, 1450-1815

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Robin Briggs

As even the most casual observer of the British historical scene must know, the 'agricultural revolution' has proved both elusive and highly contentious. French 'immobilism', on the other hand, has become something of a commonplace, although explanations for this supposed failure are less consensual. Philip Hoffman's very welcome new book has two overriding merits. Firstly, it challenges just about every facile assumption about the structure and performance of the agricultural sector under the French *ancien régime*. Secondly, this re-examination is based on a large body of data, much of it new, handled with great technical skill. At the heart of the book lies a painstaking analysis of the leases issued by the Cathedral of Nôtre-Dame, from 1450 to the Revolution; these are naturally concentrated around the capital. Hoffman has also made a detailed study of eleven villages scattered across France, while employing printed literature on agriculture, *livres de raison*, and an extensive range of price and wage series. This ingenious mix of sources allows him to tackle problems from several different angles, often combining quantitative and qualitative approaches. In addition he makes a praiseworthy attempt to allow for regional variations, which indeed take on an important role in his broader discussion. It is also pleasant to record that amidst his repeated tilts at conventional wisdom Hoffman maintains an urbane and courteous tone; when he criticizes other historians, there is no sense that he hopes to make a reputation across their dead bodies. Indeed, he is often at pains to show how his findings can be made to concur with those of others, some of whose claims or methods he disputes.

One of the author's first concerns is to dispose of the old misconception that France was a country of small

peasant proprietors, who practised subsistence agriculture outside the market system. I suspect he is quite right to think that this is one of those errors which obstinately refuse to die, despite enduring repeated and apparently mortal blows. As he says, by the late sixteenth century hardly any peasants owned enough land to support a family, nor did they rent on anything like the necessary scale. Most of the population can be properly described as an agricultural proletariat, dependant on a mixture of day labour, various by-employments, and rural industry. Their market relations with neighbours were commonly expressed in monetary terms, even if a shortage of coin meant that the local economy functioned quite extensively on credit. The idea of harmonious villages relying on a co-operative and consensual set of economic relationships is a myth; the evidence suggests that these were divided and quarrelsome communities. Market values had already penetrated so deeply into village society that one must reject the stark contrast between traditional and market economies often found in broad-brush treatments. This is important for Hoffman, because he needs to establish that within any reasonably coherent area (although not the country as a whole) the rural economy essentially moved in concert, with rents, wages and prices following similar trends at all levels. However inward-looking villages may have been, they were integrated into much broader systems. On balance I am persuaded by the general argument, which is supported throughout the text by instances where different indices converge as the theory requires. At the same time I think Hoffman may have pushed his claims a little too far, and that some more archaic or customary features of rural society do need to be taken into account; some of the possible difficulties will emerge later.

The greatest problem in establishing a statistical model for pre-revolutionary agriculture is the lack of reliable figures for either production per hectare or labour inputs. Since these are the biggest variables involved, historians have naturally treated all estimates with extreme scepticism, whether they have been confronted with the optimistic claims of Jean-Claude Toutain or the deeply pessimistic critiques of Michel Morineau. Hoffman seeks a way round these insuperable difficulties, by concentrating on total factor productivity; essentially he seeks to establish the overall figure first, rather than trying to aggregate the unknowable. The crucial operation relies on taking long series of leases (the great majority being on rack rents for nine-year periods), then deflating the annual rents against suitable price series to produce an index of productivity. On the face of things this sounds a fairly straightforward, if very laborious, technique. To be sure, the results are less revealing than if we could reconstruct inputs and outputs more precisely. To give a crude example, a 5% increase in total factor productivity might represent two very different scenarios. In the first a 10% increase in labour inputs would raise yields per hectare by 10%, which would eventually translate into a 5% increase in rent. In the second yields would remain unchanged, but a 20% drop in labour inputs would boost profits and rents by the same 5%. The figures given here are of course quite arbitrary, and not necessarily consistent. The first pattern would presumably boost the local economy somewhat, whereas the other might drive some inhabitants into temporary or permanent exile.

How far can we assume that rents are a fair reflection of productivity? Perhaps the most extensive previous discussion of agricultural production in early modern France, that by Hugues Neveux for the Cambrésis, suggested that they could not really serve in this capacity. This was because the north-eastern border region was dominated by the custom of *mauvais gré*, essentially a 'market ring' system under which the large tenants refused to bid against one another when tenancies were renewed. Hoffman goes to great lengths to check for signs of similar patterns in the Paris basin, concluding that while landlords were cautious about replacing satisfactory tenants, there is no evidence for overtly restrictive practices of this type. He contends that over the long run the canons of *Nôtre-Dame*, like most other landlords, pushed up their rents to the maximum the market could stand. At the same time he recognizes that there were important shorter-term fluctuations. Broadly speaking, for several decades after the Hundred Years War farmers enjoyed highly favourable terms, as they brought land back under the plough; this was the crucial period for the formation of the great dynasties of *laboureurs* who came to dominate the rural scene. The sixteenth century was a period of rising rents, until the Wars of Religion brought a sharp check, and the subsequent recovery was snuffed out by Richelieu's war taxation. The later seventeenth century saw rents held at unwisely high levels, provoking a rash of bankruptcies. The landlords reacted by giving preference to creditworthy tenants, thus enabling richer *laboureurs* to consolidate their position while paying rather lower rents. A final swing from the middle of the eighteenth century involved a new series of rent rises, recouping previous gains by the

tenants. These shifts were all quite modest in statistical terms; while they could make a big difference to the viability of a tenant, they do not alter TFP very far.

A vital step in the argument is therefore that while rents were not absolutely tied to productivity, they only oscillated within quite small margins, because landlords kept a beady eye on the situation. They were determined to extract the highest rents they reasonably could, yet generally moderated their demands to avoid the very costly process of replacing tenants who abandoned their leases before term. The evidence marshalled here certainly suggests that this is much the most plausible view. One may also agree that the landlords had perfectly rational motives for accepting rather lower rents from large tenants, rather than breaking their properties up into small units whose rent per hectare would have been higher. Here Hoffman does slip into an uncharacteristically simplistic argument, however, couched in terms of a risk premium. While this is surely an element in the situation, it may be more a description than a true explanation. The differential between rents on small and large holdings is such a striking and widespread feature of the French agrarian world that it is disappointing to find it treated in so cursory a manner. Hoffman only gives serious consideration to two alternative explanations, both of which he dismisses on what appear to be very sound grounds. The first is combinations by large tenants to hold rents down, the second land hunger on the part of small peasants. The former has already been discussed; the latter is ruled out because land prices do not show the same gap, nor do rents on small plots correlate at all with population density.

Here I would argue for an alternative possibility, which only receives a glancing mention in the book, although other aspects of it are very well discussed. The situation identified here is one which raises some very acute questions about how the enormous rural population of France actually managed to exist through the year. Hoffman convincingly lays heavy emphasis on the difficulties farmers experienced in getting satisfactory performance from their workforce, and on their preference for employing a minimum number of trusted servants. He also points to the huge difference between the British and French situations by the late eighteenth century, with only 40% of the population apparently employed in the agricultural sector in the first, as against 60% in the second. What he does not consider is a widely-recognised consequence of these facts, which was the prevalence of underemployment, particularly on a seasonal basis. As a result market calculations must be modified in one very important respect, for when peasants worked on plots they owned or rented they were not usually foregoing paid employment elsewhere. So long as labour inputs came from the family itself they were effectively free over most of the year. This fits very well with the observed picture in which relatively high rents were paid for small parcels; big tenants who wanted such pieces of land, presumably for reasons of convenience such as linking other holdings, had to accept the market rates dictated by the mass of small peasants. On the other hand few peasants had much capital, so sale prices for such land were not inflated in the same way.

If this analysis is anywhere near the truth then it raises some other questions. It becomes important to know what proportion of the land was worked in large farms and in small units because we may actually be looking at two subtly differentiated agricultural systems, even where land had not been enclosed or concentrated in blocks, so that *laboueurs* and poor peasants held and worked adjoining strips. The large tenants would have had better access to manure, plough teams, storage facilities, and perhaps other facilities. They were however economical with labour, except at harvest time, whereas their poor neighbours usually had a surplus of family labour power. The latter therefore had a very powerful incentive to practice labour-intensive agriculture, even if only in the minimum form of frequent weeding. It remains perfectly plausible to suppose that yields would have been fairly similar under both systems; given the precondition of a large rural population, small plots may actually be the optimum solution in many circumstances, capable of remarkably high yields per hectare. The downside to peasant production of this type is that it is inefficient in labour usage, and therefore produces only a small surplus for the market. The weakness of TFP based on rent levels as a way of evaluating the performance of such peasants is, I think, that it is so hard to assign a value to labour inputs when for most of the year there was such a wide gap between supply and demand on the labour market. Rural industry - particularly textile manufacture - does of course complicate the picture, although it is notoriously hard to measure its economic impact with any great accuracy.

My own suspicion - and it can hardly be more - is that the productivity of small peasant holdings was

already high in the sixteenth century, but that in areas of cereal monoculture like the Ile-de-France it would only have grown very slowly after then, or perhaps stagnated. If this were the case, then Hoffman's claims for a 40% growth in TFP over his period might need to be scaled down somewhat, over the agrarian region around Paris as a whole, despite the advances made on large farms. In other parts of the country, where he posits a static performance, the balance might actually be the other way. Areas of polyculture, where sharecropping was the normal practice on larger units, may well have been more favourable to small peasant producers, who could readily exploit a range of labour-intensive crops. Again this would not have transformed the picture, so there is little reason to doubt Hoffman's pessimistic view of TFP trends in these regions, but total yields per hectare may have been a little more mobile. Sharecropping is another interesting problem in itself. The author offers us a very plausible account of why this practice could make economic sense, even if it was generally a second- best to proper leasehold tenures. He is surely correct to argue that sharecropping was a consequence of existing economic backwardness rather than its cause. Yet here too there may be more to be said. For all the ingenuity with which modern development economists have demonstrated its viability in some conditions, sharecropping as actually practised in much of ancien régime France looks like a dismally regressive system, far too harsh on the wretched tenants to encourage better farming practices. Moreover a phenomenon generated by backwardness may also form a new barrier to improvement, helping to perpetuate the conditions from which it arose.

The nature of the labour market raises questions about another important element in some of Hoffman's arguments, his claim that rural and urban wage rates were generally similar. This may well be true, but the data adduced to prove the point are far from conclusive, because they relate to the harvest period. Here was the great exception, the period when virtually everyone could find work, and when large numbers of town and city dwellers migrated temporarily into the surrounding countryside. In such a situation we would naturally expect wages to reach urban levels; the need to attract labour may also have meant that payments in kind, through the provision of food and drink, formed a significant addition to costs. What we cannot reasonably do, without supporting evidence, is conclude that similar levels prevailed through the rest of the year, when market factors should really have made labour cheaper. No doubt élite groups such as threshers were well paid for their backbreaking toil, but easier tasks may have been another matter, not least when they could be performed by women. Neither gender nor specialisation within the rural proletariat gets much of a place in Hoffman's picture; although this is perfectly understandable, given the limitations of his chosen sources, there are some serious issues lurking here. Perhaps this links up with an instinctive feeling that while quantitative investigations of this type are essential, they will only become wholly convincing when integrated with a microscopic analysis of agricultural practice. Such a global view might require us to match Hoffman's fine book not just with the magisterial recent thesis by Jean-Marc Moriceau on *Les fermiers de l'Ile-de-France*, but also with a study we still do not really possess, which would carry the *longue durée* into the village community and the lives of its humbler members.

I have left too little space for a proper discussion of some of the major conclusions, concentrating on what are arguably lesser matters where I saw problems or the need for more research. What I find wholly convincing is Hoffman's vision of French agriculture as far more varied and potentially successful than previous orthodoxies have implied. If the regional divergences he identifies create as many problems for the historian as they solve, that seems to me one of the merits of a truly important book. As he suggests, if the Parisian market could stimulate rates of growth in its hinterland which often matched those in England, then one cannot readily argue for some inherent features of French rural society which compelled general economic stagnation in France. I think the jury might still be out on another important claim, that if these 'advanced' regions of France took three centuries to match what English agriculture achieved in two, the essential cause was disruption by war and royal fiscality. War damage could certainly be severe, but there is an argument that rural societies of this type bounced back remarkably quickly from such disasters. If fiscality must be a prime suspect, it is still questionable whether it varied enough over time to explain the fluctuations in TFP. Nevertheless Hoffman must be right to assert that the agrarian economy can only be understood as part of a larger whole, and that it was not simply a drag on a more progressive France signalling wildly to be let out. One implication might well be that the ancien régime itself, both governmental and social, was a large part of the problem, which should not be blamed on the rural

populations that régime exploited and neglected. There is much more that could be said, and many other aspects of the book which deserve proper attention. *Growth in a Traditional Society* is a major achievement, a truly original study on a fundamental topic; it deserves both widespread acclaim and that equally significant accolade, a vigorous debate.

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