REVIEWS OF BOOKS

J. DEWALD, *Pont St. Pierre: Lordship, Community and Capitalism in Early Modern France* (Berkeley and Los Angeles: University of California Press, 1987) 326 p.

H. L. ROOT, *Peasants and King in Burgundy: Agrarian Foundations of French Absolutism* (Berkeley and Los Angeles: University of California Press, 1987) 277 p.

Social scientists return with fascination to the events of the French Revolution, its origins in the structures and institutions of old regime France and its consequences for social and political life in France and Europe. Each generation poses new questions, new answers to old questions; each is marked, in this return, by the larger theoretical issues under debate in their disciplines in their time. Such is the importance of the Revolution that a novel interpretation can influence not merely how we think about the Revolution, but also basic assumptions which frame and define disciplines and approaches.

The two books under review, not surprisingly, are deeply marked by the contemporary reorientation of work on the Revolution. The direction of that reorientation has been rather explicit. Since Cobban's work[1], the interpretation of the French Revolution as a bourgeois revolution has been given up[2]. This, in turn, has produced a series of attacks on the theoretical framework which supported the conventional interpretation. One line of criticism has attempted to free the revolutionary period from the structural constraints of

[1] For example, see ALFRED COBBAN, *The Social Interpretation of the French Revolution* (Cambridge: Cambridge University Press, 1965).

[2] But see ERIC HOBSBAWM, "The making of a 'bourgeois revolution'," *Social Research* 56 (1989), pp. 5-32; *idem., Echoes of the Marseillaise: Two Centuries Look Back on the French Revolution* (New Brunswick, New Jersey: Rutgers University Press, 1990).

the old regime. From this perspective, the events of the revolution have an autonomous dynamic[3] – a political and cultural dynamic – which cannot be read off from the structural (especially economic) context of old regime France. Furet, Baker and, more recently Hunt and Sewell[4], are illustrations of this approach and other work on the symbols, rituals and cultural processes of the revolutionary conjuncture[5] continues this recent interest, derived in part from the increasing influence of cultural anthropology and literary theory within history and particularly amongst historians of France[6]. Within France, the contemporary surge of interest in liberalism and theories of rights has coincided with the bicentennial of the revolution and this has focussed attention on the political Revolution, understood as a debate about the principles of political society[7]. Others, at the same time, have begun to examine gender relations in the old regime and the revolutionary process[8].

Other critics, however, have been rethinking the structural and institutional contexts of the Revolution and, at this stage, this literature has dominated historical revision[9]. An important part of this debate has centred on the French countryside in the seventeenth and eighteenth centuries. In stylized terms, we might say that relations between lords and peasant communities, as these were mediated and affected by the state and market relations, have an important focus of discussion.

We might also outline an interpretation of these relations consistent with conventional wisdom before the revisionist onslaught: relations between lords and peasants were essentially feudal, and seigneurial revenue was an im-

[3] See JOHN DUNN, "Understanding revolutionsm" *Ethics* 92 (1982), p. 302.

[4] FRANÇOIS FURET, *Interpreting the French Revolution, trans.* ELBORG FORSTER (Vambridge: Cambridge University Press, 1981); KEITH L.M. BAKER, "On the problems of the ideological origins of the French Revolutionm" in DOMINICK LA CAPRA and STEVEN L. KAPLAN (eds.), *Modern European Intellectual History: Reappraisals and Perspectives* (Ithaca, New York: Cornell University Press, 1982), pp. 197-219); LYNN HUNT, *Politics, Culture and Class in the French Revolution* (Berkeley and Los Angeles: University of California Press, 1984); WILLIAM H. SEWELL JR., "Ideologies and Social Revolutions: reflections on the French case," *Journal of Modern History* 57 (1985), pp. 57-85.

[5] For example, see MONA OZOUF, *La fête révolutionnaire, 1789-1799* (Paris: Gallimard, 1976); EMMET KENNEDY, *A Cultural History of the French Revolution* (New Haven, Conn.: Yale University Press, 1989).

[6] For a discussion, see DORINDA OUTRAM, *The Body and the French Revolution: Sex, Class and Political Culture* (New Haven, Conn.: Yale University Press, 1989), ch. 3.

[7] For example, CHRISTINE FAURÉ, *Les déclarations des droits de l'homme de 1789* (Paris Payot, 1988); JAMES TULLY, "How to Do Things with Rights," *Canadian Political Science Association Meetings*, June 1989.

[8] See, for example, JOAN WALLACH SCOTT, "French feminists and the rights of 'man': Olympe de Gouge's declarations," *History Workshop* 28 (1989), pp. 1-21; SARAH HANLEY, "Engendering the state: family formation and state-building in early modern France," *French Historical Studies* 18 (1989), pp. 4-27; OLWEN HUFTON, "Women in revolution 1789-1798," *Past and Present* 53 (1971), pp. 90-108.

[9] WILLIAM DOYLE, *Origins of the French Revolution*, 2nd edition (Oxford and New York: Oxford University Press, 1988).

portant component of the income of lords. Peasants produced predominantly for use and peasant communities provided important subsistence safeguards in times of crisis. Absolutism was the political form of late feudal social relations and provided institutional support for the local position of lords *vis-à-vis* peasant communities. Peasant communities were both anti-feudal and anti-capitalist[10]. They resisted, as best they could, the extension and rationalization of seigneurial control over individual and communal property rights. Peasant also were hostile to markets in produce, land and labour because these worked to weaken the moral economic basis of peasant communal life.

The books by Dewald and Root question this interpretation at several important places. They argue that lord-state relations were not instrumental. They point instead to the autonomy of the development of bureaucracy and the implications of bureaucratic autonomy for relations between lords and peasant communities. Here, they (especially Root) pick up a theme from de Tocqueville, neglected in the sociological and more narrowly Marxist interpretations of the pre-Revolutionary period. Both authors, as well, emphasize the participation of peasants and peasant communities in market relations, thereby questioning their assumed anti-capitalist orientation and, as well, opening up once again the general problem of conflicts of interest within peasant communities. These are not, by any means, the only concerns of these two books but they are central to their research and arguments.

Root's research is in seventeenth and eighteenth-century Burgundy; more specifically his primary research was conducted in the departmental archives of Côte d'Or (primarily the C series which includes the archives of the intendancy) and the national archives. The major theme of the book is the rise of the French administrative state and the implications for peasant communities. In working to assure the fiscal basis of the state, Root argues, administrators strengthened peasant communities. The basic reason, according to Root, were the transaction costs (the costs of measuring tax contributions and of ensuring collection) of taxation (e.g. p. 33). In insisting upon collective responsibility for tax assessment and payment, state administrators essentially passed on the costs of monitoring, and devolved some component of public authority, to communities themselves.

The same attention to monitoring is essential to Root's interpretation of the activity of intendants *vis-à-vis* village assemblies. Royal officials supported "general suffrage" (p. 101) and "full participation" (p. 75) and resisted the creation of Councils of Notables in order to make community decision-making more transparent — thus less liable to be controlled in unobservable ways by factions (especially wealthy peasants) — and to bind minorities to decisions taken by the majority (pp. 75-104). Finally, royal officials also de-

[10] While anti-feudalism makes historical sense, whether or not one agrees that this description accurately summarizes peasant orientations, the notion that peasants were pro- or anti- capitalist makes much less sense. This latter debate reads too much of twentieth century concerns into seventeenth and eighteenth century peasant behavior.

fended the preservation of communal properties and collective agricultural practices, even against attempts to weaken them by the royal council, because the restriction of communal rights would have made it more difficult for communities to pay taxes.

In strengthening and supporting peasant communities in these ways, intendants weakened seigneurial control and encouraged peasants to challenge the payment of seigneurial dues. In a chapter which earlier won a prize as the best article in modern European history for 1985-86, Root analyses the litigation in royal courts in which peasants disputed seigneurial rights (including *guet et garde, triage* and *droit d'indire*). His account echoes de Tocqueville[11]: as royal officials began to perform the functions previously supplied by lords, feudal dues became less acceptable (p. 158). At the same time, peasant communities in the eighteenth century had the financial resources to pay court costs because communal properties had increased in value. They often had also the active support of the intendant in taking their cases to court. The result was an increase in litigation which challenged the principle of lordship itself.

Root's focus, then, is institutional change and he examines how administrative centralization shaped the opportunity structures of social actors, especially peasant communities and by implication, lords. However, there is no direct analysis of the action of lords within the changing institutional framework of the old regime.

In contrast, the focus of the research by Dewald is on a lordship, the barony of Pont-St.-Pierre, an estate near Rouen in Upper Normandy. Relying on field research primarily in the departmental archives of the Seine-Maritime and the Eure, Dewald examines practices of estate management, interaction between the de Roncherolles (the familial holders of the barony), local nobles, peasant communities and market towns from the late fourteenth to the mid-eighteenth century when the barony was sold. He is concerned with how this barony, and other nobles in the Pont-St.-Pierre region, adapted to two kinds of change: the decline of the seigneurial system and the spread of market relations (pp. 282-4).

In assessing the consequence of changes in the seigneurial regime, Dewald examines seigneurial revenues, and the role of seigneurial justice in local life. Fixed rents and seigneurial monopolies declined drastically between 1398 and 1780 as a proportion of total revenue. The change was not gradual but was marked by a sharp drop between 1520 and 1590. As seigneurial revenue declined, sales of wood (and, to a lesser extent, hay), became more important. The barony was well-situated — with easy river transportation to both Rouen and Paris — to take advantage of the increased urban demand for firewood.

[11] Alexis de Tocqueville, *The Old Regime and the French Revolution*, trans. Stuart Gilbert (Garden City, New York: Doubleday Anchor, 1955 [1856], pp. 30-31 ff.

The primary limit on exploitation of the forest was political, and stemmed from increasing royal regulation of forest resources (pp. 184-186). After 1669, royal officials could intervene "almost at will" (p. 186) in order to protect the forests from profit-minded owners such as the Roncherolles who, in responding to market opportunities to compensate for the decline of seigneurial income, were cutting early growth with no attempt at conservation (One of Colbert's interests in the forest stemmed from the importance of a reliable supply of mature trees for ship timber). In other words, the ability of the lordship to substitute income in the market for seigneurial revenue was restricted by changes in administrative practice, an argument which is entirely consistent with Root's analysis.

Further, the forest, as in Burgundy, was a source of contention between the lordship and peasant villages which attempted to protect their rights of usage against seigneurial encroachment (pp. 147-150). Like Root, Dewald stresses the importance of the judicial system in the defence of communal rights. From the sixteenth century, villages hired attorneys and engaged in litigation in royal courts. "... [The] state, with its courts, commissioners and legal procedures, was not an alien or purely tax-collecting body to sixteenth- and seventeenth-century villagers. They could use it effectively to defend their situation" (p. 151).[12]

The declining role of seigneurial justice, according to Dewald, was marked by the smaller number of cases which came before the court, the lower profits associated with position in the High Justice and the challenge to the barony's notarial monopoly caused by the increasing presence of royal notaries (pp. 254-256). The decline occurred even as villagers became more litigious; this combination of changes occured as royal replaced seigneurial justice and as the informal authority of *laboureurs* to resolve intra-village disputes increased in the eighteenth century (p. 263). The emergence of influential groups of *laboureurs* in peasant villages is an important part of Dewald's story. The growth of relatively prosperous farmers was linked to opportunities in rural industry and commerce, particularly the trade in spun cotton, and the market in wood. Their presence was both caused by, and contributed to, the monetarization of the village economy. Tenant farming based on cash rents prevailed by the late eighteenth century and cash rents meant production for the market (p. 59). In the village he studied most extensively, the inhabitants worked as wage labourers or farmed (on the basis of cash rents); agricultural production, Dewald concludes, was not embedded within a peasant community (p. 88).

In contrast to Root, Dewald argues that communal practices did not hinder agricultural progress. Root suggests that the intendant supported com-

[12] The recent article by Markoff provides an excellent analysis of peasant evaluations of institutions of the old regime on the eve of the Revolution. JOHN MARKOFF, "Peasants Protest: The Claims of Lord, Church and State in the Cahiers de doléances of 1789," *Comparative Studies in Society and History* 32 (1990), pp. 413-454.

munal rights in order to stabilize the tax base of communities. These practices explain the stagnation of French agriculture because they meant that land enclosure was not pursued (Root, 1987: 150-154). But in the part of Upper Normandy studied by Dewald, communal crop rotation, rights of gleaning and of *vaine pâture* did not prevail (pp. 87-88), yet "progress" did not occur. Weak or absent communal rights are not sufficient causes of increases in agricultural productivity; enclosure does not guarantee improvement. Dewald emphasizes instead the obstacles to investment in agriculture in accounting for stagnation.[13] These include conflicts of interest between tenants and landowners over the distribution of increased profits which might be produced by investment, and changes in the availability of fertilizer which made it more difficult to reduce fallow land and replace it with forage crops.[14] Sheep and dairy cattle, important sources in the region (especially sheep) of fertilizer, became increasingly difficult to maintain because of forward linkages to the transformation of agricultural products. Farmers had lost their access to local centres of woollen production at the turn of the eighteenth century, as higher quality Spanish wool replaced poorer quality local wool, and consequently sheep raising was no longer profitable. Local dairy producers likewise had to compete increasingly against specialized products of higher quality (pp. 84-86).

Dewald argues that the nobility was much more of an obstacle to economic development than the peasantry. This position converges with Root's, who has argued recently that peasants resented the restrictions on access to markets associated with seigneurial privileges.[15] The de Roncherolles, Dewald states, were sheltered from the market economy until about 1700 because their household was relatively self-sufficient (p. 286). Their attitude towards the market economy was paternalistic (p. 212) — oriented towards the maintenance of just prices and the limitation of competition. Their regulation of the local economy was also designed to protect market monopolies in transactions in grain and cotton (pp. 244-250, 233-234). In other words, the local position of the de Roncherolles was tied in part to rents generated by comparative advantages in the administration of justice and economic activity. These monopoly rents, which together are for Dewald central features of feudalism, were threatened by state centralization and market expansion.

[13] Dewald's argument is generally consistent with Grantham's work on open-field farming. See GEORGE W. GRANTHAM, "The persistence of open-field farming in nineteenth century France," *Journal of Economic History* 40 (1980), pp. 515-531.

[14] On the importance of animal husbandry for agricultural innovation, see GEORGE W. GRANTHAM, "The diffusion of the new husbandry in northern France, 1815-1840," *Journal of Economic History* 38 (1978), pp. 311-337 and PHILIP T. HOFFMAN, "Institutions and agriculture in old regime France," *Politics and Society* 53 (1988), pp. 244-245.

[15] See ROOT, *Peasants and Kings*, p. 67; *idem.*, "The case against Georges Lefebvre's peasant revolution," *History Workshop* 28 (1989): 88-102, 106-110 and the response by PETER M. JONES, "A response to HILTON L. ROOT, 'The case against Georges Lefebvre's peasant revolution'," *History Workshop* 28 (1989), pp. 103-105.

However, Dewald also points out the ways in which the Roncherolles benefited from their relations with the state. Military service, governorships and marriages with prominent Parisian families (pp. 179-182) provided income and connections to patronage networks. The relevant comparison here would be with the work of Beik[16] who, among contemporary historians, has most forcefully argued (following Anderson)[17] that French absolutism was a feudal state. The relationship between the absolutist state and lords is essentially instrumentalist, according to this view. Dewald attempts to strike some balance in his account by arguing that the relationship was neither this mechanical, nor purely conflictual. He points to both the benefits and problems of absolutism for lords. The Roncherolles, he argues, were part of the state, but they were also its victims. (p. 187). In contrast, Root pays less attention to how local nobles may have benefited from the absolutist state; he emphasizes the degree of conflict, rather than cooption, in state-lord relations. It might be useful, as a counterpoint to this interpretation, to investigate in Burgundy the extent to which intendants were not only agents of the state but also clients of local elites,[18] and the consequences of clientage for administrative practices.

Both Root and Dewald have something useful to say about peasant communities. They speak as well on other problems — notably state-formation and the relationship between states and markets. They can, finally, be read as evidence for the substitution of de Tocqueville for Marx as the master figure in recent work on the old regime and the Revolution.

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P. MATHIAS and J.A. DAVIS (eds.), *The First Industrial Revolutions*, Preface by LUIGI DE ROSA, Oxford, Basil Blackwell, 1989, pp. 175

The volume brings together the lectures given during the first of the annual economic history seminars that the *Istituto Italiano di Studi Filosofici of Naples* has been organising since 1984 in collaboration with the Universi-

[16] WILLIAM R. BEIK, *Absolutism and Society in Seventeenth Century France: State Power and Provincial Aristocracy in Languedoc* (Cambridge and New York: Cambridge University Press, 1985).

[17] PERRY ANDERSON, *Lineages of the Absolutist State* (London: Verso Books, 1979).

[18] JAMES B. COLLINS, *Fiscal Limits of Absolutism: Direct Taxation in Early Seventeenth-Century France* (Berkeley and Los Angeles: University of California Press, 1988), p. 19; SHARON KETERING, *Patrons, Brokers and Clients in Seventeenth-Century France* (New York and Oxford University Press, 1986), p. 167.

ties of Oxford and Warwick. The seminar courses, held by leading economic historians as well as by business studies experts and spokesmen from industry, deal with the process of economic development from the eighteenth century to the present. The very title of the series inaugurated by this volume, *The Nature of Industrialisation,* reveals the breadth of the themes considered and the ambitious scope of the work. The studies aim to investigate the principal aspects of the process, or rather processes, of industrialisation, illustrating the development of each productive sector, the role played by science and technology, the regional characteristics, the financial needs and social and demographic links.

The main purpose of this and of the subsequent volumes is to provide scholars and, more important, students with a critical guide to the debate — still wide open — on the history of industrialisation, introducing the most recent research and ideas on themes which have been amply studied in the past but which invite new contributions and interpretations.

In this respect the essays by Nick Crafts, Professor of Economic History at the University of Warwick and by Peter Mathias, currently at Downing College, Cambridge, are of particular relevance. Crafts illustrates the significant contribution that the "New Economic History" has made to our understanding of the industrial revolution. The explanation about the productive transformation and the rethinking about the role of agricultural growth as a support to British industrialisation are only two such examples.

Of special interest is the essay by Peter Mathias who opens the volume with a fundamental reflection on the very concept of industrial revolution which he considers to have been too often abused. The English historian recalls that the term "industrial revolution" was first used in the 1820s by French commentators who considered the economic transformation in England a process as deep-rooted and as overwhelming as the political revolution of 1789 in France.

Mathias believes therefore that a new definition is needed and suggests two essential parameters: higher growth rates of the economy as a whole and, closely related to the first, structural change. Consequently it is necessary to distinguish economic expansion, which can be achieved by adding labour, capital, land and resources to existing modes of production, from economic development. The latter implies changes in the nature of the economy, a culmative increase in the efficiency of the system and a change in basic economic relationships and in society. Yet the main distinguishing feature of a process of industrial revolution lies in the possibility of verifying the said aspects over the long term.

It is this long-term view which is central to Mathias's study of the first stages of the process of English industrialisation and of the concept of "proto-industrialisation", a phenomenon dear to the late-lamented Mendels[1] and

[1] Franklin F. Mendels, "Proto-industrialisation: the first phase of the industrialisation process", *Journal of Economic History,* 32 (1972), pp. 241-261.

concerning the expansion of production in England in the eighteenth century which although still based mainly on artisan technology and on cottage industry was no longer exclusively destined to local markets but was rather commercially oriented. The phenomenon illustrated by Mendels was especially important in the textile industry (for example in Flanders in the late seventeenth and eighteenth centuries, in the western counties of Great Britain, in Lombardy and in Bohemia) and contributed to posing significant preconditions for the subsequent process of industrialisation. It was in this context that the figure of the merchant-entrepreneur developed (providing the agricultural-artisan nuclei with raw materials and sometimes machinery and organising distribution of the products) and it was in this context that the training of skilled labour and the accumulation of capital for reinvestment in industry both occurred. In 1982, ten years after its appearance, Mendel's model was nevertheless widely criticised: during a congress in Budapest more than one historian attacked the theory of proto-industrialisation for being too mechanical since it generalised on the experience of certain regions to support a functionlist vision.

On the other hand, Mathias believes that the hypothesis contains several interesting elements: firstly the important links between demographic dynamics and proto-industrial development; secondly the primary relationship between industry and agriculture, which have wrongfully been considered by many historians as separate entities. For Mathias, too, however, Mendel's model has great limits. It neglects the urbanisation factor which is generally recognised to be fundamental in the process of industrialisation, and more important it falls down when empirically tested. Many of the areas which underwent a phase of proto-industrialisation did not afterwards experience subsequent evolution (Wales for example) while it has been shown that a sector which had been an important "protonucleus" was not central to subsequent industrial development (textile production in Germany and Bohemia for example).

The path to industrialisation, therefore, has involved other dynamic factors apart from proto-industrial growth. Moreover, judged over the long term, the initial phases of industrialisation represented a big historical turning point, a "megaphenomenon" that occurred in the late eighteenth century as Mathias shows, comparing the aggregate and per capite data of long-term growth rates.

John Davis joins Mathias in criticising certain *clichés* which have beset historiography: he also defines industrialisation as the fruit of a slow process of growth, occurring simultaneously but over a very long period and in many sectors of the economy. According to Davis, the different ways to such growth should be considered in all their local and sectoral specificity. In this respect the contribution by the "new economic historians" is important since it undermines one of the fundamental axioms of comparative historiography on the subject of the industrial revolution. Until now, many scholars have

approached the issues of European industrialisation posing the English case as a paradigmatic case, as a basis for carrying out comparisons or variations on a theme, without examining the single paths to industrialisation taken by different European countries. Davis, on the contrary, dwells more on the case of French industrialisation than on the English case, reconstructing the historiographical debate around problems of location of productive development, and examines the relationships of integration and competition between different industrial regions. This opening by Davis is particularly significant and is probably related to the wide knowledge and great interest shown by the English historian in Italian economic history which have led him to make numerous and valuable comparisons with both English economic developments and those of other countries, and which have above all prompted him to reconsider the role of the state in economic development.

In fact many Anglo-Saxon historians deny its importance. Pollard goes as far as stating that such a role was negative for economic development, which depended instead on the market and which took place independent of state economic policy. Davis dissents from such a conception: in his opinion the state was not absent from the process of industrialisation in Great Britain in the eighteenth century. With the creation of the Bank of England in 1694, the state paved the way for the reorganisation of public finances and consequently for England's economic stability in the eighteenth century and for most of the nineteenth century. Moreover the state made provision for the expansion of colonial trade and as a result, for the enlargement of the market, and passed laws to discourage speculation and fraud, helping thereby to promote transactions.

These and other observations lead Davis to confirm his hypothesis about the importance of the role of public institutions in industrial development and bring him to find common ground with French historiography — which has recently revised the role of the state in the economy in the eighteenth century — outlining various European models of development based on the interaction of state intervention with private initiative. As far as European nineteenth-century history is concerned, Davis illustrates several "classic" cases of state intervention in the economy: the construction of German railways, departure point for the country's industrialisation, was supported indirectly by the state which reduced the elements of risk, stimulating private investments through its own guarantee. For Italy the example is Cavour's Piedmont where the principal contribution of the state was that of creating a general context in which economic activity and investments (including foreign investments) were encouraged and stimulated.

Nevertheless, as Davis admits, it is not easy to assess the real contribution of the state, which needs to be studied at various levels and not limited to measuring public contributions for the construction of infrastructures or government contracts awarded to specific industrial sectors. Recent studies, for example, have reassessed the importance of human capital and therefore of

the population's level of education. This has led to focusing attention on the systems of schooling and training institutes of many European countries in the second half of the nineteenth century.

Five thematic essays complement the valuable insights made by Davis and by Mathias: one on transport, in which Professor Barker illustrates the coexistence of old and new systems of transport in the phase of the first English industrial revolution; one by Robert Woods, geographer and demographer, on the ties of reciprocal influence between population growth and economic change; the essay by Kristine Bruland, a historian of technology, on the transformation of work during industrialisation. Peter Mathias is author of the other two essays. After his opening essay on the concept of industrial revolution, Mathias goes on to examine in one study the relationship between agriculture and industrialisation and in the other how the industrial revolution was financed. In this latter essay, Mathias evaluates the size of the market of British capital in the period covering the last twenty years of the eighteenth century and the first half of the nineteenth century, analysing at a macroeconomic level the national context of finance in which English enterprises acted in this crucial phase of industrial development and at a microeconomic level the needs of financing and capital supply.

In conclusion we can say firstly that although it is the work of different authors, the volume is marked by an underlying coherence and unity; secondly that although it is intended to present the state of art on an issue of such importance, it offers countless occasions for reflection and throws open many questions and historiographical aspects which were once considered closed.

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T. W. MOODY - W. E. VAUGHAN, (eds.), *A New History of Ireland, Volume IV: Eighteenth Century Ireland, 1691-1800*, Oxford, Clarendon Press, 1986, £ 65.00, $ 115.00.

The idea of a *New History of Ireland* was first given expression in 1962 by the late T.W. Moody, Erasmus Smith Professor of History at Trinity College, Dublin, and from that time until his death in 1984 he was, as its general editor, the moving force behind the project. Over the last twenty-five years, in fact, the *New History* has undergone a very considerable number of transformations in its format, at least, if not in its scope. The *New History* is now projected at ten volumes, seven of which carry the story from prehistoric to modern times, and three of which consist of chronologies, maps, and statistics to supplement the historical account. To date, only two of the seven his-

torical volumes, III (1534-1691) and IV (1691-1800), have been published, and two, VIII (chronologies) and IX (maps) of the three supplementary volumes.

The *New History* has obviously run into serious difficulties in spite of its having been generously supported by the Irish government and private benefactions, and whether it will be completed before the end of the present century is now very doubtful. But whether the project is completed or not, the volume under consideration here poses some very serious problems in itself, and by extension also for those volumes to come. Leaving aside all the vicissitudes involved in collective ventures in historical scholarship, the main problem with this volume is that it has been too long in the making. Most of the essays in this volume were submitted in 1973, and because of the very long delay in publication, had to be revised in 1981-82. Between 1973 and 1986, however, a veritable revolution has taken place in the writing of Irish history in the eighteenth century, and the sad fact is that, even in the light of the revisions made, very little attention has been paid to that historiographical revolution in this volume. In a word, this volume of the *New History*, ironically, is now obsolete.

Still, in a volume of some seven hundred pages, including some twenty essays, and a bibliography of nearly a hundred pages, there are obviously levels of obsolescence. Those essays that are most out-of-date are those that deal with the political scene, as well as those that treat of the social, ecclesiastical and literary dimension that have been written by historians, whose professional expertise is primarily political. Indeed, the introduction and twelve of the twenty essays in this volume have been written by four historians whose basic research and writing may be characterized as political. The other eight essays include two each by an economist, an art historian, and a musicologist, and one each by a geographer and a Celticist. These eight essays vary in their obsolescence, and some, of course, are more interesting than others. The less interesting are those that have a tendency to survey and catalogue without being very analytical, while the more interesting make not only new and original contributions to our knowledge, but rise by their power of generalization out of the slough of mere historian nominalism. Pride of place for the new and original must certainly go here to the two essays by Brian Boydell on Irish music before 1700 and from 1700 to 1850. Finally, special mention must be made to the truly awesome and comprehensive bibliography compiled by David Dickson, which gives sad and ironic point to all the various levels of obsolescence in the essays in this volume. Let us hope also that the bibliography will serve as both a caution and a warning to the editors of the *New History of Ireland* that anything less than a "harvesting of the best contemporary scholarship", promised in the preface of their third volume (1976), will simply not do.

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G. Mori - P. Roggi (eds.), *Firenze 1815-1945. Un bilancio storiografico*, Florence, Le Monnier, 1990.

Essays on historiography give cause for complaint. One frequently comes across works which are inevitably centred upon disappointment about the discipline's unfortunate destiny. It goes without saying that, in many cases, this is merely an over-used expedient for drawing attention to one's own contribution. However, there are cases in which the historian's torments are motivated by a situation of real abandon or at least of inadequate attention. The historiographical assessment carried out in the present volume belongs in effect to the second case. This is borne out by just one observation which the two editors are careful to make in the introduction: no general work exists on the period considered (that is from 1815 to 1945, as is apparent in the title), if we exclude the work by Spini and Casali (G. Spini - A. Casali, Florence, Rome-Bari, Laterza, 1986) in the series "Storia delle città italiane", published by Laterza which has produced some good books on national urban historiography, although these take a completely different angle from the one used by Mori and Roggi.

Having read the volume, Florence certainly appears to have been neglected by historiography. Almost all the essays, which are very detailed, emphatically denounce the existing lacunae while at the same time taking heart at the quality of what has already been produced, and not without reason; the bibliography at the end of each essay systematically confirms the different theses argued by the scholars being referred to. Overall, if, on the one hand, the more than five hundred pages appear comforting and give cause for satisfaction, on the other hand the meaty list of absences are an invitation to start work immediately to fill the numerous gaps. If the work also bears the character of a project other than that of a work tool, then the possibility of coming to grips with the history of the city to provide a global interpretation — something which has not yet been attempted — is very real and is certainly worth exploiting.

Twenty authors with their respective disciplines contribute to the work: Carlo Corsini is responsible for the chapter on demography, Michele Lungonelli and Domenico Preti for that on the economy; Carlo Pazzagli has studied social life, Angelo Varni the workers' movement; Zeffirio Ciuffoletti and Pier Luigi Ballini have been given the task of commenting upon political and administrative life; Carlo Cresti has examined urban studies and architecture, Simonetta Soldani education; Cosimo Ceccuti has looked at cultural institutions; Daniela Donnini Macciò and Antonio Magliulo have been concerned with economic thought, Francesco Malgeri with religious life; Paolo Galluzzi has considered science and technology; Umberto Baldini has had the task of writing the delicate chapter on art, Giorgio Luti that on literature; Marcello De Angelis is the author of the chapter on music, Paolo

Poesio of the chapter on theatre and prose and finally Luigi Mascilli has written the last chapter on foreigners in Florence.

The picture emerging from the essays appears exhaustive; references range from the most recent books to works of the last century, from purely celebratary volumes to articles that have not appeared in specialist historical journals. Moreover the structure makes it possible to follow the various historical agendas for each subject. Another undoubted merit should also be ascribed to the architects of this operation: that of having attempted to get beyond the myriad of *clichés* which have always weighed on the city, by no means an easy task. One of the most encumbering of these is indoubtedly the image of Florence projected from outside of Florence and in particular from abroad, which both Mascilli Migliorini and the editors in their introduction dwell upon. Finally there is the merit of having contributed to the definition of the identity of a complex city which has had a very special role in the context of Italian and European history and whose economic and cultural decline — a perculiarity not just Florentine but involving a large number of major Italian cities — is subject to deep contradictions.

Here we are presented with a central issue which is unavoidable in a study dealing with the historiographical assessment of a city such as Florence. The objective of a book of this kind, above and beyond a fundamental accounting need, was, in fact, not only that of determining the moments of change and the turning points — in short, the critical points — but also the unexpressed choices relating to what the city did not achieve, caught within the eternal dilemma which even today characterises its dual role as the Italian Athens on the one hand and as a productive city on the other. The attempt has been made and has undoubtedly yielded a valid result despite the restricted nature of the periodisation chosen. Certainly the fact that the research has been confined to one hundred and thirty years is a limit with the risk of producing an analysis that is somewhat narrow in scope: the editors have probably been confronted with organisational problems and have consequently decided to work on a more reduced timespan. But if somone has already in mind the volumes of a great history, in that case a different formula is perhaps required. The structure and choice of the essays could have also been slightly different and consideration taken of the more important methodological developments in recent historiography dealing with urban history, which have given rise to studies on urban networks and systems; inclusion of such studies would have unquestionably rendered the approach less traditional than that intended by the authors themselves, as stated in their introduction. A unitary study of infrastructures and urban services would, in our opinion, provide a new way for interpreting the profound transformations affecting the urban fabric in the transition "from pre-industrial city to city of capitalism".

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M. NORTH. *Geldumlauf und Wirtschaftskonèunktur im südlichen Ostseeraum an der Wende zur Neuzeit (1440-1570)*, Sigmaringen, Jan Thorbecke Verlag, 1990 (Kieler Historische Studien, vol. 35), pp. 276, with diagrams, tables and illustrations.

With Goethe and Eichendorff, who have written ballads about the "treasure hunter" — set to music respectively by Schubert and Schumann — literature has reelaborated an extremely popular and persistent motif of European imagination: the discovery of hidden treasures (see heading "Schatz" in *Handwörterbuch des Deutschen Aberglaubens*, Berlin, Leipzig, De Gruyter, 1935-1936, vol. VII). Such a motif is based on the historical reality of the burying of money under the ground or behind the walls of one's dwelling, as means of hoarding and protecting one's wealth. There are many studies and controversial theses about the economic and psychological causes of this behaviour; it is certain, however, that treasure discoveries have been very useful in numismatics and economic history. On the basis of this source and of financial and book-keeping documents (the two sources complement each other: the first is concerned with more precious currency, the second with money in wider circulation), M. North investigates money circulation in the Schleswig-Holstein area from the middle of the XVth well into the XVIth century, skillfully exploiting the data. The origin of the money, the type of currency, the content of gold and silver with the relative value of the two metals and the quantity of species are all analysed. However, the aim of the research is broader and more ambitious: the author sets out to illustrate the relationship between monetary variables and real variables, namely, the balance of payments and the conjuncture, which are examined more closely in the following chapters of the book.

The development of the work — divided into chapters which are virtually autonomous in content — reflects the two sides of the dilemma around which the interpretation of the period rotates: primacy of money or primacy of the real economy and of demography in the history of the European economy between the depression at the end of the fifteenth century and the price revolution? For these two major phenomena the question appears to be posed in equal and contrasting terms. The dispute between monetarists and "neo Malthusians", for whom demographic fluctuations were the primary cause of deflation and inflation, has given rise to a large quantity of literature which North takes account of (chiefly in the introduction and in chapter IV). For those who have recently tried to examine the question and to throw light onto the assumptions implicit in the two positions, the debate is heavily conditioned by two elements: the scarcity of data and the misuse of tools taken from economic theory, in particular Fisher's equation (see the two lucid contributions by D.D. Flynn, *The Population Thesis, View of Inflation versus Economics and History*, and *Use and Misuse of the Quantity Theory of Money in Early Modern Historiography*, in "Münzprägung, Geldlumauf und

Wechselkurse", edited by E. Van Cauwenberge and F. Irsigler, Trier, Verlag Trierer Historische Forschung, 1984, pp. 361-382, 383-418). The fact is that in the contrast between the two schools "a stalemate has resulted: both sides call for more evidence. Better data are always preferred but better theory can sometimes resolve a debate" (ivi, p. 362). The reformulation of the monetarist thesis through more sophisticated models than those applied until now by historians on both sides, could resolve the issue.

North's contribution, on the other hand, lies more in the increase of data than in the improvement of theoretical tools. The conclusions he reaches about his area can be defined as moderately neoMalthusian: the effects of the monetary factor were more substantial in the recession phase of the XVth century, while demographic growth had more of an impact on the price revolution than the increase in the circulation of precious metals, which was in reality only moderate. The middle way of the plea for empiricism and of the rejection of unflexible models in favour of historical complexity is at the same time the strength and the weakness of North's work. On the one hand his attention to detail and his capacity to exploit the sources is most admirable; on the other hand, his analysis of the single variables is perhaps a little laborious and overgenerous while he is somewhat parsimonious in drawing correlations. Work still needs to be done if economic theory is to be used correctly and productively (in this work, as in others and for other periods, the interpretation of equations such as Fisher's laws of cause and effect can lead to misunderstandings) and if the monetary factor is to be integrated into the disordered bulk of economic and social history. For the moment, rather than forming a constituent part of it, theory still appears, at times, to be more a mysterious reflection, a shadow which is neatly outlined but difficult to grasp.

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