REVIEWS OF BOOKS

I. Bohstedt, *Riots and Community Politics in England and Wales 1790 - 1810*, Cambridge, Harvard University Press, 1983.

This monograph is a valuable addition to our understanding of riots and their relationship to evolving English Society. Riots were tactical weapons in the hands of the not-so-powerful against established interests, in Bohstedt's view, and they were used to achieve limited, concrete goals. Conventions understood by both sides guided the course of riots, and ameliatory programmes were often undertaken by community leaders to dampen the force of riots. Officials regularly increased welfare assistance in the face of riot threats, and rioters apparently recognized that violent attacks on officials or wanton destruction of property would force a punitive response by the community. Riots, moreover, were shaped by the social organization of the community: some communities burst into riot repeatedly; others with much the same provocation rarely did.

In short, Bohsted offers us a sophisticated analysis of early modern English riots; he documents his analysis with persuasive evidence from a variety of original and secondary sources; and he comes up with an authoritative account of English riots during the transition from a pre-industrial to an industrial social organization. Bohstedt's accomplishment is impressive, and his work will be used and cited by scholars in this area for years to come.

From Bohstedt's viewpoint the conventional thesis that riots are a rational response to intolerable grievances is at best a partial explanation. He notes that food riots were by no means consistently associated with rising wheat prices, and after a careful statistical study he shows that only 17 percent of the variance in food riots can be attributed to wheat price fluctuations. Rather, he suggests that riots are likely where external threats (e.g., rises in food prices) galvanize the lower classes into collective action and evoke a sympathetic attitude among the middling classes. These external threats, moreover, must be concrete, immediate inssues that can be resolved at least temporarily by direct, concerted action. And finally, these issues must be ambiguous in that rioters, though acting illegally, are nevertheless seen as justified in their actions. When these several conditions were ripe, the use of riots to gain immediate political or economic ends were a distinct possibility.

The protocol of riots dictated that the rioters limit their actions to justifiable ends and that the authorities moderate their response, lest one side or the other be provoked to extreme, disruptive behaviour. Instead of solving the immediate problem, such riots simply inflamed the community, and in the end, defeated the purposes of both sides.

Bohstedt's concept of a riot protocol is intriguing as a type of social control and no doubt basically valid, but further documentation is badly needed to flesh out its dimensions.

In the course of his monograph, Bohstedt examined several different types of riot: but primarily food and labour riots, and he found that food riots declined rapidly after 1800. Labour riots, on the other hand, continued unabated well into the nineteenth century. Food riots became rare because wheat prices began to fall in the early nineteenth century, and with a developing national market in food stuffs, the control of local prices through direct action became impossible. Food riots simply drove suppliers to other markets where the prices were better and the threat of violence less. Labour riots continued, however, because it was possible to affect, at least temporarily, wage rates through riots. The labour market was less well developed. As industrialization proceeded, however, a national labour market emerged and the opportunity to influence local wages similarly disappeared.

Curiously, though, some communities experienced periodic riots, while others in the same region had few if any. The appearance of national markets affected most communities similarly, and market conditions, therefore, were only an indirect factor in food or labour riots. Devon had many small and well contained food riots in the smaller towns, but Plymouth, as the fastest growing town in Devon, experienced several riots that degenerated into violent, distructive confrontations.

Devon had many smaller communities in which the social bonds within the lower orders and between them and established leaders were still strong. In such communities neither side was likely to exceed accepted limits in rioting, and concerted action to achieve immediate goals was possible, without devestating the town. In Plymouth, however, the bonds in both directions were weaker, and riots more quickly got out of hand and became violent and destructive.

At the same time, rural areas had few riots despite severely depressed economic conditions. Farm workers were particularly transient with few social bonds to weld them into an effective force, and they were particularly dependent both for wages and food supplies on the gentry. Thus, it was difficult to mobilize those who were suffering, and established groups who could relieve their suffering were only likely to be more punitive in the face of a direct confrontation. Though their suffering was real, the opportunity to riot among farm workers was not there, and few riots occurred.

Occasionally, however, the ferment grew to explosive proportions, and a riot erupted in farming areas. When it did, it often became extreme and resulted in severe countermeasures by the authorities. The thin web of social bonds both limited the number of riots and made aggravated violence more likely when they did occur. Social organization channels the response to grievance: thwarting collective action in rural areas; limiting it in small marketing centres; and setting it free in large, booming port cities. Scholars, studying the sources of social disorder, need to assess its social as well as its economic basis in future.

Bohstedt also examined the context of labour riots in nineteenth century England. They have usually been regarded as defensive attempts to reclaim wage levels that have been eroded under pressure from free labour. Bohstedt suggests, however, that labour riots were one of several tactics used by workers to win new, higher rates and not simply attempts to reclaim former wage levels. He found, however, little interest among labourers in forestalling technological change, as some have maintained.

True to his thesis, Bohstedt sees labour riots as particularly likely where working class customs fostered strong links among workers with similar grievances. In such settings a " moral community " was established that permitted labour unity and collective action on communal grievances. When successful, a " moral economy " and not a free economy was enforced. As a national labour market developed, however, the moral economy withered and labour riots dwindled. Labour riots depend as much on the social context as on the provocation, and to study the nature of the provocation only is to miss half the picture.

Bohstedt has given us a more complete understanding of the sources of civil disorder, and in so doing he has advanced our knowledge of riots considerably. His work, morever, has considerable application. Although the complaints have changed, and the social organization of today's communities differs from that of early modern English communities, the thesis that well organized communities have limited riots, while disorganized communities have destructive riots, is still probably valid.

There are, of course, other types of violent civil disorder: crime and revolution, for example, and Bohstedt has laid the foundation for a careful analysis of the broader relationships between these several forms of violent disorder. Limited riots probably bear little relationship to crime since the cohesive structure of small communities is a prerequisite for the former but not the latter. Destructive riots, on the other hand, tend to occur in weakly organized communities and probably are accompanied, therefore, by considerable crime both before and after.

The relation of riots to revolutions, of course, is much more complex, which Bohstedt freely acknowledges. But he inclines to the view that riots are more a tactic for immediate gain and *not* for radical reorganization of society. Riots, in other words, do not often foreshadow political revolution.

This is exactly the kind of study that historians can feel a certain pride in. It is based on a thorough study of historical materials; it is guided by a sophisticated thesis; and it has bearing on current theoretical and practical issues. Bohstedt is to be applauded for the fine way he has combined and achieved these several goals.

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I. M. DRUMMOND, *The Floating Pound and the Sterling Area, 1931-1939*, New York, London, and Sydney, Cambridge University Press, 1981, pp. vii, 308.

This study focuses on the decisions made by governments of the British Empire and of other countries, during the period until the outbreak of war, in response to Britain's suspension of gold convertibility of the pound on 21 September 1931. Drawing on documentary material in a host of archives, the private papers and memoirs of participants, and secondary sources, the author provides a narrative of the process leading to the formulation of exchange rate policies by the governments he is concerned with.

The early chapters deal with developments in the overseas sterling area, India in particular, and the Dominions. The middle chapters discuss the background to, and the policy pursued by the U.K. authorities at, the World Monetary and Economic Conference that met in London in June 1933. Drummond describes the grounds for the mutual suspicions on the one hand of the British, the French, and the Americans, and on the other hand, of the British and Empire countries. It is no wonder that the conference turned out to be a fiasco.

The final chapters deal with the financial diplomacy that culminated in the tripartite agreement and its aftermath. Until June 1936 Britain resisted pressure to peg to the dollar or gold. Drummond denies that anything changed in the management of sterling with the simultaneous issue of individual declarations by Britain, France, and the United States upon the devaluation of the franc on 26 September 1936. Britain made no promise about its exchange rate other than to refrain from retaliating against the franc. If the pound nevertheless varied little relative to the dollar until mid-1938, the support for the exchange rate that London provided owed less to the influence of the tripartite agreement and more to concern to prevent a flight from sterling or a dollar devaluation against gold. As capital outflows mounted thereafter, pressure on the pound forced the authorities to remove the peg. Drummond finds no evidence of "consciously organized cooperation in matters of substance or harmony and consistency in monetary policy" (p. 249) of the three countries, and precious little to show in the way of trade agreements and relaxations of quotas that can be connected with the agreement. He does not regard the agreement as a precursor of Bretton Woods because exchange rates were far from stable, and the United States were actively concerned about the dollar price of sterling and the franc, unlike its passive behaviour under the postwar exchange rate arrangements.

Drummond refutes the view that "competitive devaluation" characterized the world monetary system of the 1930s. He acknowledges that sterling floated dirtily but denies that it, the franc before 1937 and currencies of smaller European countries were devalued competitively. They were devalued because of market forces. Similarly, exchange controls and multiple rates were introduced by Germany in response to the weakness of its balance of payments and the expansionary policies the government pursued after 1933, while the yen left gold and fluctuated for the same reason as the pound. Drummond concludes: "It was the Roosevelt administration, alone among major powers and almost alone in the world, that deliberately depreciated its currency against gold, thus winning an advantage in world trade even though its balance of payments was already in surplus on current account and even though it had virtually ceased to lend abroad. Further, it was the Roosevelt administration that insisted on retaining the right to make further devaluations, not just in 1934 but for the rest of the decade" (p. 257). I agree with Drummond's conclusion, but he leaves himself open to the charge of crankiness in asserting that the competitive devaluation view is "an echo of American economists' guilt: it was their country, not Britain, that deliberately destabilized the set of exchange rates."

With one notable exception, the author is not reticent in stating his viewpoint on the matters dealt with in the study. In discussing fears of inflation in South Africa before it pegged to sterling in February 1933, Drummond notes that the Reserve Bank would accumulate gold and foreign exchange if the South African pound were undervalued relative to gold currencies and sterling. The commercial banks would then inevitably hold increased high-powered money, as was the case after December 1932. Drummond comments: "To those who like the quantity theory of money, this extra money would seem bound to raise prices, and we may suppose that to some extent it did so" (p. 86).

The study summarizes or quotes the views of an enormous cast of political leaders, central bank personnel, advisers, consultants, and experts, so it is a great convenience that the index identifies the individuals named in the text should the reader miss a statement noting the roles they played. The one name not listed in the index whose role I am not familiar with is referred to in the text as Lochhead (p. 208), one of Henry C. Morgenthau's entourage.

Although it was not Drummond's intention to explain the actual movement of the sterling exchange rate from the time the British freed the pound from gold convertibility until the outbreak of the war, his discussion of the management of the rate would have been enhanced by a chart showing the sterling-dollar and sterling-franc exchange rates.

Apart from these quibbles, the study is a scholarly investigation that is worth reading even when the author covers ground that others have surveyed earlier. He corrects his predecessors on the basis of the material he has examined that they did not have and takes issue with them when he finds their interpretations faulty.

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H. I. Dutton-J. E. King, *Ten Per Cent and No Surrender* " *The Preston Strike, 1853-1854* Cambridge: Cambridge University Press. 1981. 274 pp.

This is a puzzling book. A detailed history of a long lockout-strike in the Lancashire textile city made famous in the Charles Dickens' novel *Hard Times*, the book is meticulously, almost tediously, documented. The authors provide 1195 notes for only 208 pages of text, nearly 6 per page and over 100 per eleven chapters and epilogue. Yet for all their attention to minute detail and extensive documentation, the authors conclude that the Preston strike of 1853-1854 " was not of outstanding importance. Neither for Preston nor for the cotton industry as a whole does the dispute mark a crucial turning point (p. 203)." And they add that " The Preston strike was no Rebecca, no Peterloo, no Tolpuddle; there were no riots, no deaths, no martyrs (pp. 195)." What, then, justifies the attention which they lavish on Preston and the events of 1853-1854? A careful reading of the book offers no clear answer to that question.

Indeed the book is peculiar in other ways as well. It is neither narrative nor analytical history. Rather than follow the conflict in Preston clearly from start to finish, the authors jump randomly from topic to topic and geographical place to place. A chapter on the strike might be followed by one on Lord Palmerston, the Press, the town government, or " Guardians of the Poor " (Ch. 7). The scene shifts unexpectedly from Preston to Stockport or Burnley or Wigan or Manchester or even to London. Interpretation rarely raises its head in a book based largely on reports in the local and national press, and when it does, the authors' interpretations are not necessarily derived from their data. Messrs. Dutton and King criticize Dickens for creating caricatures in the novelist's cast of Prestonians. Yet our economists turned historians rarely bring to life the participants in the long lockout-strike despite their attempts to humanize the local working-class leaders, George Colwell and Mortimer Grimshaw, "the thunderer of Lancashire." Moreover, one never understands clearly how Preston's workers were able to survive and maintain their solidarity over eight months, the more so since the vast majority of strikers, women workers, rarely appear as participants in the struggle. The authors' treatment of the "woman question" is especially troubling. They observe that 11,200 of the 18,000 estimated strikers were female, almost two out of every three. Yet they note that "the appearance of a woman on the platform was evidently a rare event (p. 51)," that women played a subordinate role in the entire ten per cent campaign, and that the fundamental ambition of both Preston's male and female workers was to secure for women the right *not* to work. Were these "marginal" women textile workers the wives and daughters of male spinners and weavers, or were their husband/fathers employed in other local trades? Dutton and King never address or answer such questions.

Is there anything, then, to learn from reading this book? Yes, the authors provide a number of revelations about the beliefs and behaviour of workers and employers in Victorian England. They make abundantly clear that although the conflict manifestly concerned only the rate of pay, its latent causes were more crucial and contentious. For King and Dutton, and I believe them right, the struggle in Preston was over the question of mastery; employers refused to surrender to unions any right to influence factory policies and local public officials saw in the strikers' claims a challenge to the prerogative of the bourgeoisie to rule society. The book also reveals how widespread working-class solidarity was in the England of the early 1850s. Preston's workers were sustained materially in their struggle by financial contributions from workers elsewhere in Lancashire and also Yorkshire, Derbyshire, and London. Only such financial assistance from fellow workers enabled Preston's strikers to persist for over half a year. King and Dutton also reveal the dimensions of class warfare in Victorian England but their contending classes are not monoliths. Instead, a substantial part of the local and national bourgeoisie seeks to act as mediators and, on occasion, even defends the strikers against unscrupulous, exploitative masters. And employers turn to "Knobsticks" (strikebreakers) to defeat Preston's workers.

Where Dutton and King become more venturesome in their interpretations I find it harder to follow them, however much I may sympathize with their conclusions. They try to use the Preston strike to prove that English workers of the 1850s had not forsaken a radical, revolutionary past, nor assimilated the values of the ruling class, nor accepted the hegemony of the bourgeosie. Yes, violence and clarion calls for revolution may have been absent from the struggle in Preston, but, according to Dutton and King, not because workers had assimilated into "a culture that presupposed middle-class pre-eminence" or because they had dampened their aspirations. Dutton and King instead assert that Preston's workers "rejected the values and norms of the ruling class...but...were forced by the realities of the situation in which they found themselves to act as if they accepted the relations of dominance which those values upheld (p. 55)". Much truth may inhere in that conclusion, but it does not follow logically or necessarily from the evidence or narrative of the book. Whatever the larger meaning or significance of the Preston strike of 1853-1854, Dutton and King have made it unnecessary for others ever again to have to go over the same ground.

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R. B. EKELUND JR. - R. D. TOLLISON, *Mercantilism as a Rent-Seeking Society*; College Station, Texas: Texas A & M University Press, 1981.

Once again a large gun has been brought up to batter down the walls of misunderstanding surrounding early modern economic policy. Ekelund and Tollison have wheeled into position a new paradigm which is to be substituted for the "power vs. plenty" paradigm prevailing since the days of Heckscher and Viner. The new paradigm has as its central focus the phenomenon of rent seeking. Instead of treating rent seeking "as a manifestation of some greater mercantile objective, such as the balance of trade, as do the historians," they say, rent seeking should be seen as "a central element in an interpretation of the period." We should regard the essence of mercantilism as a situation in which society's timeless propensity for rent seeking can avail itself of a ruler's new and relatively unchecked ability to create artificial scarcities. If we do, they claim, we can make more sense out of changes in and contrasts between XVIIth and XVIIIth century French and English economic policy. Instead of pitying mercantilist writers and statesmen for irrational fixations on balance-of-payment and specie-accumulation problems, we should recognize that when ever-penurious kings used their new powers, and when producers and merchants purchased these favours in order to capture some consumers' surplus, all parties were acting quite rationally.

Ekelund and Tollison begin their modelling of a "rent-seeking society" by defining economic rent as a receipt in excess of the opportunity cost of a resource. Rent seekers can proceed by bidding for royal favours: or they can compete to join favoured groups (regulated companies, guilds, monopolies); or they can obtain positions in favoured branches of state officialdom (sinecures, inflated wages, tax privileges). Given the absence of adequate competition and information in these years, rents will not be competed away. The monarch as rent seeker cannot obtain all potential profits, since he has to take certain political considerations as well as some consumer interests into account. But he will try to make the crown as much of a successful " rent-seeking leviathan " as he can, since — because of the low level of tax enforcement then available — revenues from such rents tend to be higher and easier to obtain than from taxes.

In the first of their central chapters, Ekelund and Tollison gives us a decidedly engaging explanation for the relative decline of internal (not foreign) regulation in late XVIIth and XVIIIth century England. " The historians," we are told, have given the main credit for this phenomenon to the intrinsic persuasiveness of anti-mercantilist arguments — the sheer intellectual superiority and sounder legal basis of the (internal) free enterprise position — as interpreted by English common law courts. But the logic of economics affords a more reliable concept, they believe, one that springs to mind when we observe how, after the 1640s, " a form of representative democracy shifted the locus of rent-seeking activity (from the king) to new forms " — that is, to common law courts and to Parliament. The new and confusing competition for monopoly-granting power among king, royal courts, Parliament, and common law courts raised costs and uncertainties facing rent seekers, who thereupon could be expected to reduce their " demand " for " regulatory supply."

Our authors also dispute Heckscher on the motivation of the common law courts and Parliament; these factions, they believe, were acting not out of objective convictions but because they were themselves competing for rents, trying to arrogate monopoly-creating powers to themselves: " it was a question of which organ of government (royal or parliamentary) had the authority to collect rents." But when Parliament finally won, the costs facing rent seekers were too high, now that so many interest groups and even individual legislators had to be taken into consideration — rather than the previous near-monopoly enjoyed by the king. Gradually the English enterprisers (at least so far as internal transactions were concerned) fell out of the habit of bidding for rents. This helps explain English (relative) free enterprise just before the Industrial Revolution.

While " the rise of Parliament (meant) the fall of mercantilism " in England, in France the absence of a comparable parliament and the crown's " absolutist " tax powers, plus France's huge and quite venal fiscal and magisterial bureaucracy, meant the spread and rigidification of mercantilism, with all the bad effects of these developments on economic growth. Furthermore, the collusion of the French king and interest groups in rent seeking, by shifting emphasis from cost reducing to high cost, high quality production, warped French technological development, lowered incentives for investment, and expanded the grip of the guilds on production. Why the French emphasis on luxury crafts? Not because of "aristocratic tastes," say our authors, but because luxuries were produced in cities and towns, while more basic products came from the countryside, where rents were harder to obtain. In such a view, it is not the impact of Colbert's idiosyncratic economic concepts that counted (according to Charles Cole's "monolithic" interpretation) but rather rent seeking, so assiduously pursued by so many elements of French society. Why was calico production inhibited for so long? Not because of "a mindless attack on innovations," as Cole and Heckscher have argued, but because the introduction of yet another textile product would raise the costs of "regulation" (read: "artificial scarcities") and thus reduce profits from rents.

These are large enough claims: the revision of our interpretation of preindustrial policy in its fundamentals and in its details. But there is more: the authors believe their views shed light on "neo-mercantilism", that is, on late industrial economic policy and even on the present. At this point, some of us might be excused for feeling justified in throwing out the baby with the bath water. "The historians" might observe that rather than giving a "monolithic" interpretation of mercantilism Charles Cole gave a flabbily eclectic one; that for the most part their refutations of Heckscher are engineered out of long quotations from Heckscher; that in spite of the central place allotted by them to France there are no works in French in references or bibliography; that they do not seem aware of Charles Wilson's important essay on mercantilism in Volume IV of the *Cambridge Economic History of Europe*; that, in spite of stipulating they are concerned only with internal regulation, their failure to deal, at least in some minimal way, with the international economy in their model is confusing, since foreign regulation in post-1649 England was still a powerful factor (why do they treat at length the English wool trade?); that after a one-phrase mention of the Poor Laws they drop the subject, thereby ignoring those of us who feel that regulating the labour supply was a key factor in mercantilism; and that they have handled possible Marxian counter-explanations and even Immanuel Wallerstein & Co. by simply ignoring them. The central thesis of the book, however, is very attractive and deserves careful attention from all of us interested in the preindustrial economy, whatever our disciplinary perspective.

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D. GALENSON, *White Servitude in Colonial America, an Economic Analysis*, Cambridge, Cambridge University Press, 1981, pp. 303.

A major problem which confronted European colonising efforts in the Americas almost from their discovery was the relative shortage of labour.

In the absence of indigenous sources, the solution lay in immigration. In the past fifteen years there has been a wide-ranging debate about the dimensions, direction and chronology of the Atlantic slave trade but less attention has been paid to the white migration, perhaps understandably since the slave movement was a flood and the white a trickle by comparison. More than thirty years ago Abbot Emerson Smith wrote *Colonists in bondage: white servitude and convict labor in America 1607-1776* (University of North Carolina, 1947), a study divided into three parts dealing in turn with the trade in servants, penal transportation and the servant in the plantations. In the past decade or so renewed interest in white migration has been shown by Mildred Campbell, James Horn, David Souden and particularly David Galenson who has now followed his journal articles by turning his PhD thesis into this book. As its title indicates, it is both narrower in scope and different in approach from Smith's book. While Smith considered the range of white forced migration, Galenson concentrates his attention on the indentured servant. Further, he largely confines his study to English migration and to a shorter period. By and large his analysis is based on six sets of records compiled between 1654 and 1775: for Bristol 1654-79, 1684-6 (dealing with 10,632 servants), London 1683-6 (878 servants), Middlesex 1683-4 (812 servants), Liverpool 1697-1707 (1443 servants), London 1718-40, 1749-59 (3187 servants) and for the whole of Great Britain for the period immediately before the Revolution, December 1773 - March 1776 (3709 servants). Thus the geographical coverage is patchy and the chronological coverage incomplete. Apart from the last set of returns, there are no direct records for much of Britain while records are missing for about 40 of the 120 years under discussion. It is estimated that the 20,657 servants considered here form between 5 and 7 per cent of all migrant indentured labour. About the use of these records as the basis of generalisation about white servitude in America, two caveats must be entered. First, there is no consideration here (though Galenson has dealt with the matter elsewhere) of the representativeness of this particular sample. While, further, a considerable number of servants — and quite probably the majority — sailed without indentures which they were assigned according to the 'custom of the country', the terms varying with the laws enacted by the various colonial legislatures. Barbados (17 per cent), Jamaica (11 per cent) and Nevis (in the later seventeenth century) were the main West Indian destinations, while Virginia, Maryland and Pennsylvania (where the eighteenth-century information is slight) were the chief mainland colonies to which the indentured servants went. So much is common ground with other discussants. But then differences emerge. Analysing the seasonality in greater detail, Galenson shows that migration was concentrated in late summer and autumn though perhaps he exaggerates his difference with Smith.

Proceeding then to analyse the characteristics of indentured servants by age, sex, occupation and literacy, Galenson reports that if the population for which records are available are typical of the whole, then at least three-quarters of the migrant servants were male, mostly aged 15 to 25, 'drawn in significant numbers from all levels of a broad segment of English society bounded at one end by the gentry and at the other by the paupers'. On the question of skill, Galenson has had a running battle with Mildred Campbell concerning the social origins of these early Americans, as a result of which he continues to maintain that servants came almost equally from four groups: artisans, farmers, labourers and young men without occupations who were probably unskilled. The difference turns on the interpretation of the returns which do not indicate occupations: Campbell assumed that nil returns were a random sample of the whole while Galenson maintains that those for whom no occupation is indicated were predominantly unskilled.

To explain the changing contribution which indentured servants and black slaves made to the colonial labour force in America and the West Indies, Galenson distinguishes three phases. In the first servants did both skilled and field work, in the second slaves displaced servants in the unskilled jobs while in the third creole artisans servants in the skilled jobs. By the 1770s the West Indies had entered the third phase while the Chesapeake colonies were moving out of the second. Thus Galenson provides an explanation for the virtual disappearance of indentured servants from the American colonies on economic grounds in terms of price. On this proposition perhaps two comments are in order. First, the price data are slight and not always consistent with Galenson's conclusions. And then, to what extent can it be said that the decisions of planters to shift to the employment of slaves was made purely on such grounds? Were they not also affected by the unruly behaviour of the servants, by the fact, as Edmund Morgan suggests, that servants had become a social problem? It may also well be argued that Galenson's portrayal of the recruitment position in Britain exaggerates the ability both of servants to bargain over terms and their awareness of the market situation in other English ports.

To complete his presentation, Galenson provides in his appendices new decennial estimates of white and black migration by colony 1650–1780 and a tabulation of the colonial destinations of his sample of indentured servants. Though the second half of this book is more difficult for non-economists than the first, it clearly makes an important contribution to an understanding of a substantial group of migrants to the British American colonies. By his analysis, Galenson has added significantly to our knowledge. But because not all his conclusions will gain universal consent, he has also set the scene for a renewed and better informed debate about the issues involved.

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R. S. Gottfried, *The Black Death: Natural and Human Disaster in Medieval Europe*, New York; The Free Press: London, Collier Macmillan: 1983, XVII, 203 pp.

Professor Gottfried's book is a well-written essay for the non-specialist reader which presents the theses that the Black Death was a powerful and pervasive influence in transforming medieval into modern Europe. The book's primary emphasis falls on demographic effect, arguing that plague's monstrous mortalities not only shattered the established social, political, and economic institutions of an increasingly moribund traditional society, but that its repeated recurrences with continued high mortalities over a century and a half made it the major contributor to Europe's radical depopulation in the fourteenth and fifteenth centuries. It was that depopulation, according to Professor Gottfried, which not only enabled Europe to escape a 'third world' Malthusian trap of overpopulation and subsistence agriculture, but which opened new economic opportunities, forced the creation of new modes of thinking and acting, and altogether revolutionized Europe's cultural order. Such extraordinary results required a similarly extraordinary cause, and in the conclusion to his book Professor Gottfried informs us that the Black Death "should be ranked as the greatest biological-environmental event in history, and one of the major turning points of Western Civilization." (p. 163)

The very high valuation on plague's significance as an historical cause marks a return to a much older viewpoint, and largely ignores the argument that the horrors of epidemic bubonic plague created a vocabulary of terror which included vastly exaggerated mortality reports and ascribed a causal role to the disease which was actually much greater than could be demonstrated. Professor Gottfried takes the argument away from the endless (and ultimately irresolvable) debates over cultural and psychological issues to put it on the apparently firms ground of demonstrable social and economic fact as revealed by modern scholarship, his own included. With that base under the argument, he then feels free to discuss the plague's attitudinal and behavioural influences from the breakdown of traditional religious practices to technological change and a radically new view of time. The plague's effects strengthened centralizing political tendencies, weakened feudal relationships including serfdom, promoted machine production and an expansive commerce, and, in a variety of other ways, substantially advanced what were to become fundamental elements in modern European society. Though Professor Gottfried is careful to recognise prior influences or tendencies pointing in these directions, and to eschew simplistic, monocausal explanations, his fundamental position is that the plague's role was central and more significant than any other individual factor.

The innovative element in this book is the argument. There is little factual material which is new, and not all the new material is represented. This is not a balanced account which weighs different and opposed positions, but rather a well organized argument which presents a distinctive point of view It is an improvement on Philip Ziegler's earlier (1969) essay on the Black Death, if only because it includes material which has appeared since Ziegler wrote, and its interpretations are more sophisticated and comprehensive. On the other hand the author simply disregards some of the knottier problems his facts entail, and the uninformed reader will find little in notes or text to indicate how thin or treacherous the informational ice may be. Yet given the known uncertainty of mortality reports, the continued vagueness of information on the plague's incidence — how much of Europe and how many Europeans were actually infected — and the difficulty of separating plague's effects from those of other diseases, famine conditions, lawlessness, and war, there are pitfalls enough to trap even the wary reader. To have gone into these problems would have made the book longer, less dramatic, and more convincing. It also would have been more satisfying as history.

There is one other point to mention. Professor Gottfried treats the Black Death as a major influence on Europe's development. His detailed, analytic evidence, however, comes primarily from studies focussed on parts of Italy, France, and England, and the cultural consequences he describes seem most appropriate for those areas. There is relatively little in the book based on detailed analyses of southern Italy or Iberia, the Low Countries, Scandinavia, the Germanies and eastern Europe, the Balkans, or Muscovy and its borderlands. In fact, Professor Gottfried is able to tell us very little about the plague in most of Europe, yet he urges us to consider it a major turning point in the history of Western Civilization. Without raising the convoluted problem of who is 'western' (Poles and Lithuanians but not Russians? French and Dutch but not Germans? Swedes but not Spaniards?), it would seem obvious that the informational base for the book simply does not support speaking of the plague in medieval Europe. A more modest formula would have been more accurate, while more emphasis on the point, hinted but not developed, that plague's effects could be different in different areas, depending on the dominant characteristics of the region and its stage of cultural evolution, would have helped to correct the picture.

Taken as a whole, Professor Gottfried's book is probably worth reading for an up-to-date (but not definitive) summary of recent and traditional materials on the Black Death set in a modern historiographical context. It is a book which shows clearly how important it is to understand the role of disease in history, and it presents a strong case for special attention to the Black Death. The weakenesses in that case show some of the difficulties in studying disease and history, and so far as the Black Death is concerned, how much still remains to be done.

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D. B. GRIGG. *Population Growth and Agrarian Change: An Historical Perspective.* Cambridge: Cambridge University Press, 1980, Pp. XII, 340.

David Grigg's magisterial synthesis of current research on the relationship between demography and agrarian change delivers much more than its subtitle, "An historical perspective," proclaims. Not only is this survey a primer in European population growth since the year 1000, but also an attempt to theorize upon the causes, symptoms, and responses to population increase in a pre-industrial society. Beyond history, the lessons which Grigg draws from the European experience guide his narrative of contemporary problems in third world development. Grigg is neither historian, demographer, nor prophet, but a geographer whose earlier studies of world agricultural systems (1970 & 1974) and wide-ranging reading in the social sciences ground his work in a true comparative method of time and place. Before superlatives distract prospective readers, however, I must emphasize that Grigg's tightly reasoned and judiciously nuanced argument is not straightforward orthodoxy:

> it is difficult to argue that there was radical change in farming productivity in Western Europe before 1850, and thus difficult to see how agricultural progress could be a necessary pre-requisite to industrial revolution, as it is invariably assumed to be (p. 293).

This extraordinarily provocative thesis deserves a thoughtful hearing by all manner of economic historians and planners.

Grigg begins with a methodological inquiry into the meaning of overpopulation and defines the problem faced by agrarian societies in adjusting agriculture to changing numbers as one of standard of living. Three theories are tested throughout the book: (1) the Malthusian system, (2) optimum theory, and (3) rural underdevelopment models. Since Grigg finds little consensus for indentifying or measuring overpopulation among these theories, he presents a collection of verifiable symptoms (subdivision of farms, fragmentation, land values — rents — prices, landlessness, technology—labour supply, and land use), a number of possible ways to increase output (expanding the arable, increasing the frequency of cropping, shifting to higher yielding crops, technological advance, division of labour — regional specialization, and domestic industry — seasonal migration), and the alternative demographic adjustments to reduce population (fertility control, migration ,and mortality). Such a systematic outline of variables and their interaction typifies the thoroughness, yet simplicity and incisiveness of Grigg's approach to agrarian change.

Grigg's schematization continues as he divides European population history over the last millennium into three discreet periods of growth, separated by two of decline. Only England, France, the Netherlands, Ireland, Norway, and Sweden are reviewed to demonstrate how Malthus was justified for the earliest period (increase, 1000-1347 and decline, 1347-1450); how Malthus was refuted during the second period (increase, 1450–1600/50 and decline, 1600/50–1750); and what that implies for the present period of growth from 1750.

Readers should be alerted to the Northwestern European bias and to the exclusion of some important examples. Herlihy and Klapisch-Zuber (*Les Toscans et leurs familles* [1978]) on the 1427 *catasto* in Tuscany should most certainly have augmented the four earlier Herlihy citations. Gerard Delille (*Agricoltura e demografia nel regno di Napoli nei secoli XVIII e XIX* [1979]), although his research is much less publicized, has good evidence and theory to supplement Grigg's working hypotheses. Each national specialty can, no doubt, point out such omissions.

Cavils aside, the point of Grigg's meticulous overview is comparison between the three periods. By employing logistic curves to visualize population change, Grigg argues that "*rural* (sic) population of Western Europe repeated the pattern of stagnation or decline that had occurred in the fourteenth and seventeenth centuries, if for different reasons" (p. 292). Although output per head was maintained throughout this period of overall population increase, Grigg doubts that any increases per head were "spectacular" (p. 233). He argues that no significant increase in yields can be detected before 1830 because the slow and steady improvement in yields since the 1650's was dependent upon pre-industrial farming methods (manure, marling, and liming). With industrialized agricultural inputs (machinery, compound feed, and artificial fertilizer), for their part, only available after the 1820's and later, the real transformation of rural Europe only came about in the second half of the nineteenth century. History teaches us that world agricultural and demographic policy, thus, must be responsive to both production and demographic responses.

To understand Grigg's rejection of the generally accepted paradigm of a European agricultural revolution which fostered industrialization, we must give his measured reprise and cautious theorization its due. Grigg is keenly aware of the demographic limitations in data and, therefore, his exercise in economic logic is not meant to be quantitatively convincing with its 72 tables and 25 figures, nor authoritatively intimidating with its 37 pages of notes. He develops his intermediate conclusions not as strident fact, but as probable explanation by casting the rhetorical thrust of his book in the subjunctive mood. Here is no false modesty lulling us into believing some polemical position or distracting us from critical evaluation of his speculative hypotheses. Instead, Grigg attempts to assess overpopulation in light of two clearly stated operative assumptions: (1) the problem of overpopulation and agricultural productivity are not new, but subject to historical examination (p. 7), and (2) agricultural communities employ diverse strategies (either demographic or productive responses) to avoid the problems of overpopulation (p. 15). Grigg's review of the literature, then, does not resolve innumerable controversies with infallible acumen, but tackles each issue with workmanlike precision in a remarkably successful attempt to present the most probable, coherent explanations. Even if his macro-synthesis ultimately fails to persuade a skeptical audience, his succinct, micro-demographic summaries cannot fail to stimulate similar synthetic attempts from his readers.

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T. RAYCHAUDHURI - I. HABIB (editors). *The Cambridge Economic History of India Vol. I: c.1200 — c.1750.* C.U.P., Cambridge, 1982. Pp. XVI + 543.

Mention India to any well-read economic historian and you can expect to get a very standard set of questions reflecting a similarly standard body of writing. Aware or unaware, these scholars are drawing their interpretations of Indian history largely from materials written during India's long struggle for independence from Britain. These works contain not only a controversy about the effects of British rule but also, often implicitly, sometimes explicitly, one of several views of what India's economy was like before the beginning of the East India Company's rule. These arguments about the nature of (essentially) Mughal India fall into two broad categories. In the first, the wealth and magnificence of the Empire, its fine textiles, its superb swords are stressed to build a picture of a society at least as likely as Europe to have industrialized had not the English conquered it. In the second, the poverty of the masses, the low and stagnant level of productivity, the high costs of transport, the extortionate tendencies of the tax system are emphasized to argue that India was very unlikely to have undergone industrialization under any set of rulers.

With the publication of the *Cambridge Economic History of India Vol. I,* covering the period from 1200 to 1750, well-read economic historians have a new and much more sophisticated source from which to draw information on India before colonization by Europeans. While a substantial part of the information contained in these essays is not absolutely new, it has certainly not been conveniently available to the non-specialist. Valuable as the presentation of this information is, the greatest contribution of the CEHI may well be to encourage readers to seek more complex interpretations of India' economic history.

For instance, there is the matter of regional diversity. The CEHI consistently treats South India (always outside the Mughal domain), and Maharastra and the Deccan (often in conflict with the Mughals and only briefly conquered) separately. Assam is also given a chapter (inappropriately titled an appendix) of its own. As a result of this structure the reader is made aware of the very different forms of political, social, and economic structure in different regions. In chapters written by Burton Stein we learn that in South India, where Muslim penetration was quite minimal, temples were and have remained important actors in the agricultural economy. Funds contributed to them to provide offerings to the gods in perpetuity were frequently used to establish and improve irrigation systems. Part of the revenue from irrigated fields was then used to finance the offerings to the gods. In the north, perhaps because the Muslim conquests had destroyed many temples or, more likely, because temples had never had such functions in the Indo-Gangetic Plain, this source of investment in agriculture was lacking. Instead, what agricultural investment occured was done either privately or by government agents.

The various authors of the CEHI also have a general emphasis on determining *how* the economy functioned as well as what it produced. In an excellent chapter on non-agricultural production, Tapan Raychaudhuri devotes much of his attention to how production was organised and how increases in demand were met. While market transactions were many, Raychaudhuri argues that very few producers faced any competition as " the bulk of production was geared to the reciprocal arrangements of the rural caste organization " (295). Most of the remainder of production was either for the luxury markets of state officials and wealthy merchants or for the export trade where Indian producers were generally monopolists. This lack of competitive pressure was, according to Raychaudhuri, an important element in freeing India artisans from the need to actively seek either innovation or mechanization.

In chapters on agricultural production and agrarian relations and land revenue in the Mughal Empire, Irfan Habib gives similar attention to how production was organised and what effects demands by the state for revenue tended to have on the agrarian economy. Habib conclused that the Mughal system had " a tendency to subvert superior cultivation, while it simultaneously increased the distance between the rich and the poor in the countryside " (240).

This substantial volume also provides thoughtful chapters on inland and foreign trade, the monetary system and prices, and standard of living. In total the book gives its readers enormous access to materials and work on pre-colonial India. If this volume has a major failing, it is one which seems almost inevitable in any volume with eleven notable authors — the many useful and new interpretations of individual chapters have not been reconciled (or even brought together) to give a coherent picture of the interactions of different parts of the economy and the subcontinent. The well-read economic historian now has access the best work on pre-colonial India, but still needs a new framework in which to consider this information.

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M. S. Seller (ed.), *Immigrant Women*. Philadelphia: Temple University Press, 1981, pp. 340 + X.

Social historians have long lamented the lack of available source material which could help them know more about the quality of everyday life. It is well known that scholars have relied so heavily on quantitative data partly because it is only in the manuscript censuses, vital statistics, and court records that information on the lives of ordinary folk have been recorded in any systematic way. But the search for the meaning of the experience behind the statistics goes on. Certainly an understanding of the daily life experience casts light on important questions in women's history. But for historians of women, the problem of giving voice to the silent is particularly difficult because women of the popular classes, even more than men, rarely took pen in hand to record their activities, to write about their lives. Maxine Schwartz Seller's *immigrant women*, a collection of documents about American immigrant women in the nineteenth and twentieth centuries, is an example of the extent to which the prodigious effort at finding qualitative materials is reaping benefits, how much more we know today than we did a decade ago.

Immigrant women draws on autobiographies, novels, contemporary social survey, recent oral histories. The assortment is enormous and evidence of the tremendous work on the author's part to uncover many materials not familiar to scholar and teachers in the field. The book deals with women who came to the United States from all parts of Europe, as well as Latin America and Asia. *Immigrant women* is organized thematically rather than chronologically. Thus, the book is divided into eight sections, beginning with documents about the experience of women prior to emigration and why they came, early adjustment in the United States, then work, family, and community life of more settled immigrants, education, social activism, and finally, a look at second and third generation offspring. Accompanying each section is an introductory overview which places the documents in context. Making use of the recent secondary literature, it contains useful material for beginning undergraduate students who need basic information about the social experience of immigrant women. For example, the section on work discusses the nature of women's in work patterns among ethnic groups and outlines how patterns changed over time.

The real strength of the collection lies in its commitment to portraying women as subjects, rather than as objects, or victims of an oppressive history. We learn through poignant and vivid accounts that these women met problems of poverty, adjustment to a new setting, discrimination, and feelings of alienation in the public world, even conflicts with male relatives in the private world, with determination, forcefulness and a sense of their ability to affect the lives of their loved ones. These materials give us a picture of working class life that is complicated and not romanticized, not all bleak.

In offering a glimpse of the private world of immigrant life so often closed to outsiders, this collection alters our stereotypical notions of immigrants in general, women in particular. As Jewish-American poet Sally Ann Drucker, quoted in Seller, put it, "I never knew/women were weak/I never knew until/I read it in books/in school/in English." (p. 288)

This reader is not without flaws. Scholars certainly disagree about some of the generalizations which introduce the documents. It is not clear, for example, as Seller notes, that Italian women in the United States suffered any greater loss of power within the family as they adjusted to American life than did American Jewish women. Such generalizations are frustrating because the sources of these analyses are not indicated. The section on social activists is hampered because while the author does indicate that most of these women were not social activists, she also wants to show that some ethnic women, indeed, were active in the political arena. Certainly, this is true, and an important aspect of the lives of some women, but such summary statements as "participation in the labor movement cut across ethnic groups and historical periods," (p. 247) followed by various examples is confusing and not very illuminating. While it is true that all ethnic women worked in one way or another, or had to struggle with family responsibilities, it is not true that women of different ethnic groups were activists to the same degree. It would have been more worthwhile to have made some systematic comparisons, and perhaps offered some explanations to why, for example, Jewish women, more so than other immigrants at the turn of the twentieth century, were political activists.

A collection of documents which covers such a long time period and deals with so many groups cannot, alone, as the above criticesms suggest, give a student an in-depth understanding of immigrant women. But these documents do give us some wonderful detail on the lives of women as daughters, wives, mothers, and workers, and along with generally good introcutions, make this reader a useful introduction to themes in immigrant, working class, and women's history. And scholars doing research in these areas will find references to excellent qualitative sources in the quest for understanding the human historical experience.

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J. W. SHAFFER. *Family and Farm: Agrarian Change and Household Organization in the Loire Valley, 1500-1900.* Albany, N. Y.: State University of New York Press, 1982.

In the 1830's and 1840's, government officials and urban travellers who ventured into France's rural wilderness remarked on the survival of *communautés* in the Morvan: large households of three or more families organized around the collective ownership and cultivation of the soil. Initially noticed as a variety of rustic exotica, the *communautés* of the Morvan were eventually celebrated later in the century by the conservative sociologist Frédéric LePlay and his followers, as the very foundations of paternal authority and social stability in France, albeit rapidly disappearing under the dual impact of the Napoleonic Code's inheritance laws and "modern values." John Shaffer's object in this book is to explain both the survival of the *communautés* and their eventual decline by examining their history over a period of four centuries. He thus develops a theme suggested some years ago by Guy Thuillier, that the fate of the *communautés* in the Nivernais was tied not to changing concepts of property rights or value systems, but to the transformation of the agricultural economy.[1]

Shaffer shows that although the legal traditions of the *ancien régime* supported the existence of the joint family (through an attenuated form of serfdom called *bordelage*), once seigniorial and legal restrictions on tenure were relaxed, *communautés* nevertheless continued to survive. In the XVIIth century bourgeois and noble engrossers expropriated small peasant landowners and through a process of primitive accumulation, established large cereal-growing and cattle-raising estates farmed by *métayers* (sharecroppers). This process however, did not lead to an improvement of farming methods and to satisfy the immense labour requirements of their farms, *métayers* constituted a work force of parents, adult children, and their spouses, all associated in a *communauté*. To bolster his case for the role of labour requirements in determining household organization, Shaffer compares sharecropping families to the families of independent landowners (who tended to hire labourers) in the commune of Larochemially in the Morvan and elsewhere in the Nivernais. He shows that independent proprietors tended towards stem family organization whereas sharecropping households were almost exclusively more complex joint households. Nor did variations in family organization "correspond to areas where customary law favored the *communauté*. (112)"

By the beginning of the XIXth century, the emergence of the modern, centralized state and legal system in France accentuated the differences between sharecropper and landowner households. Landowning peasants eagerly made use of new revolutionary legislation on inheritance to equalize successions and took few opportunities to keep property intact (in contrast to the peasants of the Stéphanois studied by James Lehning), thereby accelerating the decline of the *communauté*.[2] Sharecroppers, on the other hand, were not as affected by Revolutionary and Napoleonic inheritance laws; their labour needs remai-

[1] Guy Thuillier, "Les Communautés de Laboureurs en Nivernais (XVIII et XIX siècles)," *Revue d'histoire économique et sociale* XXXVIII (1960): 433–452.

[2] James Lehning, *The Peasants of Marlhes*, Chapel Hill, North Carolina: University of North Carolina Press, 1980.

ned the same, and their *communautés* persisted. Ultimately, however, the rise of agricultural capitalism was more decisive than legislative reform in paving the way for the demise of the *communauté*. The growing urban demand for beef and the improvement of transportation and access to markets after 1840 stimulated landowners to improve farming methods and became more responsive to market forces. Shaffer argues that the expansion of pastureland for cattle changed the permanent, year-round labour requirements of tenant families and hence altered household organization. Areas which shifted to cattle-raising saw the dramatic decline of joint households and extended families within a single generation, whereas in areas where the older cereal economy persisted (i.e., the Morvan), the requirement of a large labour force continued to foster joint families. Although Shaffer provides a convincing demonstration of the comparative labour needs of cattle-raising and cereal-growing areas, his comparison of mean size of domaine labour force for two communes, Limanton (cattle) and Larochemillay (grains) is much weaker. With the exception of the year 1841, the labour requirements of each commune do not seem to have differed that radically and in fact, between 1861 and 1872, Limanton's mean domaine labour force grew, while that of Larochemillay declined. This discrepancy is not explained.

If capitalist market agriculture eventually helped to alter the shape of the peasant household, it also changed the nature of peasant tenure in the Nivernais, by stimulating owners to improve their land in order to increase profits. Throughout most of the Nievre, *métayage* disappeared and was replaced by *fermage* (tenant farming), which gave the tenant complete control over cultivation of the soil and marketing of produce in return for a fixed money rent. Under *fermage*, peasant wealth rose as the *fermier* became an agricultural bourgeois, attentive to the accumulation and reinvestment of capital. Only in the more isolated Morvan, where landowners had less incentive to improve farming, did *métayage* continue well into the XIXth century, bringing with it a decline in the living standards of those who laboured under this system.

Shaffer has mastered a vast quantity of source material ranging from census and land survey records (*cadastre*) to notorial archives and memoires of *métayers*. Aware of both the richness and the limitations of his sources, he has built a generally convincing case for his view that household organization was determinated not by patterns of traditional culture, but by specific economic and social relations. Shaffer's exclusive attention to the material bases of household organization, however, has led him to omit significant aspects of the history of the *communautés* which he has otherwise so carefully outlined: the substance of peasant life and the popular culture which reinforced the effects of material life on household organization. The eleven pages at the end of the book devoted to "la vie en commun" partly compensate for this lack by offering a picture of family life which helps to dispel the idealized view of the *communauté* once propagated by LePlay and company.

In a similar vein, we learn very little about the relations between *métayers* and their employers or about the contacts between *métayers* and independent proprietors. How did the agricultural revolution and the growth of capitalist agriculture affect class relations in the countryside? Shaffer tells us that large landowers in the Morvan chose to increase income by taxing their sharecroppers more heavily rather than by making capital improvements. But one would also like to know how *métayers* reacted to increased taxation in a period in which their landowning neighbours' wealth was increasing. The text of *Family and Farm* suffers from errors in the French (*veille* instead of *veillée*, the nightly social gathering of peasant families and neighbours; *travaille* instead of *travail*; *maîtress* instead of *maîtresse*) and the occasional logical leap. In trying to show that proprietors in the XIXth century did not attempt to preserve the integrity of their farms, by allowing one heir to remain on the farm " while paying rent to his her coheirs (98)," Shaffer assumes that because the number of non-resident landowners was small, that " heirs preferred their share in the family farm to a cash payment of equal value" — an assumption which underestimates the amount of land which may have been acquired not through inheritance, but by purchase. Indeed, Shaffer's own evidence (193) suggests that considerable amounts of land were acquired by purchase, particularly in the period 1866-1875.

These comments notwithstanding, *Family and Farm* is on the whole a perceptive and probing analysis of the process by which the state, the market and capitalist agriculture all worked to trasform the peasant family over a period of four centuries. Thus it provide a longitudinal perspective not often attempted by most rural or family studies and makes a fresh contribution to our understanding of the relationship between economic development and the family in rural France.

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