dant, toute tentative de systématisation risque d'être prématurée. Tels sont par exemple, le rôle de la ville et de l'économie de marché, le poids des corporations de métiers, l'évolution des mentalités et leur rôle dans le processus d'accumulation.

Par ailleurs, et bien que l'A. se défende bien de faire de l'histoire régionale, il n'en demeure pas moins que ses données concernent toutes la Normandie orientale et elle seule. Ont-elles valeur de modèle pour les autres régions de France et pour le reste de l'Europe? Rien n'est moins sûr. L'A. peut bien récuser l'empirisme, railler les historiens qui croient à l'importance des études régionales pour la construction éventuelle d'une étude globale, il doit bien finalement admettre lui aussi que «l'effort d'abstraction et de généralisation n'a de sens que s'il prend appui sur la masse des matériaux que l'investigation historique se donne précisément pour tâche d'accumuler» (350). Dès lors n'est-il pas prématuré de définir déjà le «féodalisme» comme un système où domine la petite propriété paysanne? Dans un ouvrage bien connu portant sur une tout autre région de l'Occident, Witold Kula a vu, au contraire, dans la grande propriété foncière l'unité fondamentale de production du système féodal? Une définition doit-elle nécessairement chasser l'autre? Il semble que l'auteur le voudrait bien car la Normandie sur laquelle il travaille fait figure selon lui, «de secteur de pointe sur les plans technologique et démographique». Elle fait partie d'un ensemble géographique où la féodalité a vécu de la façon la plus complète, et est partie intégrante de ce «peloton de tête que l'on peut considérer comme typique des campagnes de l'Occident médiéval» (13). On est porté à se demander ici si l'A. n'a pas succombé à un certain attendrissement à l'égard de «sa» région. Sur ce point, la démonstration que la diversité médiévale ne peut être soutenue que par une «problématique désuète», n'est pas convaincante.

En définitive, parti à la recherche des lois régissant l'économie féodale, ce livre n'atteint pas vraiment son but. Mais, en cours de route, il offre beaucoup de suggestions intéressantes, provocantes, qui devraient stimuler la réflexion des historiens de l'économie.

Denise ANGERS,
Université d'Ottawa.

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JEAN-PIERRE LABATUT. — *Les noblesses européennes de la fin du XV^e^ siècle à la fin du XVIII^e^ siècle.* Paris: Presses Universitaires de France, 1978. Pp. 184.

This brief book is essentially descriptive and comparative rather than explanatory and interpretative. The author, who has given us a substantial volume on the seventeenth century French dukes and peers, not surprisingly concentrates on France, but he includes the European aristocracies from Russia to England in his study. He uses no footnotes and judging by his brief bibliography he did not penetrate deeply into the history of the aristocracy of any country except France; four books and two articles suffice for England. Nevertheless, the book is not without value or interest.

Professor Labatut asserts that the nobility of what we would call the early modern period differed from the nobility of other epochs. He then proceeds to discuss the traits of this unique class in a topical fashion. In the eighteenth century he finds that 15 percent of the Poles, 7 to 8 percent of the Spanish, 2 to 3 percent of the Russians, and 1 percent of the French were nobles, but he offers no explanation of why this should have been the case. Furthermore, although the European nobility as a whole was organized in a hierarchical fashion, it evolved in an exceptional manner in Russia because of the concept of service, in Poland because of the concept of noble equality, and in England because the gentry, though noble [sic], was easily confused with the bourgeoisie. Labatut also believes that the position of the French nobility of the robe was at variance with the European norm. To resolve the dispute between Bluche, who asserted the equality of all nobles whether of the robe or the sword, and Mousnier, who saw a marked difference between the two based on social condition and *mentalité* (though not legal status), he suggests that in addition to robe and sword families there was a third group of families that embraced both robe and sword. He seems to regard this as a 17th and 18th century phenomenon, but I suspect that a significant percentage of the more important offices were held by men of noble ancestry as early as the 15th century.

In the second part of the book Labatut deals with the fundamental values of the European nobility. Here he stresses the pride of birth and the pursuit of glory that characterized the class. Though there were individual exceptions he sees the nobility as being endogamous. In Eastern countries the crown protected the nobles' property, but even in the West where it did not, he believes that their financial difficulties have been exaggerated. He also quite properly attacks the cliché that the nobility managed their estates in an incompetent fashion. In the third part of the book Labatut argues that as time passed there was a tendency for the most marked differences between nobles to be effaced and for the order as a whole to become a closed caste except in England. This rejection of the leading members of the third estate at a time when there was a growing egalitarianism among the intellectuals, he sees as one of the causes of the French Revolution. Before this happened, however, the nobility had become a truly international institution whose leading members shared a French-inspired common culture and were welcome in the various courts of Europe.

Perhaps the most severe criticism that one can level at the author is that he centres too much on the upper nobility when he deals with the values and relative unity of that order. It is my impression that minor nobles were less wedded to the pursuit of glory, less inclined to engage in affairs of honour, and more certainly less cultured. His almost total unwillingness to explain why the nobility of the various countries differed and why the nature of the order changed is annoying to say the least. The economic and other changes that were taking place in Europe during the period are largely ignored.

J. Russell MAJOR,
Emory University.

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PETER CLARK. — *English Provincial Society from the Reformation to the Revolution; Religion, Politics, and Society in Kent, 1500-1640.* Hassocks, Sussex: Harvester Press, 1977. Pp. *xiii*, 504.

Peter Clark's *English Provincial Society* is one of the first in the well established genre of shire studies to treat the early sixteenth century as well as the latter part. Equally rare is its analysis of the role of the towns within the shire. Even if it had not been well done, Clark's work would have had to be considered as important; happily for us all, he has written a good book as well as an important one.