

benz and Wolfgang von Stromer. The author's findings are important not only to specialists in the German field but to all Europeanists with comparative interests in urban or economic history. The monograph itself may prove too detailed for most scholars, however, even though the fine index, clear organization, careful highlighting of conclusions and excellent writing make the book accessible and readable. Non-specialists may prefer to use Irsigler's superb chapter in the impressive two-volume collection entitled *Zwei Jahrtausende Kölner Wirtschaft*, edited by Hermann Kellenbenz (Köln: Greven Verlag, 1975), I: pp. 217-319, where the author summarizes economic developments as well as the broader social and political context in exemplary fashion. He has also placed an English summary of his views in the *Journal of European Economic History*, 6 (1977): pp. 269-306. In these places the reader meets an economic historian of the first rank. Along with the Kellenbenz collection and Wolfgang Herborn's recent books on the city élites and on the wine trade, Franz Irsigler's publications, which now include an important edition of grain and bread prices in the city from 1368 to 1797, make Cologne fare very well indeed in current historiographical competition with Nuremberg.

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BOB SCRIBNER and GERHARD BENECKE, eds. — *The German Peasant War of 1525 — New Viewpoints*. London: George Allen & Unwin, 1979. Pp. 206.

ROBERT H. LUTZ. — *Wer war der gemeine Mann? Der dritte Stand in der Krise des Spätmittelalters*. Munich and Vienna: R. Oldenbourg Verlag, 1979. Pp. 122.

The German Peasant War has acquired in the course of centuries — actually beginning soon after the event — political and ideological significance that almost overshadows the actual impact of the uprisings. Of course, it was one of the most widespread revolts of early modern Europe, also the last major one before the age of "bourgeois" revolutions and, connected in one way or another with the Reformation, something of a watershed between the medieval and modern world in Central Europe. Scholarly interest in it has coincided regularly with the turning points of German history: first "discovered" as an early pattern for a revolutionary future by the romantic democrats; then used by Engels to teach a lesson to the un-revolutionary German middle classes; and interpreted by the more or less National-Socialist *völkisch* writers of the 1930s as the *Volk's* great moment in history. After World War II the German Democratic Republic, in search of "progressive national traditions" (a commodity in short supply in the area) latched on to it and claimed that it had been the "early bourgeois revolution" in German lands, where the "real one" was missed at least until 1918. Belatedly, in reaction to this, and clearly connected to the turns in West German *Deutschlandpolitik*, the "objective" social scientists of the Federal Republic joined the discussion in an attempt to declare the democratic aspects of this "rising of the common man" as their heritage.

Although such political considerations certainly hampered the systematic study of the Peasant War, they also challenged some of the best minds to join the

debate. In preparation for the 450th anniversary both sides presented "theses" about the proper analysis of the event and a considerable literature has been published during the last decade: of the near-300 titles in the bibliography of Scribner and Benecke (henceforth SB, pp. 190-99) at least two-thirds date from the 1970s. The more orthodox official Marxists (represented above all by Max Steinmetz's "Theses", SB, pp. 9-18) marked out the points *quod sint demonstranda* and set out from the assumption that revolution is the "normal" or "typical" case. Their Western counterparts (such as Peter Blickle's "Theses", SB, pp. 19-22) ask rather why it was necessary to resort to violence in resolving the conflicts in early modern German society and what united the different groups of the disenfranchised commoner (*gemeine Mann*) in what has been named the German Peasant War. The answers to these questions, and many others, are even on the dogmatic side much less one-sided than the respective theses. A fine example of this is Karl Czok's study on the plebeian strata in the suburbs of Leipzig, Halle, Mühlhausen, etc. (SB, pp. 84-97). Starting off from the proposition of the Peasant War having been an "early bourgeois revolution", he elaborates on the hitherto overlooked pioneering role played by the legally and economically disadvantaged poor craftsmen, labourers, beggars, etc., who supported radical reformers and carried rebellious ideas and deeds into the cities and from town to country. He still retains the official notion about the revolution's class character, but his work tells more about the problems inherent in this concept than volumes of dogmatic squabbles.

Scribner and Benecke (in the Introduction, pp. 1-8) mark out four major theoretical issues which they feel that the articles raise beyond the veritably new viewpoints on the Peasant War: the conflict between the particular and the general ("historian" vs. "social scientist"), the short-term events and the long-term context of social transformation (from "feudalism" to "capitalism"), "monocausal" vs. "pluralist" interpretation, and the challenge to interdisciplinary procedures. It may be augmented by one more, certainly less general, but very relevant issue: the place of the Peasant War in the long-term development of the German state and society, the special problems of Central European "transition", as it were.

Indeed, almost all authors struggle with presenting an overall analysis while, wisely and laudably, basing their arguments on local and particular evidence on local events. They all concur in recognizing the similar patterns in the protest movements from, say, 1490 to 1526, while emphasizing the often very different social, economic and ideological contents of the various revolts that added up to the Peasant War. So, for example, Rudolf Endres ("The Peasant War in Franconia", SB, pp. 63-83) notes the lack of unity between rural and urban rebels and denies that any co-operation existed between different princely territories. Karl Czok places, as mentioned, great emphasis on the semi-burgers of the suburbs. Heide Wunder ("The Mentality of the Rebellious Peasants: The Samland Peasant Rebellion of 1525", SB, pp. 144-59) stresses the legal and cultural differences between Germans and Prussians in the area she studies. David Sabean ("Family and Land Tenure: A Case Study of Conflict in the German Peasant War", SB, pp. 174-89) draws attention to the tension within communities (even families) between the "losers" in the system of land inheritance under South German *Leibeigenschaft* and the tenured peasants. The recognition of relevant distinctions coupled with the search for general trends is a common denominator of almost the entire new research on the Peasant War.

Different models of thought are recommended by the authors for proceeding from the particular to the general: many (both Marxists and non-Marxists) see the proverbial red thread in one or another aspect of the rebels' ideology. Others refer to the Weberian concept of legitimation (e.g. Blickle in the excerpt,

entitled "Biblicism versus Feudalism", from his book on 1525, SB, pp. 137-43), to social science models of interaction, quoting Eric Wolf (Wunder, SB, pp. 144ff), or to "systems-conflict", citing Barrington Moore (Jürgen Bücking, "The Peasant War in Habsburg Lands as a Social Systems-Conflict", SB, pp. 160-73). Of course, the classical Marxist concepts of class and class struggle are present as well. Siegfried Hoyer ("Arms and Military Organisation in the German Peasant War", SB, pp. 98-108) orders the disparate evidence around the organizational forms of peasant *Haufen*, their weapons, supplies and command structure.

The studies focusing on ideological aspects are in themselves varied. Heiko Oberman ("The Gospel of Social Unrest", SB, pp. 39-51) abandons the traditional position of blaming or crediting the Reformation with the rebellion, and rather emphasizes the tragic contradictions between the theological message and the social reality. (Actually, he is one of the few who point to problems with roots reaching back beyond the early sixteenth century which became apparent in the Peasant War.) The excellent study by Horst Buszello ("The Common Man's View of the State in the German Peasant War", SB, pp. 109-22) and its parallel piece, Hoyer's sometimes too detailed but interesting polemical analysis of a 1525 pamphlet, *To the Assembly of Common Peasantry* (SB, pp. 123-36), use the ideas contained in their texts more as points of departure to a deeper analysis than as explanatory, causal bases à la *Geistesgeschichte*. The reference to "mentalities" by Wunder (SB, pp. 144ff) is rather a challenge than an accomplished fact: the discreet demands and grievances following from the divergent status of free and unfree men, Germans and Prussians are at the centre of the discussion.

A good overview of all these approaches, in the form of a critical survey of the state of research, is offered by John C. Stalnaker (SB, pp. 23-38), even though the brief sketch of social strata offered as a conclusion barely warrants the title: "Towards a Social Interpretation of the German Peasant War" (unless, of course, one stresses the preposition!). Together with the editors' introduction, containing many a challenging new idea, this article is in a way a "built-in" review of the volume and should perhaps be read first by anyone who wants to study the German Peasant War.

Where does all this bring us? There is no doubt that the Peasant War as a sum of different but convergent revolts has to be seen as a crucial period in the history of early modern Central Europe and a milestone in German social and political development that revealed many a problem of long-term significance. Although I still cannot see the heuristic or analytical value of insisting on the appellation "early bourgeois revolution" (especially because the adjective "early", or one should rather say "premature", rarely receives the proper emphasis!), these studies make it clear once more that the Peasant War contained elements pointing far beyond an uprising for better conditions within the frame of feudal society. When Horst Buszello writes that, "The great mass of rebels did not question the existing social system. They 'merely' accepted demands to place nobles and clergy on the same footing as burghers and peasants" (SB, p. 119), he must surely be aware of the irony of this statement. At the risk of sounding old fashioned and oblivious of the primacy of socio-economic factors, one might raise the question: was it not the case in sixteenth-century Germany that just those social traditions and political institutions were missing which would have permitted these "accepted demands" to become the bases of a civic society — a society that has failed to develop east of the Rhine ... ever since?

On the other hand we shall probably stick to the received term of "peasant war", even though Oberman (SB, p. 39) convincingly argues that *tumultus rusticorum* could be translated differently and many other authors also point to the

non-agricultural masses and leaders in the Peasant War. The sources often use the more general term, "common men". But I suspect the present delight of West German historical circles in this expression originates not only in its *quellenmässig* authenticity, but in their — not unfounded — unease with class analysis in a pre-capitalist society. However, peasantry is surely not regarded as a class in any sociologically informed discussion, even less in Marxist terminology. The East Germans' use of *gemeiner Mann* is not meant analytically, but here, too, the ideological background is apparent: by reducing the rural emphasis of "peasant", the argument for a bourgeois class content could be strengthened. While it is quite clear, and the articles in Scribner and Benecke underline this very well, that the alliances of the disenfranchised in the Peasant War were quite extensive, the notion of "common man" does not seem to be a step towards, but rather away from, precise social analysis.

The dissertation of Robert H. Lutz, prepared at Bochum University under the guidance of Ferdinand Seibt, who wrote a brief introduction to it, proves this, so to say, by default. Following the rules of textual criticism, the author set Ockham's razor to contemporary and later sources in order to establish who were (or rather, who were not) commoners. It is interesting to be reminded that *gemein* can derive from *Gemeinde*, common from community (or vice versa?), but this still does not address the central issue of social group conscience and cohesion. There are many worthwhile details in this study, nicely demonstrating and summarizing, in eye-catching graphs, how the upper and also the lowest strata of society (labourers, beggars, outsiders) did not seem to be included in the term *gemeiner Mann*. Lutz's thesis is that this agglomeration of commoners claimed, as something of a "low middle class" to use an anachronistic term, the status of an estate. To wit, the third. Save that one cannot help noting that the *Tiers État* was not the German craftsman and *Vollbauer* of the sixteenth century but the non-seigneurial commercial and urban population of eighteenth-century France. East of the Rhine there was (with few exceptions in some principalities) no third estate forceful enough to establish for itself a place in the *ständisch* monarchy and finally to be instrumental in replacing it by a new kind of state. It may very well be that the difficulty of establishing by semantic inquiry "who was the common man" stems from the very fact that there was not to be an Abbé Sieyès in Germany. Thus this laudable exercise in clarifying usages and meanings may not sharpen the tools of historical and social analysis but in the last resort may help to build some kind of a Federal German variant of Whig history.

Scribner and Benecke express their hope that their volume will not only enhance the readers' knowledge about the Peasant War but also lead to further debates on the issues of its history and its study. Lutz has also demonstrated that inquiries into words and their meanings may yield fruits (even though I am sceptical about his harvest). Topics that would deserve comparative treatment, in regard to other times and places, should include the role of ideology (in particular, religion) in revolts, the relations between kinship, family, property and social tensions, and the conspicuous problem of the Peasant War: regional resistance, centralized retaliation. The list can be augmented, of course.

The SB collection also contains a good map, a chronological table and an index. The translation of the often difficult German texts is excellent: they make good English reading, which is a rare delight. To quarrel about the selection, regretting the absence of one's favourites and taking exception to less appealing pieces is a tedious exercise; I beg to refrain from it. Finally, Allen & Unwin deserve special thanks for having brought out a reasonably priced paperback edition — even from your reviewer, whose somewhat similar collection, published

by Cass only in an outrageously expensive hardbound format, was thus easily priced out of classroom use.

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JAMES B. WOOD.— *The Nobility of the 'Election' of Bayeux, 1463-1666. Continuity through Change*. Princeton, Princeton University Press, 1980. *Xiv*, 220 p.

L'auteur de ce livre entend, à partir de l'exemple de l'élection de Bayeux, de la deuxième moitié du XV<sup>e</sup> au milieu du XVII<sup>e</sup> siècle, montrer la faiblesse des thèses habituellement admises au sujet de la noblesse française d'Ancien Régime: l'affaiblissement en nombre et en influence de l'ancienne noblesse d'une part, et l'antagonisme profond qui aurait divisé la classe noble de l'intérieur, opposant familles d'ancienne noblesse et familles nouvellement anoblies d'autre part.

Cette étude est basée principalement sur les « Recherches de noblesse » — il y en eut cinq entre 1463 et 1666 —, sur certains « Aveux de fiefs » et quelques rôles de la taxe du ban et de l'arrière-ban. Il est quelque peu étonnant de ne pas trouver dans la bibliographie le manuscrit Nouv. acquis. franç. 12394 de la Bibliothèque nationale (Paris), qui contient les arrêts du Parlement de Normandie relatifs aux preuves de noblesse, de même que l'édition de P. Devillard et M. Norrier de la Recherche de Roissy qui complète l'édition de l'abbé Le Mâle interrompue en 1919 (*Cahiers Léopold Delisle*, 9 (1960) et 11 (1962)).

J. B. Wood tente de prouver d'abord que les familles nobles et spécialement les familles anciennes connurent au XVI<sup>e</sup> siècle une expansion démographique sans précédent. Ce groupe social en croissance, il serait faux par ailleurs de l'imaginer coupé en deux par la division classique « noblesse de race — noblesse de fonction »: l'ancienne noblesse, en effet, détenait en priorité certains offices et les nouveaux anoblis, quant à eux, abandonnaient souvent leurs fonctions antérieures tôt après leur anoblissement. En fait, c'est la richesse qui constitua la véritable ligne de clivage entre les familles. D'ailleurs, les actes de mariage montrent que les nouveaux anoblis réussirent jusqu'à un certain point, à s'allier aux familles de vieille souche en y choisissant leur femme; en revanche, c'est à un moindre degré seulement que l'ancienne noblesse faisait de même. Ces familles anciennes ne furent pas, autant qu'on l'a dit, victimes d'échecs financiers. En petit nombre, elles se maintinrent au sommet de la hiérarchie économique et les faillites, qui ne manquèrent cependant pas, se firent généralement plus à l'avantage d'autres nobles qu'à celui des bourgeois. On chercherait en vain les traces d'une invasion de la bourgeoisie dans la campagne bayeusaine. Puissante et riche mais ébranlée par un pouvoir royal toujours plus envahissant, une fraction de la noblesse trouva dans l'adhésion au protestantisme une voie idéale d'affirmation de soi et d'opposition à la centralisation du pouvoir qui l'atteignait jusque dans la définition même de son statut. Ce n'est cependant pas la noblesse qui fut obligée de s'accommoder des institutions en plein développement. Il faut ici, d'après l'auteur, renverser les termes de la proposition et comprendre que les institutions elles-mêmes ne purent se développer qu'en fonction d'une noblesse influente et vigilante.