

Book Reviews

H H LAMB, *Weather, Climate and Human Affairs*, Routledge, 1988. xiv + 364pp. 9 plates; 120 line drawings. £60; J M GROVE, *The Little Ice Age*, Methuen, 1988. xxii + 498 pp. 166 line drawings; 57 plates; £85.

The likelihood that recent increases in the greenhouse gas concentrations in the atmosphere will cause significant changes in global climates has generated a growing interest in the study of climatic changes. The atmosphere is naturally highly variable and before it is possible to predict likely climatic consequences of increasing atmospheric greenhouse gases it is necessary to know the range and understand the causes of this natural variability. Thus to predict future climatic trends it is essential to understand the past. The most important impacts of climatic change are those on agriculture. This was recognized fully when the United Nations Environment Program and the International Institute for Applied Systems Analysis funded an international integrated climate impact assessment for the agricultural sector, the results of which are published in the volumes edited by Parry *et al* (1988). Again to understand the impacts of future climatic changes it is necessary to understand the nature of past climatic impacts. This is an area where the Climatic Research Unit, University of East Anglia, first under the direction of Professor H H Lamb and now under Professor T M L Wigley, has been particularly active.

Very large climatic changes have been observed in the geological past but they are not particularly relevant to the study of the changes taking place now because boundary conditions are not the same. That is, the distribution of land and sea or large ice sheets was different in the past. The most relevant period of study is therefore the recent historical period. During this period the largest and most interesting variation was from about 1430 to 1850 and is commonly known as the Little Ice Age. Historians differ about its dating, because the long cold period was punctuated by warmer decades and the effects differed from place to place. The Little Ice Age and its consequences are discussed in the definitive work by Jean Grove. In contrast, the book by Lamb is a wide-ranging collection of essays covering much of historical time.

Weather, climate and human affairs by H H Lamb is a collection of essays. Six of the chapters have not been published before. The rest, taken from a variety of sources, have been thoroughly revised and brought up to date. The book is divided into three parts, with the chapters concerned with climate and society in the prehistoric and historical past grouped together in Part I, as nearly in chronological order as possible; Part II deals with the mechanisms

of weather and climate and their changes; and Part III contains one chapter which looks at the climatic future of the earth. There are many illustrative case histories of how societies lived in climates that differed from today's and in climates which had become erratic or were changing significantly.

Lamb is at his best when describing the history of climatic changes. He starts his book with a chapter on long-term geological changes but the bulk of the book is about climate and history over the last thousand years. There is a wealth of detail about climatic changes. Thus for western Europe Lamb establishes temperature levels for the warmest and coldest times including the medieval warm epoch around AD 900-1300 and the Little Ice Age in more recent centuries. Important case studies include climate and life during the Middle Ages in the mountains of Europe, transgressions of the sea along European coasts, the Great Irish Potato Famine of the 1840s, and African drought. There is a sample sequence of daily weather maps for the summer of 1588 partly based on observation data reported by the ships of the Spanish Armada. Part II contains interesting chapters on birds and wind circulation, weather fronts, and a particular interest of Lamb - volcanoes and climate.

The style of the book makes it more suitable for the general reader than the professional scientist, and as such it should appeal to many people interested in history. Lamb has published several books and the question arises as to how this book differs from previous works. It updates and amplifies *Climate, History and the Modern World* (1982), but it lacks the detail of Lamb's major work *Climate: Present, Past and Future*, Volume 1 (1972) and Volume 2 (1977). The person seeking an in-depth study of climatic change over the historical period should consult the latter.

Lamb takes a broad view of climatic history, in contrast Grove concentrates on one important element of that history - the Little Ice Age. *The Little Ice Age* is a major research work and in this respect should be compared to Lamb's major work. Grove considers that the Little Ice Age may be seen as beginning in the thirteenth and fourteenth centuries and then, after an interval of more clement conditions, culminating between the mid-sixteenth and the mid-nineteenth century. It was also a period of lower temperature over most if not all of the globe, sufficiently marked to have had important consequences, especially in certain sensitive areas in high latitudes and at high altitudes where conditions for plant growth and agriculture are marginal. Indeed, in late medieval times and in succeeding centuries, the impact of climatic fluctuations was felt most painfully and persistently in highland areas.

Historians sometimes not only overlook but deliberately discount the influence of climatic change on human affairs. This attitude may be explained in part by historians having made use of climatic chronologies that are now known to be incorrect. Some early climatic historians were less rigorous in their use of sources than is now required and they often made use of compilations and other secondary material, some of which was unreliable. This is not true of recent work, such as that by Jean Grove.

The early chapters of Grove's book present a history of the Little Ice Age based on the records of glacier advance and retreat in northwest Europe. Information is then brought together about the behaviour of glaciers in this period in other mountainous parts of the world. The Little Ice Age is set in the longer time perspective of the Holocene, the period since the time of the last glaciation, and finally an attempt is made to assess the significance of the research presented for an understanding of human affairs. This last chapter is reporting the results of climatic impact research along the lines of that in Parry *et al*, *The impact of climatic variations on agriculture*, (2 vols, Dordrecht, 1988). Indeed it is a pity that the book does not contain more on the agricultural impacts of the Little Ice Age. The book is scholarly, with a wealth of data contained in many diagrams, and contains an extensive bibliography listing all the main sources.

Both books are expensive and this may limit their sales. Lamb's book should be published in a cheaper form as a paperback, it would then sell well. Grove's book is a major research text and as such should be in the library of every institute studying climatic history.

J G LOCKWOOD

A-G HAUDRICOURT, *La Technologie Science Humaine. Recherches d'Histoire et d'Ethnologie des Techniques*, Maison des Sciences de l'Homme, Paris, 1987. 343 pp. numerous diagrams. 230 Fr.

This is a volume of collected essays and extracts by the polymathic André-Georges Haudricourt. His training in genetics, botany, linguistics, folklore and geography are reflected in his flair and insight, based, as it were, on skilled detective work that integrates his learning into a consistent ethnological outlook. As a practical man, he believes firmly that the inventory, the listing of things and along with them their names, and description of the inventory items in their contexts, should precede the construction of theory.

For him the inventory should also include the human being along with the whole assemblage of movements and actions of everyday living, which he believes are not genetically inherited but are part of the social milieu in which the performer finds himself. The essays frequently touch on muscular

movements as the primary techniques which, when extended through the use of handled tools, foot-operated levers and pedals, etc., lead directly from the human being into technology. This is one of the bases for his view, strongly supported by François Sigaut in his 32-page preface, that technology is a social science, part of the cultural patrimony of mankind. Both argue strongly that 'technical culture' is in no way a contradiction in itself.

In exemplification of the point, Haudricourt ranges widely in time and place. He looks at forms of transport by man and beast, and seeks to show the evolution of harnessing equipment as burdens came to be pulled rather than carried on the head or back. The drawing of ploughs and ards by animals forms part of the story. The evolution of such equipment is seen in terms of function and geographical determinism, and he prefers a classification of types that does not depend on purely structural criteria (like three- or four-sided frames). In this context, the book he wrote jointly with Mme Jean-Brunhes Delamarre, *L'Homme et la Charrue à travers le Monde* (1955) gives an even fuller expression of his ideas and ideals. Alongside the question of tools and equipment, he also considers the domestication of plants and animals, invariably making strong use of linguistic data as an auxiliary in tracing origins and movements. For him, Europe is almost a promontory to Asia, a recipient area for early agricultural influences, including certain kinds of equipment, as much as an area of origination.

This is a stimulating book by a writer with a lively mind. It makes the reader think; the question of whether or not there is agreement on all points is secondary. But the essential plea is for a proper awareness of technology, the science of productive forces based on techniques, (i.e. knowledge of the human actions or acts necessary to attain desired results), as an autonomous discipline.

A FENTON

MARIE-CLAIRE AMOURETTI, *Le pain et l'huile dans la Grèce antique: de l'aire au moulin*, Centre de Recherches d'Histoire Ancienne Volume 67, Annales Littéraires de l'Université de Besançon, Paris, 1986. 376pp. 9 maps; 34 figures; 41 plates. No prices stated.

The title of this work does not reveal its central theme, which is a discussion of the 'blocages des techniques', that is the problem of the apparent lack of technological development in the ancient world. As Amouretti admits, this is something of a peculiarly French obsession. One thinks immediately of Lefebvre des Noëttes and of the 1935 edition of *Annales*. Not that it has been entirely ignored by other scholars and, indeed, it is a subject worthy of wide attention. There is a useful debate still to

be had between agricultural historians of both the classical and medieval periods and economists concerned with why capitalism fails to 'take off' in many under-developed countries. Amouretti notes correctly that the agrarian systems of Classical Greece have received considerably less detailed analysis than those of the Roman world. Her approach owes much to those scholars, such as André, White and Kolendo, who have so illuminated Roman agriculture in the last thirty years. Her decision to concentrate on cereals and oil and to omit viticulture, the third of the Mediterranean triad, seems eccentric – all the more so since the production of wine involved the gradual development of kinds of vine and the introduction of some new technology which so neatly supports her overall thesis. Her main target is those French historians who see no significant development between the eighth century BC, the period of Hesiod, and the fourth century, the time of Theophrastus (she recognizes that Anglo-Saxon scholars have been far less dogmatic). Her approach is to examine in turn the techniques of cultivation, set against the geographical and meteorological background, the production of bread and oil, and the manpower involved. The careful analysis of Greek terms for agricultural implements and processes has an antiquarian flavour about it, but performs the same sort of useful task as K D White has done for Roman farm equipment. Amouretti is quite skilled in her use of the scattered literary evidence. She also produces a variety of interesting comparative material from other periods, although both here and elsewhere in the work there are significant omissions in her reading (Gordon Hillman's demonstration of what can be learned from plant remains about the processes involved in gathering and treating corn would have been a help). Although she asserts the importance of archaeological evidence, she shows little knowledge of the important results of recent field surveys in Greece on land use. Her conclusion is sensible and unsurprising, that the classical period saw slow adaptation and development rather than major technological advance. It might have been more exciting, if she had enlarged on her acute perception that the failure to use technological advances in agriculture, even when they are theoretically available, is not a sign of lack of rationality. Faced with the geography of the land and with the climate, the ancient farmer had a limited range of choices available and it was far from clear that the expenditure on rotary mills, water wheels and the like would certainly increase his income.

JEREMY PATERSON

A EVERITT, *Continuity and Colonization, The evolution of Kentish settlement*, Leicester UP, 1986. xxi + 426 pp. 16 maps. £47.50.

There is a growing dilemma in the history of early English settlement. As the evidence for agrarian continuity from Romano-British if not prehistoric times accumulates, continuity of population, if more debatably of occupation and the organizing structures of settlement is indicated. Comparatively full exploitation of the landscape is pushed further and further back. Yet almost a millenium later, there is clear, documentary evidence for new settlement and internal colonization from the twelfth century onwards. The dark centuries between are becoming a crux of settlement history. Were they static, maintaining relatively intact the pattern produced by the end of the Roman era, a long prelude to the new colonization and population pressure of the high middle ages? Or was colonization and/or population growth continuous? If so, was it clearance of secondary woodland, of land abandoned during population recession at the end of the Roman period? Or has the degree of early permanent land-clearance been exaggerated: was the scope for internal clearance still large? It is to this period and dilemma that Everitt addresses himself, to argue for both continuity and colonization, but allowing a major role for the latter between the seventh and eleventh centuries. If this exceptionally fine study leaves me still open-minded, it is a sign of the intractability of the problem in the current state of the evidence.

The early Kentish landscape and economy emerges clearly. It was an economy dominated by woodland and pasture, by transhumance from Foothills to Downland and Marsh, from Downland and Foothills to Weald. The resulting landscape was one of dispersed settlement lacking common fields, it and its social structure set apart from the fielden areas of England, though Everitt's wide-ranging comparative conclusion blurs such simple divisions. The sharp regional contrasts within Kent forbid any general picture of either exceptional wealth or high population. The role of London in shaping its early economic development is especially questioned: settlement and its extension progressed in a north-east/south-west direction, early centres of wealth were in the eastern foothills rather than the west of the county. Soil, land use, settlement pattern, parish and manorial formation and social patterns all are combined in a reading of the landscape. The Weald and Downland were both areas of woodland, colonization and transhumant pastures, but differ in age of settlement and relations with the parent community. The late-settled Weald is characterized by huge parishes and continuing links with remote parents, it was a society of freedom and yeomen. The early-settled Downland was more dominated by close parent settlements, but became an area of separate manors and parishes, the basis for a prosperous gentry.

The Downland is crucial to Everitt's argument,

as the area of transhumant forest pasture allegedly colonized between the seventh and eleventh centuries. A range of evidence converges to indicate early settlement and clearance: the *variety* of place-names for wood and pasture, the siting of churches close to the boundaries of parent parishes, small daughter parishes which were nonetheless of sufficiently early formation to produce daughter parishes of their own. Settlement here was early, but can it be dated? Kent is unrivalled for early charter evidence. 24 per cent of places named in early eighth-century charters were in the Downland, a proportion which had steadied at 36 per cent from the ninth century onwards. Settlement advance, as Everitt claims? Or, given the size of samples and the vagaries of production and survival of documents, an indication that the Downland was already well-settled by the eighth century? Charter, place-name and parochial evidence on which modern studies rely may indicate changes in settlement, but equally in ownership, ideas of property, the nature of ecclesiastical provision. Until historians have elucidated these areas and settlement archaeology been extended Everitt's thesis is possible, highly plausible but unproven.

PAULINE STAFFORD

GRENVILLE ASTILL and ANNIE GRANT, eds, *The Countryside of Medieval England*, Basil Blackwell, Oxford, 1988. 282 pp. 50 illustrations. £35.

Landscape studies have been riding a wave of popularity ever since W G Hoskins' pioneering work was published over thirty years ago. More than twenty county volumes are now available in the *Making of the Landscape* series, and their success has been in presenting academic research to a wider audience in a clear and uncomplicated manner. Clarity and scholarship are both hallmarks of *The Countryside of Medieval England*, but its perspective is slightly different from most landscape texts. Instead of analysing the forces which have formed and shaped the landscape, Astill and Grant are concerned solely with the methods of *utilizing* the landscape, with the management and exploitation of rural resources in the Middle Ages. In ten readable chapters, the book considers the whole gamut of agricultural resources, from land and labour, to tools, plants and animals. The seven contributors, all established scholars, are supported by a battery of maps, diagrams, aerial photographs, and examples from illuminated manuscripts.

Astill and Grant have deliberately drawn upon a wide range of evidence and expertise, from archaeology to botany and history, and the result is a refreshing, multi-disciplinary approach to the medieval countryside. With admirable clarity and brevity, the ten essays present the main outlines of each subject, highlight existing controversies, and point

the way for further research. As such, the book provides an excellent synthesis of existing scholarship on a wide range of subjects, and should prove popular and useful reading for undergraduates. The essays are generally well-balanced and structured, and those by Dyer, Smith and Langdon are outstanding. Dyer provides an invaluable summary of the documentary sources available to medievalists, and their limitations, a subject on which little accessible work exists. Richard Smith's prowess as a demographer is well-known, and here he treads a clear path through the thorny problems of medieval demography.

There are few quibbles. Oliver Rackham has made a significant contribution to the history of the countryside, but there is hardly any reference to his important, recent work, and no assessment of its methodology or conclusions. Also, some sections are occasionally lopsided. For instance, there is a whole chapter on the management of woods and parks, but relatively little on meadows, heathland, or moorland, despite the fact that the latter covered a considerably larger area of medieval England.

This should become a valuable text-book. If it has a weakness, then it is born of the nature of countryside studies themselves. Any attempt to reconstruct the features of a 'lost' countryside must inevitably rely upon evidence thrown up by a variety of academic disciplines. Yet to assimilate all of this disparate evidence requires competence in a variety of skills and methodological approaches: unfortunately, our highly specialized academic system is ill-suited to producing such multi-disciplinary knowledge. The book side-steps this problem by inviting specialists to contribute on individual aspects of the medieval countryside: the archaeobotanist writes the chapter on plant resources, the demographer the chapter on human resources, and the archaeologist that on rural settlement. This is an acceptable compromise, but it can create its own problems: hence, the chapter on animal resources – by an archaeologist – is based primarily on evidence of physical remains, and largely ignores the mass of information available in manorial accounts. There is still a tendency to write about the countryside on disciplinary lines, and it is difficult to see how any genuinely integrated work can be produced until the study of the countryside itself becomes an integrated discipline. The book is constantly aware of this, but thus far Oliver Rackham stands as a rare traveller across disciplinary boundaries.

MARK BAILEY

ANTOINETTE FAUVE-CHAMOUX (ed.), *Évolution Agraire et Croissance Démographique*, Ordina Editions for Commission Internationale de Démographie Historique, Liege, 1987. 389pp. \$40.

This book consists of eighteen papers on the theme of 'Boserup versus Malthus', presented at a conference held in Stuttgart in 1985, together with two introductory overviews from the editor and Eiras Roel and two postscripts by van der Woude and Boserup herself. The purpose of the conference was to consider the impact of population change on agricultural development and vice versa over long spans of time and in very different geographic, cultural and institutional circumstances. The coverage includes countries in Asia and Latin America as well as the better known European examples and concentrates (although not exclusively) on the sixteenth to the nineteenth centuries. The emphasis of the papers is entirely empirical with very little in the way of theorizing or even explicit model building.

It is to be expected that conference volumes contain items which are distinctly uneven in standard, which stray somewhat from the supposed theme and which would in some cases be quite unacceptable in a refereed journal. The editor's task can be an unenviable one at the best of times in such ventures. Unfortunately in this case the outcome is distinctly unimpressive, the editor appears to have ruled with too light a hand, the variance in quality is excessive and the value for money is very poor.

Most of the papers are much too short to do any kind of justice to their topic and in any event usually are short summaries of already published work. In some cases these serve to whet the appetite through the provision of intriguing detail or an apparently persuasive insight. I certainly thought the work of Aymard and Delille on Italy, Eiras Roel on Spain, Gehrman on Schleswig-Holstein and Morin on Mexico/Peru seemed of high quality and worth my while to follow up when time permits. Too often, however, the other papers were only loosely connected to the main theme of the conference or were merely superficial or were badly structured. In particular, a self-serving and vitriolic review of some recent historiography on the notion of the Agricultural Revolution by Morineau not only leaves an unpleasant taste in the mouth but contributes nothing on the central issues. It also seems most curious to have four papers on Belgium but nothing at all on many potentially much more interesting countries.

The Boserup thesis that population growth induces technical change and more intensive forms of agriculture receives a good deal of sympathy and is regarded as offering potentially useful insights by many of the authors. At the same time, it is noteworthy that much stress on the importance of land tenure and on reactions to price changes emerges in the course of the volume. On the whole, as has been the case in British demographic history recently, there is considerable emphasis laid on preventive as opposed to positive checks in discussions

of the Malthusian side of the relationships between demographic and agricultural change. Both the Boserup and the Malthusian views are becoming 'bien nuancée' by historical research.

It remains to be seen how the research summarized in the papers of this volume really fits in or conflicts with formal models of population change and agricultural development. As both van der Woude and Boserup point out in their brief concluding comments, the lack of an appropriate analytical framework prevented the empirical orientation of the conference from making a serious contribution in this regard – in the words of van der Woude 'the idea behind this conference has been a source of misunderstanding. Ester Boserup and the historian may be regarded as two parties talking on different levels. They hear and understand, but cannot confirm or refute each other.' (pp 383–4)

It would be most interesting to have a major survey article of the kind of material contained in the researches of the authors of these papers looking at where the Boserup thesis now stands. Funding one such serious study would be an eminently better use of scarce resources than financing unproductive conferences.

N F R CRAFTS

ROGER B MANNING, *Village Revolts. Social Protest and Popular Disturbances in England, 1509–1640*, Clarendon Press, Oxford, 1988. xiii + 354 pp. £35.

At first sight Manning appears to have written a book of considerable importance for historians of early modern England. With justice it can claim to investigate a number of neglected areas in a revealing and rewarding fashion. Manning deserves to be richly praised for embarking on the work, but no matter how valuable the contents are, the volume finally disappoints. Sadly, Manning has written a book of chapters lacking a convincing common theme. After a general chapter on agricultural change, we have four chapters on enclosure and one on tenurial and rent disputes followed by chapters on vagrancy, apprentice riots in London and the rebellions of 1596 and 1607, then a chapter on 'social protest in pastoral and agrarian society' and a final chapter on poaching. A short conclusion follows. While each chapter has something interesting to say, the book fails to cohere.

What is the book about? The book, by turns, is not about village revolts, or social protest, or popular disturbance; nor is it solely about England, for it includes Welsh material. The inclusion of the London apprentices offers a clue, for Manning justifies their appearance on the grounds that the London crowd in this period was apolitical in its motivation. Rebellions with political aims – the Pilgrimage of Grace, 1549 (but strangely enough not 1607) – are

excluded because they are aimed at altering the policy of national government. 'Village' therefore means local. But many of Manning's instances are by no stretch of the imagination revolts. They include enclosure riots, rent strikes and even an entry into a close to provoke an action of trespass. Much of the book is barely about social protest. The gentry loom large, using violence to further their disputes over common rights and boundaries especially in the earlier part of the period; they are prominent participants in the chapter on poaching. This gives the game away. The book is largely a thematic discussion of some of the juicier cases from Star Chamber laced with a few from Chancery, dressed up in a framework of agricultural revolution and social differentiation. The theme of violence or revolt is as much a broken-backed fiction as the claims of violence which served to bring individuals before Star Chamber itself.

Chapters 1-6, 10 and 11 have a certain integrity as studies of the way in which agrarian change could result in acts of violence or litigation. Agrarian and local historians will find much to interest them here, if only for the instances of riot, oppression or friction to which Manning refers. But the problem with all these chapters is that Manning is writing about conflicts ripped out of the context not only of the history of the place to which they refer (except insofar as that can be reconstructed from the pleadings) but also of all those manors in which improvement, engrossment and the conversion of customary tenures into leaseholds went on without the notice of the courts. These chapters lead towards a more general account of lord-tenant relations within the period, but this cannot be written from the records of Star Chamber alone. To do so is to offer the equivalent of a study of the modern Anglican clergy culled solely from the columns of the *News of the World*.

The problem is that by basing the book on a limited number of closely worked out case studies from Star Chamber (and to a lesser extent Chancery), Manning raises problems of typicality which are exacerbated by the inadequate statistical underpinning to the book. His chapters on enclosure rest on relatively small samples, seventy-five from Star Chamber in Chapter 2 (1509-1548), 105 from a variety of sources in Chapter 3 for Elizabeth (the middle years of the century being passed over in silence). We are never told in detail how these samples were constructed or what proportion of known cases they represent. No allowance is made for changing patterns of litigation or for the existence of the regional councils. Consequently there are real problems in knowing how seriously to take the conclusions which emerge. Before 1548 the prime area for riots was the east Midlands but after 1558 the most disturbed area of the country became the

west Midlands. This may well be the case, but is it simply because the composition of the sample changes to include a number of cases drawn from the duchy of Lancaster records?

In retrospect the sum of the parts is greater than the whole. Manning's discussion of the events of 1607 is important. His chapter on poaching, although limited in range, is the first attempt to examine an interesting subject which cries out for a book itself. At times he says interesting things about the tightening of the definition of riot by judicial decision. There is much valuable material on rent strikes, on the ability of tenants to collect common purses and obtain legal counsel, again areas which are too infrequently considered, but no sustained discussion. The book will be heavily drawn upon for the richness of its cases. But it is inadequate in both vision and execution.

R W HOYLE

CHARLES W J WITHERS, *Gaelic Scotland: the Transformation of a Culture Region*, Routledge, 1988. xiv + 464 pp. Maps. £47.50.

The transformation of the Scottish Highlands during the eighteenth and nineteenth centuries is a theme which has received a good deal of attention in recent years. Dr Withers himself has made a notable contribution in his work on topics including the changing geography of the Gaelic language, and Highland-Lowland migration. Here he sets his detailed research within the wider framework of social and economic change in the Highlands and their closer integration with the rest of Britain. The chapters relating to economic changes, such as the one on 'Population growth and the transformation of Highland agriculture 1690-1891', are based almost entirely on secondary published work and stalwarts like the Old Statistical Account. Although the review is a competent one there is little here that has not already been covered by other specialists, notably James Hunter in *The Making of the Crofting community* or Eric Richards' magisterial survey of *The Highland Clearances*. Much the same comment can be made of the sections on the failure of industry to provide a more diversified economic base for the region. The sections which are strongest are those which relate most closely to Dr Withers' own research, particularly the ones on the Anglicization of the Highlands and on the Gaelic reaction to cultural transformation. These contain a far greater amount of original material and new insights.

Perhaps the greatest shortcoming of the book is its failure to examine in any detail the nature of the culture which was subject to 'improvement' and Anglicization. Although economic and social changes in the Highlands during the late seventeenth and early eighteenth centuries are reviewed, the

background against which they took place is not clearly delineated. The 'old order' in the Highlands, which is briefly described at the start of the book, is depicted as static through time, and geographically uniform. Traditional Highland society is treated as if its features are so generally known and have been so exhaustively studied that they require minimum treatment. In fact, as Robert Dodgshon has pointed out recently, while vast amounts have been written about Highland clans and the 'clan system' remarkably little is known about their actual structure and operation and the central role which kinship played in them. Because of the failure to explore in any detail the traditional culture of the Scottish Highlands it is difficult to find a yardstick against which changes can be measured.

Despite this, the book is a useful survey of social change in the Highlands. The author's focus on cultural geography provides a useful framework within which a wide range of themes is integrated. One unfortunate feature is the price which will put it beyond the reach of all but the more affluent institutional libraries. Given that the publishers felt compelled to charge so much for the volume it is a pity that it was produced from camera-ready copy. The dense, dark, slightly fuzzy print looks unattractive and is more conducive to eyestrain than enjoyment.

IAN D WHYTE

CHARLES H FEINSTEIN and SIDNEY POLLARD, eds, *Studies in Capital Formation in the United Kingdom, 1750-1920*, Clarendon Press, Oxford, 1988. xvii + 477. £48.

This volume has a dual role in the long-running game of attaching numbers to the amount of capital invested, or productively employed, in the British, or the United Kingdom, economy over the long period since the mid-eighteenth century. First, to resume and refine the enterprise of making discrete sectoral estimates which Sidney Pollard and J P P Higgins started at a memorable conference in Sheffield in 1969 in the bright dawn of the age of fire insurance valuations, tax-series proxies, and cumbersome computers. Second, to revise, refine, and splice into a single continuous time series the comprehensive, all-sector, estimates which Charles Feinstein has published for separate sub-periods in 1972, 1978, and 1982 in a series of well-known works which are the bibles of historical national income and capital accounting. The volume, accordingly, falls into two halves. The first, a collection of essays by individual experts on a selection of economic sectors treated over varying periods falling between 1750 and 1860, which look in some depth at coal, iron, cotton, wool, corn milling, steam engines, and roads and canals, as well as agriculture, but which do not

claim to be all-embracing: housing, for example, which has always been the largest item in the national domestic capital stock at around a quarter of the total value throughout the period 1750-1920, has not found a historian brave enough to mount a frontal statistical attack. The second, a substantial, closely-argued, and tightly-footnoted exposition by Feinstein of the sources and methodology he has used in estimating domestic reproducible capital, stocks and work in progress, overseas assets, and the unimproved value of land, which leads up to the presentation of 46 pages of tables recording the resulting estimates of capital formation and capital stocks in a continuous time series, by decades from 1760 to 1850 and annually from 1850 onwards.

This cornucopia of figures, swollen by the many smaller and intermediary tables which support the text in both halves of the book, will be seized by innumerable researchers as an inexhaustible quarry both to support general arguments about the dynamics of economic growth or inter-sectoral flows, and to put a price tag on a vast range of particular assets, whether a farmhouse in 1800, a windmill in 1816, a steam engine in 1820, or a cart in 1908. They should be warned that quarries are dangerous places, and that there is not a lot of firm bedrock in any of these figures. Feinstein's global figures are extremely plausible, and indeed he candidly describes a fair amount of nudging and fudging of some of the separate components precisely in order to ensure that the overall totals are plausible. The ingenuity, subtlety, sophistication, and careful cross-checking with which he has built up his series item by item, sector by sector, is unsurpassed and could not be bettered. The sharpest mind in the world, however, cannot raise the quality of the underlying data, and the underlying data are patchy, discontinuous, and often no more than contemporaries' hunches that chance to have been expressed in numbers as well as words. Just as the balance of payments figures in the 1980s are no doubt roughly right but can still only be made to balance by regularly inventing an enormous 'balancing item' because even the modern bureaucratic state is incapable of tracking down major monetary movements, so the historical capital formation figures are probably roughly correct but subject to wide margins of error in their individual components which the economic historian prays may cancel each other out.

The pitfalls of the numbers game are well illustrated by comparing the essay on capital formation in agriculture, 1770-1860, which B A Holderness contributes to the first part of this book, with Feinstein's own, independent, treatment of agriculture in the second part. Holderness has been in the business of quantifying agricultural investment since the 1969 conference, and has developed his tech-

niques for measuring farming or tenant's capital through the measurement of the output of crops and livestock both at the 1978 International Economic History Congress and in Volume VI of the *Agrarian History of England and Wales*. In the present contribution he provides a systematic evaluation of investment in farm buildings, enclosure and reclamation, fencing and underdrainage, woodlands, and working capital, using printed sources and secondary works published before 1980, when his chapter was written. His methodology is as conscientious and his results as plausible as those of Feinstein, but he uses different underlying data and hence comes up with different results. Holderness's figures indicate that the total reproducible capital in agriculture (valued at current prices) increased by 136 per cent between 1770 and 1860, while Feinstein's tables shows an increase of 283 per cent in the same period. For tenant's capital embodied in crops and livestock they arrive at more similar figures, an increase of 58 per cent according to Holderness and of 112 per cent according to Feinstein, which is not so surprising since they both start from similar numbers for crop acreages and livestock.

If there is any relationship between the thematic or sectoral chapters, such as that by Holderness, and the work on the national statistics by Feinstein, it is nowhere mentioned and the reader is left to guess that the two parts of the book proceeded in stately independence towards interesting but not altogether consistent conclusions. It is vaguely reassuring to know that the amount of capital employed in agriculture grew in parallel with, though not precisely in step with, the great growth in agricultural output until the 1870s, and then declined gradually – agricultural capital in 1914 was, in real terms, 95 per cent of its peak, 1887–9, level in the Feinstein calculations – while total output, changing in its product mix, remained fairly steady. The comfort, however, is almost purely technical and far from robust, since both capital and output estimates have a dominating common ancestor in the estimates of crop acreages, yields, and livestock numbers. Above all, the capital estimates are not sufficiently independent or accurate, in spite of the illusory precision of tabular statements, to bear the weight of any refined calculations of trends in the productivity of capital in farming. The spadework, economic logic, accountancy expertise, and mental gymnastics which have gone into these fabrications of historical statistics are to be saluted. Orders of magnitude have been established, which are authoritative, unlikely to be upset, and which are very important for aggregate economic analysis. The specialist agricultural historian, with an eye only for the agricultural parts of the estimates, needs to be extremely wary before trusting the literal accuracy of the detailed components of the global figures. The safeguard is that

the unwary is unlikely to shell out £48 for the problematical benefits of this experience.

F M L THOMPSON

CORMAC Ó GRÁDA, *Ireland before and after the Famine: Explorations in economic history, 1800–1825*, Manchester UP, Manchester, 1988. vii + 179 pp. £25.

This book is in a familiar Cormac Ó Gráda mould and the secret lies in the subtitle, 'Explorations' in economic history. This is achieved through five essays essentially on agrarian and demographic issues.

The background to the catastrophe which befell Ireland in the 1840s is reassessed in the first essay in the light of a catalogue of variables usually used to portray Ireland as the classic Malthusian country. In turn the author reviews and reassesses poverty, dependence on the potato, the degree of commercialization and regional differentiation in the economy, and of course overpopulation. We learn that potato failures were not uncommon, they were part of contemporary Irish life. But the 1840s witnessed two and then three back-to-back poor years. Therefore while Malthus could see famine in Ireland as a check on population growth – because this demonstrably happened anyway – he could not see it as a factor producing a sudden stop in population growth. At the same time we learn that population growth rates had slowed down in the thirty years or so before the Famine, demonstrating that the country was coming to terms with its headlong population. Overpopulation, therefore, was not necessarily a deciding factor in the excesses of the Famine. The conclusion is that 'The pre-Famine economy, for all its problems and injustices, did not contain the seeds of its own inevitable destruction by famine', instead – and this no doubt will cause a few raised eyebrows – 'Ireland was disastrously *unlucky* in the timing of the blight' (author's emphasis pp 34, 35).

All too often agriculture has been studied in terms of tenure, not least because of political considerations associated with absentee landlords, the middleman system, rack renting and so on. In the second essay Ó Gráda switches attention to the tillers of the soil and to their output and productivity either side of the Famine. His reassessment of the distribution of agricultural output refines two views of Irish agriculture: contrary to some recent assessments pre-Famine Ireland was not characterized by a switch towards pasture; and agriculture was less backward and more commercial, with a higher export context, than some would believe. Overall there may have been an increase of 80–100 per cent in Irish agricultural output in the half century before the Famine. Taking 1854 as a post-Famine benchmark Ó Gráda finds that the aggregate adjustment in agriculture

to accommodate the greatly changed circumstances resulting from the Famine produced a fall in output in real terms of only 17 per cent. A shift from tillage to livestock was striking and this continued down to the Great War. My one quibble with his findings having also looked at agricultural output is that 1854 is an unfortunate benchmark to choose, especially for tillage, and particularly for potatoes within tillage. There is a pronounced high point in the value of output that year and according to my estimation output was not as much on trend as Ó Gráda assumes.

The third essay is on the Famine itself. Non-Irish specialists might be surprised to learn that remarkably little research has been conducted on such a monumental event. In one chapter Ó Gráda can hardly rectify this but he does offer various new estimates and insights into old debates. He discounts accusations of artificially high prices due to hoarding. Food shortage was real enough and this is an important point in rebuffing political/ideological accusations of genocide. 'Food availability *was* a problem; *nobody* wanted the extirpation of the Irish as a race' (author's emphasis p. 122).

In the fourth essay, on long run agricultural change, Ó Gráda establishes that Irish agriculture did not perform poorly. Instead there was an increase in output and productivity and a redistribution of returns to the non-landlord sector. He offers an antidote to those who suppose that agricultural backwardness is explained by the much debated Land Question, by Irish indolence, and the lack of technical change. If you substitute increasing output and productivity for backwardness then you must substitute for the traditionally held brakes on efficiency. What we see instead is a ready and rational response to economic forces.

The final essay is on post-Famine inheritance, emigration and fertility. Was the Famine a watershed in Irish inheritance customs marking the move from partible to impartible inheritance? Was the long held belief in Irish backwardness a result of egalitarian provision for all siblings and did it give way to selected and therefore inequitable inheritance? Using a narrow but intensive regional sample (Cavan and Cork) Ó Gráda uses wills to identify estate succession c. 1900 and overturns a number of long held views: physical subdivision was rare and it was more common among the rich than the poor; sibling rank order is no sure guide to inheritance; the inheritor was destined to accept late or non-marriage, but yet general fertility remained high; and emigrant sons were not so much disinherited but rather their fathers recognized that they made a successful fist in the promised land (as witness the size of remittances which flowed back to Ireland) and that more was owed to those who stayed behind and worked the soil.

All in all these are extremely thought provoking essays and are sure to make an immediate impact on Irish research.

MICHAEL TURNER

DONALD HARMAN AKENSON, *Small Differences: Irish Catholics and Irish Protestants, 1815-1922: An International Perspective*, McGill-Queen's UP, Montreal, 1988. xiii + 236 pp. \$29.95.

D H Akenson is not only perhaps the most prolific scholar at present writing on the history of modern Ireland - twelve books to date plus four related works of fiction - but is also rapidly becoming one of the most interesting. Whereas some of his earlier books were long, thorough, and rather dull, his more recent publications have become shorter, no less thorough, and increasingly exciting both as to content and technique.

In *Small Differences* he tackles one of the great 'truths' of Irish prejudice and historiography, a truth more often acknowledged than examined, namely, that Irish Protestants and Irish Catholics have long been and are today fundamentally different in almost every sense. Their social aspirations differ, their patterns of settlement as emigrants differ, their attitudes to the land, to farming, to making a living, indeed, to economic behaviour generally, are, according to this accepted wisdom, marked by a gulf of impassable distinctiveness. And hundreds of books and thousands of articles concerned both with the Irish at home and (above all) the Irish in the United States attest to the fact that such differences have long been universally consigned to that category of things which 'everybody knows'.

In a book of considerable power and analytic bite Akenson, with an iconoclastic verve which obviously gives him considerable satisfaction, lays a sharp axe at all of this. He manages to do so by utilizing the evidence provided by various collections of statistics which have hitherto been shamefully ignored by scholars with similar interests. The most important of these sources are nineteenth- and twentieth-century census data from New Zealand, Australia, and Canada - all countries to which large numbers of Protestants and Catholics emigrated and all countries which, unlike Ireland, Britain, or the United States, published official materials which make it possible to examine immigrant communities over long periods in statistically valid ways or in, as Akenson would say, 'clean laboratory' conditions.

The conclusions which emerge from this process are startling. It is shown that Irish Catholic emigrants did *not* become disproportionately a city people and that they cannot, therefore, be seen as huddling together in Boston and New York because their supposed technological backwardness and their 'traumatic' experiences of farming at home had led

them to shun the agricultural sector. Indeed, both Catholic and Protestant emigrants (and the latter were by no means few in number) behaved in much the same way. Certainly there is little evidence that religious differences as such placed Catholics lower in the occupational scale either overseas or in Ireland itself. Everywhere Catholics showed a notable enthusiasm for farming, so much so, that in Australia, for example, they can be said to have 'exhibited a veritable obsession with the acquisition of land and livestock'. If in the United States Irish Catholics (as well as Protestants) in the late-nineteenth century became an increasingly urban group, this is simply because the whole population of America was rapidly moving in a similar direction.

Akenson's arguments are invigorating and deserve a careful response, though no doubt there will be weeping and gnashing of teeth among those who have made heavy political and scholarly investments in the accuracy of traditional verities. Naturally more work on the sources will be needed. In addition, Akenson's last two chapters, in which he attempts to explain why impressive mental and practical stockades have of course been erected by Catholic and Protestant communities alike *despite* their seeming lack of substantial differences, require a good deal of expansion and refinement. But the importance of this book for modern Irish history in general (and also for agricultural historians who will be obliged to see several Hibernian aspects of their subject in new ways) cannot be denied. If it is unlikely that every detail will emerge unscathed – Thomas Larcom was not, for example, Under-Secretary in 1851 – it is certain that the historical landscape will never be the same again.

K THEODORE HOPPEN

ELEANOR and REX C RUSSELL, *Parliamentary Enclosure and New Landscapes in Lincolnshire*, Lincolnshire County Council, Recreational Services, Libraries, Lincoln, 1987. 224 pp. Maps. £9.95 + £2 p & p. This is the fourth and final volume in a series of books that Eleanor and Rex Russell have written on Parliamentary enclosure in Lincolnshire. Altogether they have summarized the enclosures of 131 parishes and mapped the open field systems operating immediately prior to, and the new landscapes created by, enclosure. It has been a considerable undertaking, but one for which all agricultural historians must be thankful, because here is the raw material which can be used to support or challenge any theory of enclosure. The Russells have shown that two- and four-field systems were at least as common as three-field systems in Lincolnshire, and that open-field systems were not directly related to the county's agricultural regions. The present volume examines thirty-seven enclosures in the former Lindsey div-

ision of Lincolnshire, and provides a wealth of detail on the enclosure history of the area, including the landed accretions of the better off landowners, the rural clergy, and the bishops of Lincoln and Carlisle.

The opening chapters of the book give a general overview illustrated with pertinent examples from the case studies which follow. These chapters look at the process of enclosure by legislation, the sixty-seven men who acted as enclosure commissioners and their twenty-four land surveyors, and the open-field systems operating in Lindsey prior to enclosure. The majority of the text consists of the series of case studies which present the evidence relating to each enclosure alongside Rex Russell's beautifully executed maps. In many cases the Russells can offer only summary evidence about the division of the land before and after enclosure, but one or two accounts are rather more detailed, most notably the long study of the enclosure of North Kelsey where twenty-seven years elapsed and endless meetings occurred between the Act of 1813 and the Award of 1840. This is the last of the series, but can we now ask the Russells to produce the general synthesis which they, and they alone, can offer on Lincolnshire enclosure? This reviewer knows he is not alone in hoping that there might be a fifth book in the series which pulls all the evidence together and offers a general study of Lincolnshire enclosure.

J V BECKETT

IEUAN GWYNEDD JONES, *Communities: Essays in the Social History of Victorian Wales*, Gomer Press, Llandysul, 1987. 370 pp. £14.95.

This volume offers a number of always perceptive, sometimes seminal essays. After dealing in Part One with the motivations underlying the mid-nineteenth century building of Anglican Churches, Professor Jones in Part Two examines the culture of the emerging South Walian industrial communities. He marvels at the range of cultural activities engendered by the industrial workers' own efforts, all against the improbable backcloth of a brutalizing environment. Fundamental in working this civilizing process, it is emphasized, were the Welsh-speaking chapels. In so underlining nonconformity's positive achievement, Professor Jones is nevertheless alert to the importance of the alternative 'pub' culture, though perhaps he too readily averts his glance from the drunkenness of pay-Saturday nights, the vulgarity of the portable theatres and the like. Also, while his emphasis that the chapels were the people's own creation is undeniable, at the same time he underestimates the contribution of the late nineteenth-century Welsh coalowners in financing chapel buildings.

Another feature splendidly conveyed is the variation as between one valley community and another. Fundamentally, the steam valley communities with

their single industry base differed from the more variegated occupational structure of the older, early nineteenth-century iron towns, while the latter, with no settled independent middle class, differed from the social structure of the old corporate towns like Swansea, Neath, Cardiff and Newport where a more developed middle class was in evidence. It was the inter-relationship between these differing social structures and the level of political activity which concerns Professor Jones in Part Three. After showing that whereas it was filth in towns which generated disease, in the countryside it was poverty which produced it, he proceeds to demonstrate that the implementation of government legislation concerning town improvements from the Public Health Act of 1848 onwards was dependent upon the social structure of the towns concerned: in those places where a middle class was well-developed far more was achieved in the way of providing improvements by the 1870s. In the towns, the working classes for their part accepted the dominance of their masters just as the rural peasants accorded deference to the squire; they did so, he convincingly argues, precisely because they were immigrants from the countryside.

In so far as the politicization of the lower classes was concerned, it was, perhaps surprisingly, in the Welsh countryside that the first challenge to old forms of illiberal dominance occurred. Professor Jones points to the differing role played by nonconformity as between town and countryside in determining that this should be so: whereas in the towns it acted as a conservative force and steered the working classes around mid-century 'into the paths of middle-class righteousness', in the countryside about Bala the supposedly 'conservative' Calvinistic Methodist denomination through its incipient middle-class élite of deacons and college lecturers mounted a radical onslaught against an anglican and anglicized oligarchy antipathetic towards nonconformist claims for civic equality. The sociology of rural politics which Professor Jones here serves up is fine. His insistence, however, that 'Nonconformist ministers in nineteenth-century Wales did not create out of their fertile imaginations imaginary grievances' is doubtful: arguably nonconformist Liberal leaders manufactured or at least deliberately heightened tenurial grievances in order to achieve political and religious freedom from an 'alien' landed élite.

DAVID W HOWELL

RICHARD LOWITT AND HAROLD LEE, (eds), *Letters from an American Farmer. The Eastern European and Russian correspondence of Roswell Garst*. Northern Illinois University Press, Delkallb, 1987. xxxii + 230 pp. No price given.

In 1959 the Soviet leader, Nikita Khrushchev, visited the farm of Roswell Garst an American midwestern

farmer. The visit was the culmination of several years correspondence between Roswell Garst and officials in the Ministries of Agriculture in Romania, Hungary and the USSR. The correspondence was sustained until Roswell Garst's death in 1977 and it survived events such as the invasion of Hungary, the Uz affair, the Cuban missile crisis and Khrushchev's dismissal. The volume of Garst's letters edited by Lowitt and Lee makes fascinating reading adding colour and proportion to the subject of socialist agricultural development in the post-war period. Garst's primary purpose was to persuade the Soviet Union and Eastern Europe to purchase seed grain, machinery, fertilizers and a host of other products from America. In developing his case to the Russians for such purchases he gave a detailed analysis of the problems of Soviet agriculture in the 1950s. He painted a picture of a grossly under-capitalized agriculture, operating within severe environmental constraints. The Soviet Union, he believed, was too cautious in its agricultural policy; it needed to adopt an ambitious programme of intensification in production and the development of rural infrastructure. The priorities, as he saw them, were to expand the livestock sector through increasing the acreage sown to hybrid corn and sorghums, the development of the chemical fertilizer industry, the improvement of rural roads and grain storage facilities and improved animal breeding. Garst's enthusiasm for new technologies in agriculture pervade his letters and he clearly believed that with the appropriate 'technological fix' the Soviet Union could rival the USA in agricultural production. He had little to say about the 'collectivist' form of agricultural organization that existed in the Soviet Union, criticism of which has formed the central theme of much subsequent American writing on Soviet agricultural problems.

The impact of his ideas on Soviet agricultural policy was much less than he imagined, but Roswell Garst was clearly an instrumental figure in forging agricultural trade links between East and West. In the correspondence dealing with the details of contracts and shipments of supplies the Soviets are shown as cautious and bureaucratic; the contrast with the American way of doing business could not have been greater. Despite numerous delays, which on occasions left Roswell Garst threatening to withdraw his help, a number of deals went through. Of these, the sale of hybrid corn seeds, much from Garst's own farm in Coon Rapids, Iowa, had the most immediate impact upon Soviet agriculture, providing Khrushchev with the raw materials for his 'corn campaign'. This campaign is normally treated in western literature as one of Khrushchev's many agricultural disasters contributing to his eventual downfall in 1964, but a reading of the Roswell Garst correspondence presents it in a more measured light and suggests that the policy needs to be re-

examined within the context of the technical advice Khrushchev was receiving at the time. One of the achievements of the campaign to which Garst drew attention was that the Soviet Union made the changeover from open-pollinated varieties of corn to hybrids in five years compared with fifteen years that had been needed in the USA. By contrast with corn hybridization, the recommendation that investment be made into developing a network of rural roads apparently fell on deaf ears. Today, such a programme of rural road construction is acknowledged by Soviet specialists to be a vital area for investment in Soviet agriculture.

Roswell Garst was a successful businessman and his interest in 'selling to the Russians' was prompted by commercial considerations. He was among the first in the West to recognize the market opportunities that existed in the East and he pursued that market single mindedly, overcoming the problems that arose as a result of mutual suspicions of the two great powers. Garst was no supporter of Soviet socialism, but he believed that trade with the eastern bloc was necessary in order to break down the barriers between East and West. He was the forerunner of the generation of Western policy makers who believed that through technology transfer socialist societies could be made dependent upon the West. By the 1980s this view had lost popularity as Western leaders realized that the power of technology and example was less than had previously been thought. Just as the birth of technology transfer policy was anticipated in Roswell Garst's correspondence, so the reasons for its demise are evident in Garst's lack of understanding of, and apparent lack of interest in, the organizational and ideological basis of Soviet socialism. The collection of letters in this book, therefore, provides much more than an account of agricultural developments in the Soviet Union, Romania and Hungary over three decades, which is fascinating in its own right, but is of relevance to an understanding of the development of the politics of East-West trade in the post-war period.

JUDITH PALLOT

A R DESAI (ed), *Agrarian Struggles in India after Independence*. OUP/India, 1986. xxvi + 666 pp. £22.50. During the prolonged struggle for independence, the Indian National Congress from 1922 repeatedly committed itself to freeing the peasantry, workers, and other depressed classes and castes from economic and political exploitation, and fighting both British imperialism and Indian feudalism. After more than a decade of independence, Nehru himself stated in 1959 that he was 'not prepared to envisage a future in which this vast agricultural population remains on the verge of subsistence. I would rather have a million revolutions in India'. However, one import-

ant Indian irony is that the sole rural revolution is a capitalist agrarian one under the auspices of socialist governments. The Indian state has pumped vast funds into the modernization of the agricultural economy, and has succeeded in radically – if disproportionately – increasing productivity. An almost equally vast legislative offensive to transform 'medieval' social structures and economic relationships has essentially failed, principally through the marked ability of larger landowners illegally to preserve their holdings, and élites of wealthier farmers to take incommensurably large slices of the profits of enhanced production. The former were aided immeasurably by their historical heritage – adaption to the demands of the imperial power – combined in the post-independence era with corrupt officials and inert bureaucracies. The latter's inflated productive capacity made them indispensable to both national and state governments. The result, according to Desai's introductory survey of the massive sub-continent, is two major forms of struggle. On the one hand the proprietary classes of rich farmers, economically viable middle peasants and the 'streamlined landlords', demand more concessions, greater facilities and multifarious assistance from the politicians. On the other hand, an exploding agricultural proletariat, and poor peasantries, have experienced an intensifying economic and political exploitation, intertwined with oppression in many forms, notably cultural, caste, religious and gender.

A combination of additional key factors, especially demographic growth and the dispossession of smaller peasant proprietors, has fiercely fuelled proletarianization. 'Medieval' survivals, quasi-feudal relationships between landlord and tenant are in part responsible, together with the perennial activities of the ubiquitous money-lender. The latter's capacity to compete with official agencies as the effective provider of capital – often in kind, seed and livestock – and the exaction of interest at rates of up to 75 per cent, reveals a remarkable tenacity. The statistics – in as far as they can be known – are incredible. Official surveys state that between 1960–1 and 1968–9 alone, there was a 16 per cent rise to 54 per cent of 'rural people' existing below the calculated 'minimum level of living'. Expressed numerically, this represents an increase from 135 millions to 230 millions in under a decade, most of them casually employed and seasonally unemployed. The weakest sections of the peasantries, and the landless, have an alleged history of apathy and acceptance – somewhat shaken by several contributors to this volume – and as one puts it, 'the peasantry has always remained a potential rebel . . . The role of the ruling party, whether it is the East India Company, or the British Government . . . or the Congress Party, is the same. It sends its armed troops to protect the village gentry and crush the rebels'.

In some senses this comment sums up the experience of agrarian struggles since 1947, including perhaps the most spectacular all-India land-grab movement which climaxed in 1970, when 335,000 acres are known to have been seized, at least twenty-six people killed, and allegedly 82,000 arrested. Many other movements, galvanized by the slogan 'land to the tiller', are, however, of restricted relevance; in fact, 'land to the tenants' is more appropriate, but this excludes many millions who have, by any definition, historical or legislative, no rights to land, because a combination of capitalism and caste systems has spawned a huge class of permanent field labourers. Increased militancy by this category since the 1960s has included targets commensurate with the slogan 'land for the landless', with attempts to seize cultivatable waste and forest, together with the 'excess' land held, despite legal ceilings, by many of the ex-zamindar classes. These campaigns have been diluted, on occasion, by thrusts to remedy other grievances, including forced labour. Moreover, the essential divisions between a proletariat and a poor peasantry, have posed many strategic and ideological problems for the communist movement in India, and provided one of the numerous ingredients in the irrevocable splits in the Communist Party in the 1960s and again in the early 1970s.

Desai's collection is dedicated to the 'anonymous grassroot activists and leaders carrying on heroic struggles of agrarian poor in India and third world countries' [*sic*]. It comprises two sections, each with

an introduction by the editor, one with eight articles composed from an all-India perspective, and the other with seventeen regional case studies. Several are written by activists, others by field researchers. Both categories of authors have been exposed to the very real personal dangers deriving from any involvement in these campaigns. 'It would have been impossible . . . to make a study of the peasant movement in . . . a village in Burdwan, without taking a chance of being murdered', writes one academic; another field researcher into Kakdwip Peasant Insurrection in the late 1940s, and one of the few contributors to provide detailed source references, notes that present political circumstances dictate that 'the name of this reporter cannot be identified' in print nearly forty years after the event. Readers of this collection should be forewarned that a combination of the precise political positions and allegiances of many of the authors, and other factors, including obvious linguistic difficulties, mean that it is not a consistently scholarly work in the traditional sense. It has its values, especially in the exposure of events and details of movements commonly and deliberately obscured by the Indian press and official agencies for political purposes. While some of the contributors do, as the dust cover claims, 'give a flavour of grass root authenticity', the apparent lack of a proof reader revealed by hundreds of silly misprints, aggravate the difficulties of wrestling with the often chaotically presented pieces constituting this huge volume.

ROGER WELLS

Notes and Comments

ANNUAL GENERAL MEETING, 1989

The 37th Annual General Meeting of the Society will be held at 9 am on Tuesday 4 April 1989 at Burwall's Conference Centre, University of Bristol.

Nomination forms for officers and members of the Executive Committee should be inserted in this issue of the *Review*. Nominations should reach the Secretary no later than Tuesday 28 March 1989.