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Book Reviews

Stuart Clark, *Thinking with demons: the idea of witchcraft in early modern Europe*. (Oxford: Clarendon Press, 1997.) Pages xviii + 827. £75.00.

Between 1450 and 1700 it made sense to believe in devils and witches. That, in essence, is the argument of Stuart Clark's monumental new study. Previous work on witchcraft and witch-hunting has laid much more emphasis on the prosecutions themselves, and on the motives of the persecutors and of society at large. Reductionist historians have played down the belief-systems, the 'demonologies', which justified and promoted the hunts. They suggested that the ideas found in treatises such as the *Malleus maleficarum* or Nicholas Rémi's *Daemonolatria* were either spurious cover for a cynical attack on society's underclasses or the ravings of pathologically diseased minds. In consequence few historians have accorded the literature of demonology serious study, save for Dr Clark himself. For some twenty years Stuart Clark has argued, in articles bearing titles like 'The rational witchfinder' or 'The scientific status of demonology', that demonological theory was an authentic and even a typical product of the thought-patterns of its age. This book offers a distillation of that research, within a cogently constructed and copiously documented overall argument.

The book's 44 chapters are divided into five major themes. The first argues that witchcraft and the demonic fulfilled a necessary role in early modern thought, by offering the mirror-image, the contrary extreme, the inversion or antithesis of everything that was rightly ordered in Christian society. The second section demonstrates that there was a 'science' of demonology: that the powers attributed to demons fitted into a logically coherent Aristotelian system of natural philosophy. In the third section theories of the demonic and witchcraft are located in the fashionable apocalyptic historical schemes of the age: the rage of demons and their servants was a sign of the end of time. The fourth part considers how magic, superstition and the response to witchcraft found an orthodox place in the works of religious pastoral guidance written by late-medieval churchmen, then by

their Protestant and Roman Catholic successors. The final section suggests, rather boldly, that witchcraft engaged with political thought: that to confront witches and the powers of darkness was a primary way in which a political leader could demonstrate his or her legitimacy. Witch beliefs, in sum, 'belonged' in the thought-patterns of early modern Europe: they were not peripheral or aberrant peculiarities. Many of Dr Clark's most significant texts are not manuals for witch-hunting at all: they are treatises, pastoral guides, pamphlets or even masques and plays, all attesting to the role of the 'demonic'. It should be added that Dr Clark does not mean that *every* system of thought in early modern Europe was equally apt to accommodate devils. His conclusion makes the more modest claim that devil- and witch-beliefs fitted in with neo-Aristotelian science, apocalyptic history, comprehensive rather than sectarian churches and the politics of charismatic monarchy: they did not belong with other, rival systems.

One other feature of Dr Clark's exposition deserves notice. He approaches his material from the standpoint of the most rigorous cultural and intellectual relativism. He rejects, almost militantly, the idea that any scheme of belief may be more 'true' than any other. Systems of belief are, he argues, only 'true' within the language and cultural reference-points of those who hold them. Presumably this approach is meant to deflect and disprove the comment 'But it must have been obvious that beliefs about the sabbat/witches' flight/visible devils were absurd.' To uphold this relativism, Dr Clark introduces a good deal of philosophical, structuralist-linguistic and sociological jargon: 'The fact that forms of deviance must be relative to the standards of a community makes us all exponents of labelling theory to some extent … But the prevailing view of deviance was precisely the one that labelling theory has sought to transcend – it was normative and correctional' (p. 553). It is an undoubted strength of this book that it engages so fully, and on the whole convincingly, with modern theories about language and society. However, the theorizing undoubtedly has made the book a good deal longer than it need have been. It also makes it unsuitable to recommend for all but the very best undergraduates. This is a book for the specialist.

It is also necessary to bring quite a lot of prior knowledge to Dr Clark's exposition. Because his purpose is to show that demonological theory fitted in with broader trends in contemporary thought, he takes the content of witchcraft theory largely for granted. Nowhere does he state clearly and comprehensively what witches, or even devils, were supposed to do, though there are scattered discussions (e.g. on p. 186). Nor does he identify a specific time-frame or range of authors whom he includes as writers of 'demonology'. To some extent this attitude is deliberate, because Dr Clark's case is that demonic theory suffused nearly *all* early modern thought. However, witch theory arose out of a much earlier tradition of writing on devils and magic; Clark implies that witch-literature can be dated from the mid-fifteenth century (p. 183), but he draws on earlier writers, such as Aquinas and Gerson, while some post-1450 demonologists, for example the popular preacher Geiler von Kaisersberg, are not mentioned. His primary interest lies with the demonological writings of Lutheran and Roman Catholic theologians of c. 1550–1650; other writers are invoked when relevant, but the criteria for selecting authors for comment are not made explicit.

Dr Clark argues, essentially, that there was a 'demonology' – a science of devils and the invisible world – which was largely *shared* among early modern intellectuals, regardless of when they wrote or what their confessional allegiances were. He therefore plays down the amount of controversy *within* the demonological tradition, save where this cries out for attention, as in the case of Protestants and Catholics accusing each other of demonic activities (pp. 138ff, 532ff). When he discusses sceptical writers such as Johann Weyer it is to emphasize how much common ground they shared with the demonologists (pp. 198ff); other potential areas of divergence or disagreement are either down-played (e.g. pp. 537–40) or not reported.

This feature of the book raises the worry that texts may have been selected to make them fit into Dr Clark's scheme. Sometimes the selection is almost misleading. On page 81 he cites Book 5 of Alphonsus de Spina's *Fortress of faith*, with its war between the city of the devils and the fortress of faith, in the context of a discussion of 'binary oppositions' and symmetrical opposites. Yet de Spina's scheme was not binary; devils were just one of four opponents of the faith, alongside heretics, Jews and Muslims. Again, on pages 573–7 he includes Martin Delrio in a discussion of the belief that magistrates were immune to sorcery. Delrio actually *rejected* the idea of magistrates' inviolability as 'a vulgarly repeated opinion, and one received even by some of the learned' (see *Magical disquisitions*, 1599–1600 edn, vol. ii, pp. 71–3). While Dr Clark's linguistic virtuosity is in general highly impressive, it is worrying that Boguet and Guazzo are only used in the unreliable translations of E. A. Ashwin, and that on page 193 Niels Hemmingsen's citation of the Aristotelian cliché that a menstruating woman could spoil a mirror by looking at it is mistakenly rendered as relating to 'delusions caused to the vision'.

Mastering such a vast body of material as early modern demonology is a huge enterprise, and Dr Clark has performed a most valuable service by his heroic struggle to make its structures and thought-patterns comprehensible. His survey, despite its bulk, ought not to be the last word on the subject, however, because his homogenizing approach is only one of several viable ways to approach it. More may need to be said about the detailed differences between schools of thought both before and after the Reformation. 'Demonology' needs to be detached from its specific linkage to the witch-hunts and to be seen in the longer tradition of Western Christian writing on superstition and magic. From this longer perspective the classic witch-hunting treatises may indeed prove to be more of a departure – even an aberration – than Dr Clark claims.

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Dirk Hoerder and Jorg Nagler (eds.), *People in transit: German migrations in comparative perspective, 1820–1930*. (Cambridge and New York: Cambridge University Press, 1995.) Pages xv + 433. £40.00.

Interest in the history of German emigration, which was once low, has increased greatly in recent years. There are now several research centres in German universities: at Hamburg, which pioneered (in Germany) the close study of migrations from origin to destination; at Bochum, where the analysis of 6,000 emigrant letters is under way; and at the Free University of Berlin, which has been particularly interested in German ethnicity in the USA and Bremen, and which is investigating mass emigration and Bremen as an emigration port. The re-awakening of interest in ethnicity in the United States has spawned several projects of which the largest – on German urban history – is, not surprisingly, located in Chicago. (A third of Chicago immigrants before the First World War came from Germany.)

This book, which was sponsored by the German Historical Institute in Washington, is particularly helpful because it introduces the non-German reader to the work of a range of scholars who had not previously published in English. Thirteen of the authors are based in Germany, six in the USA and one in Poland.

There are several detailed studies of particular nineteenth-century emigration streams: from Mecklenburg, from Frankfurt/Oder and from East Prussia (to Russian Poland). A unifying theme is that emigrants tended to be labourers who had capital (such as land to sell). The agrarian reforms of the early nineteenth century also figure prominently. On the other hand, partible inheritance was not significant.

Four chapters concern internal migration and emigration. In the best, which builds on his earlier work, Jackson examines migration in and out of Duisberg. (Duisberg has extraordinarily detailed municipal records, even by German standards.) Migration rates in and out were very high. There was some increase in the mean distance travelled with industrialization, but Duisberg did not attract immigrants from eastern Germany. This is further evidence against the old view that the industrialization of the *Ruhrgebeit* was the reason for the fall in emigration from eastern Germany. In addition, no evidence of stage emigration could be found. A study, by Meyer, of rural migration to Georgsmarienhutte (a remote company town in Lower Saxony) returns to a previous theme; the author argues that migrants acquired the wherewithal to emigrate by working in industry (the 'pure' form of stage emigration) but it was only those who owned small plots who could go to Georgsmarienhutte in the first place.

The large number of papers about female emigrants is welcome. Weaner contrasts the prospects of domestic servants in Germany with those in the USA. (Only Irish and Swedish women were more likely to work in domestic service in the USA.) Single German women were able to use domestic service as a way into American society, often via marriage. (A chapter on Swedish domestics reaches similar conclusions.) In a chapter on Jewish female emigration in the 1930s, Quack argues that Jewish women were less assimilated into German culture than men and more likely to be pessimistic about the future. Hence they were more likely to

emigrate. Once in the USA, the women bore the brunt of the adjustment process because, in the main, they had not been in work in Germany and their husbands could not find employment in America.

Despite the title of this book there is only limited international comparison in it. However, there are several papers that are not about Germany at all. An excellent paper by Walszek examines Polish emigration between the two world wars, drawing on what is clearly a rich Polish historiography, including a remarkable collection of individual-level data. Here we see a new twist on the familiar eastern European story: high returns, remittances and their effect (if any) on the local economy.

One of the editors points out the lack of public interest in the history of emigration from Germany, which he ascribes to German nationalism and which will sound familiar to British readers. The Germans expected to be welcomed everywhere, but were not very welcoming to immigrants to Germany.

The book does not encompass quantitative research on German emigration even when that work is accessible to historians. (This may have been an editorial decision.) But it is, nonetheless, an excellent summary of much of the current research concerning the history of German emigration and an essential starting point for further work on this important subject.

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J. Lee and C. Campbell, *Fate and fortune in rural China: social organization and population behaviour in Liaoning*, *1774–1873*. (Cambridge: Cambridge University Press, 1997.) Pages xxi + 280. £35.00.

This is a pioneering work. Up to the 1980s, the study of Chinese population history was largely confined to examination of aggregated population figures collected from various historical records. Since then demographic data recorded at the level of individuals, largely in the forms of census-type population registration and genealogical records, have increasingly become available and investigations into Chinese demographic history have entered a new era. This is the first book based on the examination of population registers made in mainland China before the twentieth century and it will have a considerable impact on the future development of Chinese historical demography.

The book provides a variety of insights into many aspects of social life and demographic behaviour in late imperial China. It consists of five parts. The first part introduces the ecological, sociological, and demographic setting of the population of Daoyi – a rural settlement in northeast China. The second part examines the demographic system in Liaoning, the area where the community was located. Household organization and population behaviour and 'banner' organization (a kind of military and social organization of the Qing Dynasty) and population behaviour are discussed in Parts 3 and 4. Finally, an epilogue addresses the implications of the findings.

Basing their work on 25 population registers made between 1774 and 1873, the

authors have reconstructed demographic and family history for some 12,000 people who lived in Daoyi. By linking trends and patterns in nuptiality, fertility, and mortality to a number of socio-economic factors, the book reveals that complex demographic behaviour existed in the past. The proportion and age of marrying, time and frequency of having children, and length of life not only varied according to economic conditions such as fluctuations in the price of grain and differences in family wealth but were also related to such social factors as household structure, family position, and social status in the banner organization. The authors have also applied demographic techniques in the investigation of social organization and social mobility, where the demographic performance of each individual is used as an indicator. Their analysis shows that in this rural community, 'position in the traditional household hierarchy was dictated largely by heredity'. People were born with a set of entitlements and obligations, which had a very strong influence on their demographic experiences. Within the household system there was little opportunity for social mobility. However, 'there was considerable mobility outside the household in the banner hierarchies of occupation and organization' (p. 196). Even those at the bottom of the household and social hierarchy could better their fortune through personal ability and achievement. They could also subsequently improve their demographic performance. These insights greatly enrich our understanding of social organization and social mobility in late imperial China.

Another important contribution of the book is its detailed discussion of China's traditional demographic system. As far as demographic changes in historical China are concerned, the predominant view is still influenced heavily by the claims made by Malthus nearly 200 years ago. It was widely accepted that marriage was early and universal and thus played no role in constraining the increase of the population, and that traditionally the Chinese did not (intentionally) control their fertility. Accordingly, it was believed that population growth in historic China was primarily checked by mortality and that virtually no 'preventive check' existed in the past. This book is one of the few studies which have challenged this historiographic tradition. The authors show that Chinese women, in comparison with those in western European countries, married at younger ages but that the fertility rate of the newly married was much lower, the inter-birth intervals were considerably longer, and the age at which childbearing stopped was noticeably younger. More importantly, the Chinese were able to adjust both their marriage and childbearing behaviour in response to changes in socio-economic conditions. Their reproduction was under conscious control. These findings are of considerable importance in reassessing demographic systems in historical China and furthering our understanding of the origins of those equilibrating mechanisms, which balanced mortality and fertility and kept the population growth at a very low rate.

Like most other historical demographic records, the population registers used in the book are affected by under-registration problems, which is clear from Table 4.1 and Figure A.2. The under-registration is particularly serious among those under age 15, where a large number of females, and some males, are 'missing'. The authors have noticed this pattern, but they seem to have been convinced that this is 'a consequence of high early mortality and late registration' and that all the

missing children 'died before the mean age at registration, that is approximately six *Sui*' (pp. 231 and 66). However, evidence shows that this may need to be examined further. In their estimation of infant and child mortality, the authors suggest (in Table 4.4) that, depending on the actual mortality level, 57.8 to 68.8 per cent of females out of their birth cohorts may have not been recorded in the registers, and they all died in infancy or very young. Victims of infanticide 'accounted for between one-fifth and one-quarter of all girls born' (p. 69). If this was indeed the case, then it would lead to a very unbalanced sex composition – when a cohort reached age 15, the sex ratio would be close to 200, which is significantly higher than that actually observed in the adult population of Daoyi. According to Table 4.1 and Figure A.2, the sex ratio among those aged between 26 and 45 was slightly above 120.

This seems to imply that another type of under-registration (in which a considerable number of females were born and grew up in the village, later married out, and were never recorded in these registers) might have existed, because it is very unlikely that such an unbalanced gender composition would be found in any large and normal population. If under-registration of this kind indeed existed, and particularly if it was not distributed randomly through the population, then it might also have some impact on the data analysis presented in other parts of the book. The data show, for example, that the sex ratio of the recorded children of soldiers and officials is about 100, while in the children of artisans and commoners it is around 200 (pp. 182–3). This, of course, could be related to a number of factors, including those discussed in the book, but a qualification on the possible impact of under-registration of female children would certainly make the discussion more convincing.

In sum, this book, despite the foregoing limitations, is an important one. Anyone interested in historical demography and the social and population history of late imperial China will benefit from reading it.

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R. H. Britnell and J. Hatcher (eds.), *Progress and problems in medieval England: essays in honour of Edward Miller*. (Cambridge: Cambridge University Press, 1996.) Pp. xv + 317. £35.00.

It is a measure of Edward Miller's contribution to medieval English economic history and historians that a volume in his honour should range so widely and include essays by almost every significant historian of the field from his own generation and that immediately after. What is particularly nice about the volume is not only that there is very little here of the 'offcut' nature that *Festschrifts* can easily attract but that so much of the content takes further the exploration of fields that Professor Miller himself opened up or to which he made a major contribution. These include the economy of the twelfth century (King, Blanchard and Britnell); that of the north, an early instance of a regionalism that has become one of the

most exciting developments in the field (Blanchard, Britnell, Tuck, Kermode – although 'medieval' is rather an approximate term for her period); the cloth industry and trade (Childs, Fryde, Kermode); royal finance and credit (Nightingale, Fryde, Holmes and Dyer), and labour and tenants in the manorial economy (Raftis and Farmer). If John Hatcher's magisterial, if perhaps unduly pessimistic, survey of the mid-fifteenth-century economy picks up a theme owing more to Miller's own mentor, M. M. Postan, than to Miller himself, it should not be forgotten that the latter edited, and contributed substantially to, the late-medieval volume of the *Agrarian history*. It is also highly satisfactory that, whether intentionally or not, some of the essays complement each other, notably Blanchard's and Britnell's on the growth of the northern economy in the twelfth century, and those of Nightingale and Childs, whose pieces both touch, from different but mutually confirming perspectives, on London's cloth trade.

Reading this collection makes one realize that, despite the distinction of the rest of his œuvre, Miller's greatest single contribution to the subject may be his demonstration of the links between economic and political history. Political history is not a sphere whose importance economic historians are always ready to recognize. Nor is it one where, beyond some sometimes rather crude assertions about the impact of political events on economies, they are always much at home. But, in handling the interplay of governmental actions and economic developments, Miller eschewed such sloppy generalizations, and the same can be said of several of the articles in this volume, especially those by Blanchard, Nightingale, Tuck, Dyer and Holmes. However, if in this volume politics in the context of the medieval economy receive their due, social relations to a large extent do not. Only Dyer's important and thought-provoking examination of regressive tendencies in taxation within towns and villages in late-medieval England is written with a full awareness of the importance and complexity of social relations, in this case those that helped determine how local assessments were made. The most glaring example of the tendency of medieval economic and social historians to pass each other unseeing like ships in the night is Hatcher's article, where he touches on the landlord economy in the fifteenth century. Like Postan on the same subject, he treats it as something to be evaluated in global economic terms, while those he berates, such as K. B. McFarlane and this reviewer, have been interested not in whether this was a segment of the economy that was performing well or badly, but rather in what late-medieval landlords made of the somewhat unsatisfactory economic hand they were dealt in the period after the Black Death. If the general conclusion of almost everyone who has looked at this subject since the war is that – using a variety of strategies, economic and uneconomic – landlords mostly coped, that is what Hatcher himself admits. However, by insisting on the 'economic performance' approach rather than that of social and political context, he neglects key questions – such as whether lay lords were better placed than ecclesiastical ones at this time, or whether circumstances were easier for greater or lesser landlords – which are precisely those that one would like to see addressed by an economic historian surveying the landlord economy in this period.

Many would probably now agree that the economic history of this period cannot be separated from its social history, but this volume shows how much

vitality there is still in the study of the medieval English (or British, *pace* Blanchard) economy, both for itself and where it touches on, and is touched by, high politics. It ranges from the eleventh century to the sixteenth, from the macro to the micro, from royal and mercantile finances to the minutiae of village tax assessment, from the town to the country, from agriculture to trade and industry, from Edinburgh and Berwick to London, from kings to poachers. In its diversity and (mostly) liveliness of presentation and debate, it is a worthy reflection of its honorand.

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Reinhard Zimmermann, *The law of obligations: Roman foundations of the civilian tradition*. (Oxford: Clarendon Press, 1996.) Pages lxiv + 1241. £30.00.

When Reinhard Zimmermann's book was first published in South Africa in 1990, it was justly lauded as a magnificent piece of scholarship, as impressive for the range of its ambition as for its masterly execution. The book has three faces. On the one hand, it is a textbook of classical Roman law, which manages to summarize and build on a vast range of scholarship in a clear, accessible and highly readable manner. At the same time, it is a work of history, which follows the development of Roman doctrines through the era of the *ius commune* into the modern age of codes. Finally, it is a work of comparative law, for Zimmermann traces the impact of the doctrines into three essentially different modern systems, all of which have debts to Roman law: the German BGB (a codified system, based on civilian foundations), the South African Roman-Dutch law (a unique example of an uncodified Roman law system, mixed with English influences) and the English common law.

The book begins with a discussion of the concept of obligation, and how it was perceived by Roman lawyers, and gives a useful examination of how it was perceived by Gaius and Justinian. It then moves on to a treatment of the law of contract, which comprises the bulk of the book. Instead of beginning with abstract propositions, Zimmermann begins with the concrete. As he demonstrates, rather than beginning with abstract doctrinal thinking, classical Roman lawyers looked at specific transactions found in real life, and shaped their law accordingly. Thus, there were a number of forms of contracting which were recognized: the *stipulatio* (the formal verbal contract), the four real contracts (*mutuum*, *commodatum*, *depositum*, *pignus*), and the four consensual contracts (*emptio venditio*, *locatio conductio*, *mandatum*, *societas*). Each is given detailed discussion by the author.

The chapters which follow turn to a discussion of the more abstract questions which dominate modern contract thought, and which were evolved in the era after the intellectual rediscovery of the Digest: the formation of contracts, the effect of fraud and error, breach of contract and so on. Zimmermann shows that the modern general notion of contract, centred on the notion of consensus, was a development of natural lawyers and their successors; but they were able to raid the *corpus iuris civilis* for material to use, as they reconstructed the system. In these

chapters, Zimmermann traces what materials they used, and how these materials were used to different effects in different parts of Europe. The last third of the book deals with unjustified enrichment and the law of delict.

The book is at the same time an excellent guide to the Roman law of obligations and a stimulating discussion of the origins of the modern Western legal tradition. It is not a history of the civilian tradition as a whole; nor, however, is it mere teleology. In several fascinating passages, Zimmermann describes how medieval lawyers took the detail from the *corpus iuris civilis*, and developed a new theoretical approach, which in turn went down alleys which turned out to be blind ones for some jurists, but not for others. For instance, he shows how the formal *stipulatio*, which had formed the backbone of the Roman contractual scheme, while in theory a potentially excellent foundation for a general law of contract, became practically unusable in the light of Justinian's anachronistic handling of it; and instead, it was a generalized (informal) notion of *pacta* – agreements by consent – which the glossators sought to tease out of their texts. However, this, in turn, presented a potential problem in that it might allow the inexperienced to be drawn into contracts they did not intend: the *pacta* nuda, seen as unenforceable. To obviate this, medieval lawyers developed the notion of *causa*: 'an extra piece of garment, with which the canonists – charitable as could be expected – were prepared to save a poor and naked pactum from the chill of death' (p. 552). By 1700, as Zimmermann shows, most natural lawyers had abandoned this idea of *causa*: it provided only a bridge to the modern idea that every agreement begets an action. However, Pothier retained the notion of *causa*, and, through him, so does the French Code Civil. As Zimmermann says, in France we find a modern doctrine, with its origins in medieval lawyers using 'a few Roman bricks in order to build create a totally un-Roman doctrinal edifice' (p. 549). In a parallel development, he shows that the analogous English doctrine of consideration remains in place, in spite of many questions about its function.

For the lawyer, the book is full of fascinating insights into how modern doctrine was shaped, sometimes in line with, at other times in opposition to Roman doctrine. It reveals how, in many areas, Roman law still has much to teach, which was overlooked by the medieval and early modern filters through which it went. For the intellectual historian, the work is also invaluable; indeed, given that Roman law has suffused the Western legal and political tradition for over a millennium, it is essential.

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John Hudson (ed.), *The history of English law: centenary essays on 'Pollock and Maitland'*, Proceedings of the British Academy, 89. (Oxford: Oxford University Press, for the British Academy, 1996.) Pages xii + 228. £19.95.

Anthony Musson, *Public order and law enforcement: the local administration of criminal justice, 1294–1350*. (Woodbridge: Boydell and Brewer, 1996.) Pages xiv + 313. £45.00.

John Hudson, *The formation of the English Common Law: law and society in England from the Norman Conquest to Magna Carta*. (London and New York: Longman, 1996.) Pages xvi + 271. £13.99.

Perception of law simultaneously dffers for those using it, those subjected to it, and those administering it. It also looks different from the perspective of its purpose and analysis of its use. For some, its purpose is functional: the restoration of social harmony. Other historians perceive it as an instrument of social control ('gross coercion'), but some detect a 'multiple use right', allowing 'participation'. Postmodernists conceive law as part of relativist culture(s), its implementation having different meanings for diverse constituencies and within the same constituency. Moreover, law equates to power (knowledge) and the capacity to define and differentiate. Nonetheless, law has also been seen as an autonomous system of rules. Jurisprudential counterparts are found in legal realism, neo-legal positivism, critical legal studies, and the economic theory of law (rational choice theory). Regardless of ideology, one common way of seeing law within historical societies is as a resource, one among many strategies.

Centenary essays comprises eleven contributions to mark the publication of F. Pollock and F. W. Maitland's *The history of English law before the time of Edward I* (Cambridge, 1895). Most of the contributors start from the time-honoured principle that most of Maitland's *dicta* endure, despite the paucity of the source material available to him. Yet those who question Maitland attract more attention, firstly because they have a different overall philosophy of what was happening, and secondly because one contributor has the audacity to move beyond Maitland. Maitland was a late-Victorian liberal, influenced by legal positivism but circumspect about John Austin's formulation. Although a qualified barrister, he was perturbed by the idiosyncratic development of the Common Law and so adopted a comparative approach, *au fait* with continental research, but he remained essentially an English gentleman. Two consequences followed. Firstly, he was intent on countering Maine's communalism by emphasizing the individual. Secondly, his work, according to Paul Hyams, was produced for 'insiders' and could not properly address the historical questions of 'the other' or outsiders – Jews, women, those with different sexual orientation, and the margins.

Hyams is one of our foremost thinkers about the relationship between law and society and his contribution to this volume is especially meaningful because of his receptiveness to different ways of thinking. Although he discusses many constituencies of 'the excluded and the powerless' (p. 216) (including the Jews), his suggestions on 'The Gentleman and his Women' open fresh perspectives. Maitland had concluded that women could not be outlawed, 'for a woman is never

in law' (not least because outside frankpledge, although in the *mund* of a man, but thus no legal responsibility and lacking law; p. 228). Apparently, Maitland's statement, based on the treatises, has not been pursued, but Hyams brings out the meaning of 'weyve' and *waivetur* in the eyre rolls, the female equivalent of outlawry, which appeared by 1203. The possibility of women attending the [sheriff's] tourn and the rôle of female plaintiffs in trespass are also raised (p. 230). Such possibilities lead Hyams to ask: 'Did women, in short, combine like villeins effective civil rightlessness at Common Law with full "criminal" liability?' (pp. 230–1). Intriguingly too, he raises the insufficiency of female testimony as proof even when women were likely to be the only witnesses – in the case of proof of childbirth to establish curtesy (by which a widower retained his wife's lands for life). Proof depended on hearing the child's cries within the four walls or sight of the child. Although childbirth was the *secreta* of women, their testimony was not admissible.

'Law can be dynamic without lawyers or systematic lawgiving' (p. 18). Wormald's contribution to this volume is important not least since the Anglo-Saxon component of 'Pollock and Maitland' has usually been considered unsatisfactory. Although contributed by Pollock, it appears that the content was consistent with at least some of Maitland's thinking. Wormald brings together some of the points of his reinterpretation of Old English law. First, the *Leges Henrici Primi*, an 'archaicising text', needs to be used with the utmost caution. Putting aside the *Leges*, then, regulation of offences (the later criminal law) in the late Old English period had already moved from emendation (compensation – the interest of the victim – and thus purely personal satisfaction) to penalty (a 'public', that is royal, consideration). Royal interest in offences was reinforced by the existence of frankpledge and tithings (the organization of males over twelve for collective responsibility) before the Conquest, involving an oath of loyalty or fealty to the king, the breach of which was treason. Indeed, frankpledge may also have already required the presentment of malefactors. Finally, grants of judicial privileges by pre-Conquest kings conferred only fiscal rights – the profits of justice – not judicial competence. Overall, then, late Old English justice was constituted around the authority of kings.

In its concern to refute Maine's communalism, Maitland's account is so dominated by individualism that he 'exaggerated the ephemerality of medieval kinship groups' (Steve White, p. 112). Whilst Maitland consequently conceived of consent clauses to alienations to religious houses as determined by legal rules – the prospective interest of heirs – White suggests that values such as participation in spiritual services and association in kinship were important too. Maitland's predisposition towards the individual led him to confer on kinship only a 'fleeting' existence in pursuit of the Anglo-Saxon 'blood feud', from which White astutely recognizes the problem raised by Maitland: 'how were kinship groups formed and re-formed for such purposes as feuding or approving gifts of land' (p. 112). After all, each event probably affected different affines. In response, White invokes work on the feud in Iceland and anthropological 'action theories' in which individual agents recruit people into groups or 'quasi-groups' (p. 112).

In the remainder of the volume, Hudson, with regard to Maitland's insubstantial

treatment of the early twelfth century, raises the prospect of preparations for Common Law by the end of Henry I's reign (1135); Holt, in observations on Henry II's writs, suggests that law was commodified; Brand re-examines 'Bracton' in response to Barton's recent critique of Thorne; and Helmholz confirms how close to the mark were many of Maitland's intuitive suggestions about English ecclesiastical courts. Two rewarding articles present new information: Garnett on the corporate meaning of Crown and particularly Henry Summerson on criminal law. The whole volume is concluded by remarks by S. F. C. Milsom – 'a lawyer's retrospect' – reiterating the need to define over the *longue durée* 'when people's perceptions of things change' through 'shifting assumptions, analyses, perceptions' (pp. 251–4), no doubt largely as a result of client-centred initiatives, not policy.

Anthony Musson's book on law enforcement after the abandonment of the general eyres in 1294 invokes 'a new approach to legal history', reflecting the 'multi-dimensional character of the law' and 'interaction of law and society'. At issue are, firstly, experimental arrangements during the early fourteenth century, affected by competitive demands of and tension between politics and war, and the relationship between central justice and local society. Substantive issues include the inclusion of knightly society in administering local justice from the mid thirteenth century, and *pace* Putnam the complementarity of commission of the peace and assizes and the special commissions and an earlier consolidation of the commission's judicial functions.

Discussion of juries is particularly compelling, since they were at the heart of the issue of proof. The nub was their personnel: the extent to which local men were already cognisant of the facts (the 'self-informing' jury) had been replaced by men who were supposed to learn of the facts. Musson reveals that jurors presented cases in which they had an interest and might have initiated complaints for friends and neighbours (pp. 178–9). He demonstrates that there was continuity in panels of jurors, men necessarily from a 'relatively small pool' (p. 180), with the imputation of insufficient jurors (default of service) (p. 193), and notes that challenges to jurors were allowed, but jurors of presentment sat on trial juries (until 1341–1343) (p. 197). Jury nullification resulted from the severity of the general law of felony, not just homicide (p. 215); clemency might have been exercised during famine (p. 217); and ages of the accused were manipulated (below the age of criminal responsibility, defined as 12 or 14), if it was considered that the accused were 'too young to die' (p. 218). In the end, we return to the familiar notion that law was mediated by local sentiment and the sensible (if 'functionalist') suggestion that '[j]uries probably returned verdicts that would cause the least disruption within local society' (p. 222).

John Hudson's book is recommended as an introductory text on English law and dispute resolution up to the early thirteenth century. It is an entirely accessible account which nonetheless assimilates wider perceptions. Law is seen as a resource, but used alongside extra-legal recourses; one of its benefits was its predictive value. Narratives from the twelfth century are, although *ex parte*, a

valuable resource for dispute and law, even as at law the plaintiff's count or *talu* was tendentious. Felony was originally treason – breach of fidelity – but in the late twelfth century acquired its technical meaning of a serious crime in which the king took an interest. Crime as a legal category was constructed at that time, defined by royal interest in it, in contradistinction to just morality. *Leges* of the early twelfth century were 'archaicising' texts needing utmost care. Integrated into the account are Wormald's reconsiderations of late Old English law and Hudson's own revisions of the Anglo-Norman period.

Hudson's book will remain for some considerable time the most articulate introduction. More specialist in content, Musson's work raises important issues in early-fourteenth-century legal history. Specialists will undoubtedly have frequent recourse to *Centenary essays*, not least the papers by Summerson and Garnett, but it might be that their principal value will lie in what they tell us about writers on legal history and law and society.

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Timothy Lang, *The Victorians and the Stuart heritage: interpretations of a discordant past*. (Cambridge: Cambridge University Press, 1995.) Pages xiv + 233. £30.00.

At issue here is the seventeenth century's role as a mirror of nineteenth-century Anglican and Nonconformist obsessions, as one might expect in a Yale pupil of Frank Miller Turner. Dr Lang attempts to show that a reading of Hallam, Macaulay and Gardiner reveals a trajectory in Victorian historiography which carried the Puritan past into the heart of a nineteenth-century argument that rose to a crescendo in the years of Dissenting struggle and Erastian resistance and then died away when 'the English state lost its Anglican exclusiveness', allowing what had previously been a divisive and sectarian image of the Stuarts to relax into a more integrated vision of the national history.

Conceived on one level the thesis is plainly right: cultures that are themselves fixated on religious fission are likely to produce historiographies that reflect or transform that fixation. But one would also expect them to reflect more than one fixation, as Long acknowledges, and in this study it sometimes seems that a single concern has been made to work too hard as an explanation of historiographical change. The very first sentence of Chapter 1 – 'The Victorians wrote more on the Stuarts than on any other period in their nation's past' – leaves the reader more pensive than persuaded. Carlyle, apart from the edition of Cromwell? Stubbs? Freeman? J. R. Green? Lecky? Froude? Pollard? Names such as these cross the mind, without going further into second-rank scribblers of vast volumes now lost to memory, such as those of Archibald Alison, or turning toward those who chose to write about none of their nation's past at all but rather about Rousseau, Mme de Stäel or the roots of the French Revolution. Perhaps the project could more powerfully have considered how and why the focus shifted onto and away from the

Stuarts, along the lines of John Burrow's well-known treatment of Whig historiography.

There are compensations, none the less, in this well-written account. Situating Macaulay against a fabric of argument about religion puts him in an interesting place, for example, while Lang's depiction of Puritanism as a Nonconformist construct has its suggestive moments, especially perhaps in its treatment of Cromwell as a nineteenth-century icon whose intensity and function altered with context and whose symbolic significance as a heritage-item peaked at the tercentenary in 1899, when celebrating him became at once contentious and compulsory. It may be, indeed, that Lang's treatment is all we shall see of the promising 'Nineteenth-Century Cromwell' project launched by *Past and Present* in the days of the late Trevor Aston. Whatever *did* happen to that?

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David L. Smith, Richard Strier and David Bevington (eds.), *The theatrical city: culture, theatre and politics in London 1576–1649*. (Cambridge: Cambridge University Press, 1995.) Pages xv + 288. £40.00.

Laura Gowing, *Domestic dangers: women, words, and sex in early modern London*. (Oxford: Oxford University Press, 1995.) Pages ix + 301. £35.00.

In a celebrated article published some twenty years ago in *The British Journal of Sociology* Peter Laslett cautioned historians that they were in danger of looking at the past through the wrong end of the telescope if they carelessly based their work on literary sources without comparing them with non-literary ones susceptible to quantitative analysis. Many social historians continue to heed his warning and are loath to use imaginative literature as a source. However, things are changing; developments in the intellectual climate within both History and English Departments have begun to enable rapprochements between the two disciplines. Within Renaissance literary studies, for instance, new historicists and feminists advocate more historical modes of analyzing texts. Meanwhile some historians, such as Anthony Fletcher in his recent *Gender, sex and subordination in England 1500–1800*, have begun to use canonical works and literary scholars' commentaries upon them in their efforts to illuminate themes of social-historical concern. Moreover, in recent years other historians have begun to engage with the discursive structures of archival sources, either following what nineteenth-century scholars have christened the linguistic turn or exploring the implications of Natalie Zemon Davis's increasingly influential book, *Fiction in the archives*. The works under review here exemplify aspects of this historiographical trend. *The theatrical city* shows historians and literary scholars working together, while *Domestic dangers* brings sophisticated rhetorical analysis to early modern English social history.

The theatrical city originated in an interdisciplinary course taught jointly by its three editors at the University of Chicago. In the course David Smith lectured on

historical subjects and Richard Strier and David Bevington presented texts. The session on *The shoemaker's holiday* thus followed the lecture 'on the political, social and economic anxieties at the end of Elizabeth's reign' (p. 2). The published collection retains something of this organization: eight texts are each analyzed by one historian and one literary critic. There was clearly some real interdisciplinary exchange – several authors acknowledge the detailed commentary of the other member of the pair.

However, although the structure of the book and of the course offers an interesting model for future projects, *The theatrical city* does not demonstrate the power that its editors claim for its 'multiplicity and coordination of disciplinary perspectives' (p. 14). The collection begins well with Ian Archer's and Lawrence Manley's nicely complementary discussions of John Stow's *Survey of London*. Thereafter it loses its focus and degenerates into a series of studies of a rather odd miscellany of texts, concluding with the Grand Remonstrance and Milton's *Eikonoklastes*, neither of which is especially London-oriented or particularly theatrical.

Most importantly, there are fundamental problems with the ways in which much of the collection sets up the relationship between history and literature. All too many authors on both sides of the disciplinary divide treat 'History' as the *background* to the text. Thus Penry Williams unconvincingly bolts the Rude Mechanicals' play in *A midsummer's night's dream* onto the economic hardship of the 1590s, and Keith Lindley leadenly interprets Massinger's *A new way to pay old debts* as allegorizing the two conflicting images of society in early Caroline England. Yet, as Martin Butler elegantly points out in his characteristically perceptive discussion of *A new way to pay old debts*, this is entirely the wrong way to think about the relationship between drama and society. Drama was (and is) an integral and dynamic part of culture; it is part of the context. Moreover, as Patrick Collinson's splendid discussion of Ben Jonson's *Bartholomew Fair* demonstrates, dramatic satire of Puritans, through such figures as Zeal of the Land Busy, was itself a major cultural force, helping to construct the image of the Puritan. Literary stereotypes shaped common parlance and religious and political discourse.

The other astonishing aspect of this collection is that in seeking to bridge literature and history, virtually no attention is paid to old-fashioned literary and theatrical history, bibliography or the history of the book. Even though the book's titular dates refer to the establishment of London's first commercial playhouse in 1576, no one discusses the geographical and commercial setting of performances, nor does any contributor explore the possible relationship between the form and content of the play and the circumstances of the company performing it. The reason for this neglect may well lie in the misplaced assumption that relating literature to history necessarily implies relating literary texts to the grand narratives of social, economic and political history.

For many cultural historians either retain a residual neo-Marxist belief that 'great literature' necessarily refers to the 'fundamental transformations' going on at the time of its composition and performance, or argue in a more anthropological vein that the study of literature can reveal a particular society's preoccupations or even its cultural poetics. There is doubtless considerable truth in these claims, but

neither approach pays sufficient attention to what a Marxist might term the relative autonomy of literary production. If we are to treat literary texts in a social-historical way, then we need to reconstruct more precisely the particular social and intellectual contexts in which they were written and read, and not just to wonder with Leah Marcus 'whether Jonson's … lifelong abhorrence/fascination with matters scatological, had something to do with the fact that he grew up alongside a sewer' (p. 179). The histories of authorship, of theatre and of reading are surely places to begin, and sadly *The theatrical city* ends up reinforcing many of the disciplinary divisions which it sought to cross.

Laura Gowing's *Domestic dangers*, by contrast, happily reveals the possibilities of historical work which is interdisciplinary in its methodology. Impressively documented, elegantly written, sophisticated yet clear in its analysis, it is the best work of early modern English social history that I have read in years. Its focus is the ecclesiastical courts of Elizabethan and early Stuart London, and particularly the hundreds of cases about defamation and marriage which they heard over this period. She shows that the level of business before the courts rose dramatically in the late sixteenth and early seventeenth centuries and that this was caused by a remarkable increase in defamation cases, particularly defamation cases sued by women. Whereas many previous studies of the ecclesiastical courts have been primarily concerned with their effectiveness in regulating 'sexual morality', Gowing is more interested in the ways in which understandings of household, of honour, of reputation and, above all, of gender were articulated, contested and socially reproduced in the courtroom, particularly through the depositions of thousands of women and men.

The closeness with which she reads these depositions is particularly striking. Accepting that they are narratives whose veracity is questionable, she shows how deponents used words in order to position themselves in particular ways. If a woman called another a 'whore', she was not simply defaming her; she was also positioning herself as a guardian of public morality. In many cases, Gowing demonstrates, 'women might assert a claim to moral responsibility and a right to punish' through the words and gesture of insult (p. 102). Similarly, she explores the vividness and physicality with which courtship, adultery and other forms of sexual behaviour were imagined and represented, the emphasis placed upon objects exchanged or given, the locations within the house where intimacy was said to have occurred. She shows how such depositions crucially linked sex with the organization and management of the household.

Through this attention to the rhetorical strategies of insult and deposition, Gowing challenges (in a fundamental but often understated fashion) many of the analytical commonplaces of early modern social history. Categories such as honour, gender or morality are not represented as fixed and unproblematic social norms which were sometimes broken. Rather they were constantly performed and refashioned through repeated social dramas such as insult and prosecution. Individual women drew upon and subtly adapted such sets of moral prescriptions for their own ends and to create their own voice. Gowing further argues forcefully that the language of crisis is not the best way of conceptualizing notions of gender

relations – 'we might', she writes, 'usefully think … about gender construction and gender relations in early modern England as a perpetual conflict' (p. 275). Moreover, such conflict was, she argues, rooted in a powerful sense of the utter incommensurability of men's and women's sexual nature and their sexual honour – men's speech, she argues, showed 'none of the sense of guilty self-implication that characterizes women's defamations of men' (p. 74); a man could, after all, insult a woman by terming her 'my whore'. The honour and stability of the household depended on the sexual reputation of the wife. In a way that a husband could not, women constituted domestic dangers.

Impressive though the work is, questions and problems remain. Gowing has not been able to link depositions with other documents in order to reconstruct more of the neighbourhood context of some of her cases. Nor is she particularly concerned with change over time and with explaining the dramatic rise in the levels of court business; I would be interested to know if there were any changes in the organization of the courts or if proctors and other court officials contributed to the changing case load. Some critics will wonder whether there was not greater symmetry in male and female notions of sexual reputation than she argues, and others will doubtless worry about whether the depositions were not too shaped by court officials to allow the twentieth-century historian to identify women's voices in the way that Gowing does, but no scholars of early modern England will be able to ignore this powerful and very important book.

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Paul Griffiths, Adam Fox and Steve Hindle (eds.), *The experience of authority in early modern England*. (Basingstoke: Macmillan, 1996.) Pages vii + 331. £13.99 paperback.

This collection of essays is an interesting conceit. The non-alphabetical editors, all contemporaries at Cambridge, have invited five established historians and one of their contemporaries to contribute essays about the broad theme of the 'experience of authority'. The result is an invariably interesting and occasionally stimulating book to which the older hands contribute wide-ranging essays and the younger offer denser and more closely wrought pieces.

Ingram considers the reformation of manners in early modern England. His essay is helpful chiefly for considering the medieval antecedents of the Tudor and Stuart campaigns. Capp considers the interaction of women and groups of women with authority, both the authority of the spouse and the authority of the state in its local guise, arguing for a distinctive 'semi-separate female domain' which was largely closed to men and whose solidarities were both a resource and a refuge for women. Griffiths writes on the social problems of masterless young people in Norwich, some of whom were street children while others were simply trying to make a living out of service. Sharpe offers an account of witchcraft and possession to show how patterned such events could be and how they can be read as an adolescent rejection of the mores of the Protestant household. I remained a little

unclear as to how many were contrived acts of adolescent rebellion and how many reflected genuine psychiatric problems. Hindle considers the use of binding over as a means to keep the peace. This was a highly flexible instrument of authority which, one suspects, was probably resorted to only after the failure of communal and neighbourly reconciliation. Rule writes on masters and men in eighteenth-century manufacturing with much on the law of employment and ideas of contract. It is a useful essay but somewhat at variance from the others.

I propose to make more specific comments on the two chapters directly concerned with custom, an area with which I claim some especial acquaintance. I sense that the rediscovery of custom owes more to E. P. Thompson than to individuals such as myself who were writing on manorial custom a good few years ago.

Fox contributes a chapter on 'Custom, memory and the authority of writing' which considers manorial custom in the light of ideas about a conflict of oral testimony and text. Out of this Fox sees people 'attempting to transmute oral or ill-defined customs into written and codified documents… to provide themselves with what they believed to be the best means of advancing and defending their rights and interests' (p. 110). This is surely correct. My unease about this chapter perhaps begins with the jejune comment that 'In a variety of contexts, therefore, the courts came *increasingly* to give priority to written evidence over verbal witnesses when ever it was available' (p. 91, my italics). Is this merely anodyne or does it reflect an ignorance of the medieval equity jurisdictions? Then we have the belief that court rolls, surveys and custumals from the middle ages onwards 'set down the customary regulations governing many manors and boroughs' (p. 94). At the end Fox wonders whether 'this quickening process of documentation [which] was then taking place may be exaggerated simply by a survival of records much in excess of that of former centuries' (p. 110).

The first point that I would make is that custom was probably not a matter of contention, so not a matter of record, in the majority of manors until late in the sixteenth century. In the depressed demographic circumstances of the fifteenth century, custom may barely have existed. As I have argued elsewhere, the customary inheritance of tenants at low or nominal fines could well have been in the landlord's interest when tenants were in short supply. Likewise at such moments there was little interest in limiting the tenants' rights to commons and timber. The recording of custom is a dimension of the conflicts which arose when lords realized that custom limited their income from land at a time when it could be growing. Secondly, I would hold that 'custom' is a possession of the tenants. It is not to be found in the court rolls which, in their recording of land transactions, record the transfer of land and not the rules which took land in certain directions, rules which can only be discovered if the options open to tenants can also be ascertained. Hence the scene was set for conflicts, particularly in Chancery, between what the tenants *knew* went on and what the court rolls *said* went on. Tenants may have known that they had an inheritable estate and paid fixed fines; but the court rolls might say that they held *sibi et suis* and, over a long period of time, had paid fines at varying levels. Fox does not realize the sheer inconsistency with which Chancery approached the question of evidence. He cites

Lord Keeper Egerton saying that matters should be decided on the last sixty years' practice (that is, 'within the memory of man') and then shows him in a different guise (as Lord Chancellor Ellesmere) trouncing his tenants by having his counsel produce court rolls dating from Edward III onwards. The latter was a common landlord strategy when the records could be found and invariably showed a wide variability of practice over the late-medieval depression. Thirdly, Fox muddles matters still further by describing examples where the tenants could defend their rights by producing medieval grants to them. No custom is involved here. The idea behind this essay is good, but its execution needed a much deeper understanding of the records and the issues they describe than Fox displays.

Wood on free miners caused me similar unease. Partly the problem is that he seems to regard his free miners as a class apart. Early in his essay he claims that they had 'a rather different, yet highly sophisticated understanding of "the law" from that articulated by society's rulers' (p. 250). Nothing which follows substantiates that point of view. Indeed, I wondered whether Wood was so interested in seeing the distinctiveness of these people that he never realized that they were individuals protecting their property rights against aggressive landlordism and no different from yeomen farmers protecting their customary tenancies, common rights or their rights to dig stone, coal and even lead on their lands or commons. Everything in this chapter can be paralleled with regard to farming communities. Indeed, were these not farming communities practising a rather special dual economy? By page 275 I wanted to shout aloud that these people were petty capitalists who had a freehold control over their means of production. Wood swallows too much seventeenth-century counsel's purple prose about these people. It impedes his understanding. He should reintegrate his miners back into rural society.

It remains to consider what is a *de facto* introduction by Keith Wrightson. Wrightson begins with Professor Collinson's call (in his inaugural lecture of 1989) for 'a new political history', an 'account of political processes which is also social'. Wrightson then goes on to show how much recent work has already satisfied this need, that we already have a social history 'with the politics put back in'. The character of this new social history is revealed by the subheadings in Wrightson's chapter: 'The politics of patriarchy', 'The politics of neighbourhood', 'The politics of custom', 'The politics of reformation and state formation' and 'The politics of subordination and meaning'. My recollection of the lecture on which this chapter is based is confirmed by my reading of the text (one for Dr Fox here): that Wrightson is shooting past Collinson, and that quite different ideas of politics are involved. Wrightson writes of the political behaviour of local society. I don't doubt that this behaviour is 'political' just as the politics of my department are 'political' and that both are influenced by the politics and culture of the larger sphere. But Collinson was, I think, much more interested in the interaction of locality and state, which is no more than one would expect of the historian of the first great lobbying movement, the movement for church reform in the later sixteenth century. This book is a wonderful showcase for a certain form of history, but its narrowness is in some respects reflected by what is absent. There is nothing on rebellion (or civil war), compliance to taxation, political activity in elections,

the formation of party or Protestant non-conformity. This is the study of the internals of the local community. There is much less on the *externals* of the communities' relations with the state except when, as in the discussions of custom, the state (or its courts) mediates between contested claims of property rights within communities or, as in Hindle's chapter on binding over, the precepts of community prove inadequate to promote reconciliation. To me, this still seems to be social history with one sort of politics left out. Mercifully then, after the energies of Fox, Griffiths, Hindle and their contributors, there is still more to be done.

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Catharina Lis and Hugo Soly, *Disordered lives: eighteenth century families and their unruly relatives*. (Oxford: Polity Press, 1996.) Pages 230. £39.50.

Are the poor victims of the rich or masters of their own wretched fate? The latter point of view has been voiced, often ferociously, since the close of the eighteenth century in Britain and on the continent. Faced with exorbitantly rising charitable expenses, some authors blamed the poor and offered a simple solution: abolish poor relief, if you can, for it breeds more poor, or, when you cannot, lock the poor under the most undesirable conditions, which will teach them a lesson. It is perhaps an oversimplification to say that this view gave us the New Poor Law of 1834, although it helped, but it certainly gave us something more enduring. Paradoxically these opinions have given rise to the image of the poor as victims of cruel relief practice. Some intellectual currents reinforced this image during the present century. Such unlikely bedfellows as Marxist historians, disciples of Foucault and social scientists praising the welfare state each had their own reasons to distrust poor relief. Olwen Hufton's brilliant book on the survival strategies of the poor in France at the end of the Old Regime added to this by stressing the sheer inadequacy of formal relief and the importance of other, informal, survival strategies. In sum, most historians looked down on repressive and marginal poor relief systems. All the same one wonders how exactly poor relief could be both repressive and unimportant.

The 'history-from-below' movement with every new piece of empirical information on the lives of the poor has gradually undermined the view of the poor as victims. That, of course, was for most of its practitioners, its purpose: not to document the institution that produced nameless victims, but to flesh out the lives of ordinary *actors* in the past. Perhaps to no one's great surprise – at least in hindsight – these men and women turned out to be much like we normally see ourselves: with vices and virtues (victims typically are said to have only virtues) and adapting as best they could to changing circumstances (victims are not usually portrayed as historical actors but rather seen as objects of repression).

So to what extent were the poor really masters of their own fate? This, I think, is the wider intellectual context in which one may place this study by Catharina Lis and Hugo Soly of houses of confinement in four Belgian cities – Antwerp, Brussels, Bruges and Ghent – during the eighteenth century. Without process or

court order men or women could be locked up for long periods, if municipal authorities gave in to a request from family members. This was no unusual phenomenon, as the study by Farge and Foucault on the Parisian *lettres de cachet* and other studies had already demonstrated for France. In an Epilogue Lis and Soly present estimates of requests for confinement in France and the Netherlands, and draw on others in studies by Spierenburg, that range from 0.5 to 1.5 per thousand inhabitants of cities. Usually this share was on the rise after the middle of the eighteenth century, as it was in the four Belgian cities studied here.

In Belgium family members requested the confinement of unruly spouses and children for reasons of 'insanity', 'unruly living', 'work-shyness', 'prodigality', 'alcohol abuse', 'immorality' and 'assault'. The titles of chapters and paragraphs of the book are less euphemistic and speak about troublemakers from the *jeunesse dorée*, debauched daughters and little whores, street boys and scoundrels and marriages made in hell between desperate men and women who fought back. This list may serve to highlight a few conclusions by the authors. Since it was the family – and in most cases a poor family – who requested confinement, this form of social policy cannot be seen as merely an instrument of social control used by elites to 'discipline' (Foucault) or 'civilize' (Spierenburg) the poor. At the very least, these families used elite views on what formed disrespectable behaviour to their own ends. But masters of their own fate they still were not. This was not only so because the municipal authorities had to grant a request for confinement, or because the parish priest or the neighbourhood could – and often would – exercize considerable pressure to confine unruly living to the walls of a monastery or other house of confinement. According to Lis and Soly, the driving forces behind confinement were structural: the impossibility of tolerating certain behaviour when making ends meet in the face of rising unemployment, 'proletarianization' and rising bread prices. If the families in question did not immediately see the necessity of putting an end to the unruly conduct of a child or spouse, the neighbourhood could make it perfectly clear that it would not lend a helping hand in times of need, if such help went towards a drunken mother, an idle father or a debauched child.

The role of the neighbourhood is, I think, one of the most interesting topics not only in this book and in other work by Lis and Soly (see their essay in the *International Review of Social History* **38** (1993), pp. 1–30) but also in recent writings on social history in Belgium, France and the Netherlands. Will the debate on the causes of poverty here take a new turn? Are the neighbours to blame?

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Michael Mascuch, *The origins of the individualist self: autobiography and self-identity in England, 1591–1791*. (Oxford: Polity Press, 1997.) Pp. xi + 277. £39.50.

One of the more controversial topics in British social history has been the search for the roots of individualism in the distant past. Village communities, work-practices, economic policies and religious rhetoric have all been closely inspected for the first stirrings of individual voices or behaviour. *The origins of the individualist self* injects fresh energy and adds several fresh perspectives to this at times tired discussion. It is a splendid and challenging book which seeks answers to large questions in interesting cultural and psychological agendas.

Mascuch's objective is to uncover the 'secret' and 'immediate pre-history' of the development of autobiographical writing by charting 'the way self-identities take shape in the modern West as objects of representation in "autobiography"' (pp. 6, 7, 18). His quarry is the 'individualist self', defined here as 'a producer and consumer of stories about himself and other selves which place the self at the centre of the systems of relations…[quite] literally a writer and reader of modern autobiography', who has a leading part in constructing his narrative destiny (p. 21). We are taken on a journey through key moments in the development of autobiographical genres over two centuries, roughly 1600–1800. We begin with Philip Stubbe's tribute-narrative of his late wife's life (1591) and close with the publication of James Lackington's *Memoirs of the first forty-five years* of his own life (1791), which was, Mascuch notes, 'a unique cultural moment' and 'a novel instrument of self-identification' (pp. 6, 3). The ports of call on this journey are spiritual autobiography, the personal diary and notebook, nonconformist writing, John Dunton, the 'last dying words' of condemned felons, criminal biography, other forms of eighteenth-century biography and, finally, Lackington, 'the first modern autobiographical practitioner' (p. 24). Mascuch's narrative unfolds in a well-structured sequence of thoughtful chapters, and new turns in literary self-expression are connected to political and religious contexts, though more convincingly after 1660.

The point of departure for this book is the spiritual autobiography, a way of presenting lives that actively countered human 'individuality' and 'personal capacity' (p. 60). These pious works cautioned against spontaneous thought, emphasized self-mortification and depicted their heroes and heroines as mirrors of piety. In this sense, Christ was their author (p. 70). There were, however, movements towards a greater sense of self in these godly books. By the middle of the seventeenth century many of them included prose character sketches of their protagonists in more recognizably autobiographical forms, though they were little more than chronological outlines of major life-events (pp. 106–7). More godly folk also recorded their lives in notebooks and diaries at this time. Mascuch argues that 'we can observe the stirrings of a personal voice and an individual self-identity' in these regular jottings (p. 96). Yet this was a gradual, uneven and long awakening. The written thoughts of Richard Rogers are used here to illustrate the stranglehold of piety on these early notebooks: the narrative pattern of human life was still largely plotted by God; human voices are frequently silent and certainly of

secondary importance. However, Mascuch still believes that a sense of self was creeping forwards into greater prominence: he writes of 'a tendency' in these godly narratives and jottings 'to place the person before the piety' (p. 96).

The balance between individuality and piety is a principal theme of this book. In some respects its sub-text is another historiographical flashpoint: the appearance of the secular self. It is the loosening of the tight grip of God-centred motives on individual narratives that smoothes the path for more spontaneous self-expression or, in Mascuch's terms, the writing of truly human history, stories solely for and about people. Other thematic threads in this book include the decline of the role of providence and the new prominence of conscience and self in theological discourses in the second half of the seventeenth century. But Mascuch is sensitive to the double-edged nature of religious influences. This is not a simple story of tidy displacement: the case of nonconformist writing is introduced to show how religious writings could also help the autonomous voice to speak in clearer and louder tones. The potentially subversive implications of religious plurality after 1642 included greater self-expression. This was one unforeseen result of the Clarendon Code and other moves to silence post-Restoration dissent. One defensive strategy of the besieged dissenters was to write and collect 'small archives of personal historical materials out of which others might fashion a biography worthy of print' (p. 112). Richard Baxter called for more autobiographers and biographers to fight the corner for dissent. These first-person narratives mushroomed after the late 1650s and they followed an individualist turn: 'more personal, specific and concrete' in 'detail', 'more subservient to the order of chronology and the narratologic of cause and effect' in structure, more human 'history' than godly 'mirrors', these nonconformist biographies leave us perched on the edge of 'modernity by constituting the voice of an individual authoritative subject' (pp. 114–16).

This sense of self was slowly raising its head in religious discourses, but it was much more strongly summoned into existence by secular writing of the late seventeenth and eighteenth centuries and by developments in print culture. John Dunton is singled out as a massively influential secular voice. His *Life and errors* (1705) is called 'the most substantial early example we have of what was in its day an increasingly common form of personal discourse' (p. 136); his *Night-walker* (1696–1697) is called 'a seminal text in the history of modern autobiographical discourse' and 'perhaps the best early expression of a new first-person voice in written narrative, capable of assuming responsibility for the making of its own individual experience' (pp. 150, 160). These are large claims for what may well be fictionalized accounts of Dunton's 'evening rambles in search of lewd women', in which he retells the stories of the fall into sin of women out late at night. Nevertheless, Mascuch emphasizes the active, autonomous and secular voice of Dunton and his 'lewd women': narratives are no longer framed by providence; they are instead human histories and morality dramas.

Much the same momentum is also traced for criminal biographies and the scripted 'last dying speeches' of felons. A habit of 'secular confession' sets the tone for these life-histories by the eighteenth century (p. 164). Earlier efforts to write criminal lives were confined to providential interpretations of human action;

little space was left for human creativity. But new modes of thought reshaped later printed criminal lives; as in the case of pious biography, individual conscience and human responsibility for actions and thoughts caused the explanatory power of divine intervention to fade. Gallows speeches and their written records emphasize human agency; the ordinary of Newgate's *Account* was 'the immediate forerunner of the modern autobiographical tradition'. These texts and others which tell the lives of criminals, quacks, 'captains' and other shady figures 'represent the beginnings of a legitimate, fully secular mode of individualist self-identity in English autobiographical practice' (pp. 175, 188). 'The self as author' (p. 157) was also boosted by changes in copyright law that finally granted authors ownership of their texts, and by the larger presence and accessibility of print in the mental worlds of eighteenth-century England. The book was a fully blown confirmation of the identity of its author. By the middle of the eighteenth century an actress, a poetess and a courtesan were constructing discrete self-identities in biographical passages. By its close, James Lackington had published his 'unprecedented book'. Mascuch writes that at last 'the ordinary individualist self emerged in London' (p. 8).

This book is largely written as a sequence of chronological steps towards the arrival in print of its real hero, James Lackington. At times this journey through time seems too smooth; square pegs fit neatly into all of the holes. Mascuch is certainly aware of some rough edges and he carefully unravels the mixed contribution of religious writing, for example, to the sense of self. One testimony of the exciting and challenging nature of this book is the way in which Mascuch joins so many historiographical controversies where others might fear to tread. To be sure, several still hotly debated issues are arguably woven into his explanatory narrative too uncritically, such as the switch from the heart to the conscience in questions of salvation, the fate of providence or the timing of secularization. Moreover, the method behind this book is to move through decades stopping for considered analysis of certain key individuals and moments, and on occasion one might like more depth in studies of particular genres such as (especially) pious biography and criminal life-stories before 1640.

One much-asked question of textual historians is that of the audience, in terms of both numbers and reception. There are large gaps in the present state of our knowledge here, though the actions and voices of spectators at 'last dying speeches' are becoming more familiar. My point here is that I wonder whether, as Mascuch moves across genres and lets his spotlight fall on individual authors, we are also shifting between separate audiences. As we travel through time, are different constituencies, sentiments and moods being tapped by different authors for different purposes? One suspects that there would have been some crossover between the readerships of, for example, didactic godly lives, cheap criminal stories and the secularized life of a poetess. It is also true that new audiences are constantly being created by new genres and turns in content and purpose. The audience is clearly in Mascuch's mind: he writes that 'By reading biography, people … discovered the singularity of their self-identity, a dimension of personal experience previously unknown to them' (p. 101). Nevertheless, some thoughts on the social profile of readerships for changing genres would have been both

apposite and highly relevant. Are there occasional mismatches? Are audiences being constantly recreated from the same raw materials, the same constituencies? There is a sort of cohesiveness and interpretative neatness imposed on what might in fact be an otherwise muddled set of audiences and purposes. *The origins of the individualist self* is (perhaps inevitably) a very author-centred study. We can imagine ourselves into the minds of writers and count print-runs, but consumers of print can be frustratingly silent.

I also wondered about possible connections between the autobiographical self and other forms of expressing individual identities and styles at this time. Such media might include the dramatic self, the visual self or the self in cheap print. This book makes a massive contribution to the history of individualism and for this reason it would have been useful to see it more securely grounded in existing work on the historical self. Mascuch's deservedly prominent place in this debate is now certain, but when I had finished the book I was to some extent left puzzling over how he himself felt he fitted into more 'conventional' interpretations and chronologies of the rise of individualism.

Any work that pushes hotly contested debates forwards will always draw critical responses. That much is certain, and Mascuch has written his important book in a lively and provocative style so as to seek such constructive engagement with other scholars. *The origins of the individualist self* will emerge in time as yet another turning point in the hunt for the historical roots of English individualism.

PAUL GRIFFITHS
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Sheilagh Ogilvie, *State corporatism and proto-industry: the Wurttemberg Black Forest*, *1580–1797*. (Cambridge: Cambridge University Press, 1997.) Pages 513. £55.00.

Following hot on the heels of her edited anthology on *European Protoindustrialization* (Cambridge, 1996) comes this expanded version of Ogilvie's 1985 Cambridge PhD dissertation on the Wurttemberg worsted industry. This is both a long and an important book. It is informed by contemporary economic theory and applies quite sophisticated statistical measures to economic and demographic data, primarily drawn from the later seventeenth and eighteenth centuries.

Ogilvie draws a detailed – and convincing – picture of Wurttemberg proto-industry between about 1650 and 1797, during which time it operated within a 'corporatist' framework of both producer and merchant guilds. The result (at least after 1650) was a highly regulated, limited and conservative proto-industry which produced poor-quality cloth within a highly restricted market. The structure of monopoly, restricted access to the industry and rigid production limits rendered innovation next to impossible. In Wurttemberg this highly regulated and institutionally and economically conservative proto-industry did not 'break down traditional social constraints on economic and demographic behaviour', as the traditional proto-industrialization model propounded by Franklin Mendels and by Kriedt, Medick and Schlumbohm in the 1970s might suggest. In fact Ogilvie's

case study is an example of established proto-industry which is about as different from the traditional model as can be imagined: there is rural manufacturing for non-local markets, there are producers who make use of their own capital to employ cottage workers on a putting-out basis (although not like the full putting-out system seen in the sixteenth- and seventeenth-century English woollen industry) and there are capitalist merchants who buy the manufacturers' cloth. But beyond that any similarity with proto-industry as most English-speaking historians recognize it (in England and in the Netherlands) ceases. And that indeed is Ogilvie's point. In Wurttemberg, as in most areas of European proto-industrial production in the seventeenth and eighteenth centuries, state corporatism ('the symbiotic alliance in which an expanding state granted privileges to corporate groups, in return for their fiscal and regulatory cooperation') influenced and restricted economic and demographic development. Hence the original theory, that 'proto-industrialization broke down the corporate restrictions which characterized traditional institutions, and replaced them by unregulated capitalistic markets', as well as leading to rapid population growth and proletarianization, is incorrect for many if not most parts of Europe.

The majority of Ogilvie's book (Chapters 3–10) offers a very detailed, rigorous analysis of the Wurttemberg rural and urban worsted industry during the period 1650 to 1797. Chapters describe the social institutions of the region, the guild structures and how they were used to control labour and production. The chapter on population and the family shows how social and economic institutions ensured that there was little demographic growth: indeed in many of the parishes and towns where worsted weaving predominated the age of female first marriage was later than in nearby agricultural parishes. Rather than leading to larger families (which theoretically provided more hands to labour in industry) proto-industrial families in Wurttemberg had smaller families than did farming–landholding families. The Chayanovian model of self-exploiting, pre-capitalist outworkers – which the original proto-industrialization theorists adopted – is shown not to accord with the evidence of weaving households in early modern Wurttemberg. Ogilvie argues that proto-industrial families more generally were not 'pre-capitalist' and economically illiterate, but made decisions about marriage formation and allocating family labour on the basis of the varying opportunity costs in different situations. Where access to proto-industry was restricted, so families limited their size and controlled household numbers. Of course it might also be pointed out that where access to rural industry was not restricted by social and corporate institutions (as in England) rural industrial family and household size was frequently greater than among non-industrial neighbours.

This is a large work, and at some points interest flags as the detailed empirical and statistical analysis proceeds step by heavy step. It is a pity that so much of what reads like the original dissertation has been retained. There are more words and more regressions here than most readers need or desire. The book's length (and price) may discourage many readers (and prospective purchasers). For most economic and social historians the crucial and most valuable part is the two chapters which introduce the proto-industrialization debate and discuss the links between proto-industry and social institutions throughout Western Europe. In the

second the author's breadth of coverage and sophistication of analysis is better by far than anything that has yet appeared in the 'proto-industrialization' debate. My only niggle is that Ogilvie was not able to inform us more about the early decades of the rural worsted industry in the Black Forest, before the rigid structure of weaver and merchant guilds enforced stasis on that proto-industrialization process which (in a few other places) led to broader social and demographic change.

MICHAEL ZELL
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Catherine Clinton and Michele Gillespie (eds.), *The devil's lane: sex and race in the early South*. (New York and Oxford: Oxford University Press, 1997.) Pages xx + 274. £27.50.

'The devil's lane', the editors inform us in their introduction, is a southern colloquialism for disputed territory. They have appropriated it here as the title for a collection of essays each of which examines a particular point of dispute – race, ethnicity, status, sexuality, gender – in early southern American society, specifically in the period prior to the closure of the external slave trade in 1808. In historiographical terms, much of the territory being explored in this volume has been sidelined by an academic profession that has, to date, swerved away from such 'taboo topics' as the exploration of sexuality, and the editors note that research in these areas is 'long overdue'. The volume is sub-divided into four parts, beginning with an opening section entitled 'Broad strokes' comprising essays by Peter Wood, Carol Berkin and Catherine Clinton. As its title suggests, this opening section takes a broad overview of early southern history and has been deliberately constructed as a challenge to 'the image of the colonial South as a fixed backdrop to events that led to the emancipation of enslaved African Americans in the nineteenth century'. Peter Wood's impressive opening essay examines the violence of the early period, and suggests that the realistic complexities of life in the new nation on the eve of independence remain unpalatable to many Americans for whom the Revolutionary Era 'remains the most closely guarded treasure in our national mythology'. This point has been made elsewhere – notably by Joyce Appleby – but it is nevertheless a powerful one. The need to face the past squarely has never been more pressing. Carol Berkin's overview of the historiography of southern women argues that it is time to move beyond comparisons between women in the Chesapeake and New England toward the kind of history written by Elizabeth Fox-Genovese in *Within the plantation household*, namely one that resists 'a causal hierarchy that makes gender the necessary and sufficient variable in explaining women's lives'. Catherine Clinton ends this opening section by examining the role played by Parson Weems in establishing sin and redemption as 'the central contending forces within America'.

The sections that follow develop some of the points raised by the three opening essays but are in the main designed to suggest new avenues of departure and

debate. Section II focuses on the Upper South and is devoted to legal history. It comprises a varied collection of essays on such diverse subjects such as gender identity, rape cases in early southern courts, the developments of women's networks in the South, early attempts to regulate slavery and deter miscegenation and the attempts by enslaved female church members to establish rights for themselves within the disciplinary codes of the Baptist and Methodist churches. Kathleen Brown has uncovered the fascinating case of Thomas Hall, whose gender identity fluctuated according to his/her circumstances. Although this was an unusual and possibly unique example, Brown argues that the Hall case was nevertheless 'emblematic of the destabilizing effects of military conflicts, imperial rivalries, and colonial projects of early modern Europeans', Peter Wallenstein reminds us of the triracial nature of early colonial society, focusing in particular on how Indian ancestry was used by slaves in order to sue for freedom in the courts. From a different perspective Diane Miller Sommerville also examines early legal procedure in a stimulating and well-researched piece which highlights 'the fallibility and inadequacy of relying solely on statutory law to draw conclusions about the extent to which the white male population was animated by deep-seated fears of black male sexuality' in the early period. Flexibility, rather than a dogmatically hostile response to black defendants in such cases, was more the norm, until emancipation changed the rules forever. In a different vein again, Betty Wood shows how enslaved African-American women used the disciplinary procedures of the Baptist and Methodist churches both to validate the high standards of sexual morality which they set for themselves and to insist that these standards be recognized by male coreligionists.

Section III covers the Lower South, and is more eclectic than the previous section, ranging from a piece on the North Carolinian Moravians to a study of the changing fortunes of Mary Musgrove, the Anglo-Creek arbiter and interpreter. It opens with a piece by Kirstin Fischer, again on the use of the law, this time in North Carolina. In a society in which a good reputation counted – as in *Othello* – for a great deal, individuals found themselves having to turn to the law if a neighbour had defamed them in such a way as to cause them 'specific harm'. White men sued most frequently to protect their reputation for honest business dealing; white women to refute sexual scandal. Most importantly, Fischer's research reveals how allegations of interracial sex 'aided in the construction of racialist thought among European Americans'. The abuse that white North Carolinians hurled at each other, she concludes, 'reflected their developing ideas about race' at a time when slavery and racism were becoming entrenched in southern society. In a completely different vein, Cynthia Lynn Lyerly uncovers the inner world of enslaved southern Methodist women, for whom the religious experience was far more profound than historians have perhaps realized. Section IV examines the Gulf South, and comprises an essay on rape and infanticide in St Augustine, by Jane Landers; two pieces on free and enslaved black women in colonial New Orleans, Mobile and Pensacola, by Kimberley S. Hanger and Virginia Meacham Gould respectively; and a concluding essay by Gwendolyn Midlo Hall on African women in Louisiana when it was still under French and Spanish jurisdiction.

It is perhaps inevitable that such a wide range of essays will vary in terms of quality and argument, and this collection is no exception. The editors have undertaken an important and ambitious project in seeking to publicize such new and significant departures in American history. Some of the pieces, however, are rather less polished than they might have been and show obvious traces of having originated as papers designed to be read out rather than read. In places, too, the evidence shows signs of strain, being pushed rather hard in order to reach the point being made. As a consequence, the overall effect of the collection can be overwhelming and may well be confusing for readers new to this area, which is especially unfortunate since this is surely the audience that the editors most hope to reach. Such quibbles aside, however, the bulk of this collection is fascinating and valuable and adds considerably to our understanding of the early South and to the social and cultural complexities of life at that time.

SUSAN-MARY GRANT
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Peter Scholliers, *Wages, manufacturers and workers in the nineteenth-century factory*. (Oxford and Washington: Berg, 1996.) Pages xiv + 256. £34.95.

This book provides a detailed case study of the Voortman cotton mill in Ghent by a scholar already well known for his extensive work on wage rates. From the archives of the Voortman factory, Scholliers extracts copious details on wage rates, productivity levels and the efficiency of this particular firm who were major employers in Ghent and at the centre of Belgium's prosperity from 'white gold'.

It is a study in the strategy of cheap production. This was not an industry producing high-quality or specialized goods – Belgian cotton remained coarse throughout the century and, as a result, the Voortmans' profits resulted from low initial wages reinforced by cost-cutting. The workforce tended to be drawn from the 'super-exploitable' and consisted mainly of young female machine operators and male handloom weavers. Not surprisingly, with wages reduced to their lowest level, labour turnover was high. As a result of the textile industry, Ghent was an almost completely proletarian city, with a mass of wage-dependent workers and a small coterie of rich employers. Yet resistance to the outright antagonism of the owners seems to have been limited. Class conflict was overt in the 1830s when machines were broken and there were bitter fights between employers and employees. But, on the face of it, labour protest was almost completely defeated or, at other times, simply ignored. The Voortmans were able to draw in new workers and, as many employees were related, some of them seem to have remained loyal to the firm despite the fact that the Voortmans made no moves towards paternalism at any level. As their workers suffered and starved, the Voortmans became landed proprietors. By the second half of the nineteenth century, the mill was becoming distinctly backward in terms of technology input but continued to make profits by the continued use of the surfeit of cheap labour. There was limited amelioration in the condition of the workers due to falling retail

prices and Scholliers is able to analyse the budgets of some of the workers and to gauge their standards of living.

As a meticulous study of the efficiency and production methods of one firm, and because Scholliers puts the Voortman mill into the context of operations in other European factories, this book will be of great value to business historians. Social historians would have benefited from a more lengthy examination of the failure of labour protest and, particularly, the gender aspects of worker oppression. In the mid-nineteenth century, 50 per cent of the workforce was female, presumably because women were seen as more submissive and less militant than men. Regrettably, this book does not provide an insight into the raw human experience of exploitation. The records do not permit Scholliers (and it is perhaps not his concern) to examine the small and subtle acts of resistance to which the miserable workforce no doubt resorted.

In a book published in English, translation of quotations from original documents (at least in the footnotes) would have been helpful. Nevertheless, for those interested in wages and the standard of living within the context of the family economy, this in-depth contribution to current research will be most welcome.

PAMELA SHARPE
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Barry Reay, *Microhistories: demography, society and culture in rural England, 1800–1930*. (Cambridge: Cambridge University Press, 1996.) Pages xxv + 288. £40.00.

Barry Reay's *Microhistories* is a spin-off from his earlier research into the rising of Kentish agricultural labourers in 1838. Focusing on the three parishes which make up the area known as the Blean, he attempts to reconstruct a three-dimensional image of local society from a catholic array of contemporary sources supplemented by the oral testimony of those who grew up in the area during the early twentieth century. The product is advertised as likely to interest 'family and oral historians … historical anthropologists, demographers, geographers and sociologists', a list which accurately reflects the medley of influences to be found within a book which contains chapters on fertility, health, social economy, class, families, sexuality and literacy.

Perhaps inevitably, the overall impression given by the book is rather unfocused. It reads much more like a collection of essays than a series of chapters working to substantiate a larger thesis, and, indeed, several of the chapters have been published elsewhere. Whilst this, coupled with the fact that Reay is not inclined to offer any broad conclusions about the Blean, makes it rather difficult to discuss the book as a whole, it must be said at once that several of the essays are intensely interesting and suggestive. That on social economy, which exploits oral history interviews to produce a fascinating picture of the formal and informal inputs into the rural labourer's domestic economy, is a particularly good example.

Another strong feature of the book is the manner in which Reay uses traditional sources in new and creative ways, as in the section on the health of children where

the School Log Books reveal the 'local pattern of…continuous background morbidity, punctuated by several epidemics every decade' (p. 85). Similarly, the illegitimacy files at the Centre for Kentish Studies are used to shed light upon the assumptions which underpinned courtship in the area, and the informal controls exercized over couples by the wider community, especially in cases of unwanted pregnancy. The combination of statistical analysis with oral history to call into question the historiographical orthodoxy that Britain was dominated by nucleated families provides a third example.

In all these cases, the fact of the study's being focused so closely on one very small area works to good effect, providing a depth of detail and local colour which make for vivid reading. At times, however, there seems to be an unresolved tension between the local angle and the more sweeping sub-title of the book, 'Demography, society and culture in rural England, 1800–1930'. Reay's claim for the presence of class feeling in the countryside, even if accepted on the evidence he brings forward here, cannot in fact be unproblematically applied to the whole of the country. The issue of the good effect or otherwise on relationships between farmers and labourers of the latter's living-in at the farms on which they were employed provides a case in point.

Reay argues, from the evidence of cases at petty sessions brought for breach of contract by employers against labourers, that the system was not 'consensual' but 'coercive', and that 'living-in' did not lead to a close relationship between master and man. Not only will this source of evidence naturally provide an unfavourable view of such relations, but Reay also fails to address evidence from other parts of the country where 'living-in' was much more common than in the capitalistically oriented countryside of the south-east. Where communities were more scattered, farms were smaller and the social gulf between farmers and labourers not as significant, relationships between masters and men do seem to have been much better. The immunity of large areas of the country from agricultural trades unionism in the 1870s, for example, reflects the generally better climate of work-place relations.

The strength of this book (and of the microhistory technique) lies in revealing patterns of life in a very localized area. Where Reay steps outside this context, the tone becomes less convincing. Nevertheless, in its good moments this is a genuinely fascinating study of rural labourers and their place in the local community, and will become required reading for all students of the countryside.

MATTHEW CRAGOE
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Faith Robertson Elliot, *Gender, family and society*. (Basingstoke: Macmillan, 1996). Pages xii + 268. £45.00; £14.50 paperback.

Historians who attempt to locate the origins of the modern nuclear family and to identify fundamental changes in household structures and attitudes regularly find themselves frustrated, unearthing from the past as many dogged continuities as radical shifts. What a contrast with this century. Faith Elliot's survey of gender,

family and social relations in contemporary Western societies reviews the extraordinary upheavals in family arrangements and attitudes that have characterized the second half of the twentieth century. The 1960s and early 1970s witnessed a growing dissatisfaction with the dominance of the modern conjugal family; the emergence of alternative patterns of sexual relations; an increasing acceptance of sex outside marriage, of couples living together before marriage and of same-sex relationships; a rise in the number of couples divorcing; the emergence of dual-worker families and so on. Elliot describes these changes logically and succinctly, presenting the competing arguments sociologists have put forward to explain them, before moving on to investigate more recent shifts that form the core of her book.

The changes of the earlier period took place at a time of relative economic prosperity, expanding welfare systems and a new egalitarianism and optimism about social relations. Since then economic restructuring has produced instability in labour markets; optimism has given way to pessimism or to feelings of ambivalence; and attitudes and behaviour have shifted even further in complex and sometimes unexpected directions. Observers argue over whether the transformations of the last third of the century amount to a revolutionary break with past patterns of social organization, or merely a refiguring of traditional patterns into slightly different forms. Others favour a middle view, a model of family change which allows for the coexistence of 'traditional' and 'liberated' values and patterns of behaviour and which recognizes the ambiguity, contradictions and paradoxes which this coexistence so often produces.

Against this backdrop of rapid change and increasing ambivalence, Elliot describes five key issues affecting the ordering of personal, sexual, gender and parental relationships in the 1990s. Firstly, there is the influence of ethnic differentiation and gender on family life, and the diversity of family patterns among different racial groupings. Secondly, there is the effect of recent economic change on family life and in particular on male self-esteem. The restructuring of labour markets and the rise of part-time work and casual labour, growth in unemployment levels and a reduced commitment to welfare have altered the prospects for many young men, but does this, as some sociologists contend, amount to a 'crisis of masculinity'? Thirdly, there is the 'ageing' of Western populations, looking at the construction of 'old age' as a social problem and at the nature of care for the elderly. Fourthly, there is the growing awareness of the extent of violence in family life, especially male violence against women and the sexual abuse of children. Fifthly, there is the impact of AIDS, which Elliot suggests 'epitomises the ambivalence, anxieties and problems of post-permissive Western societies'. Elliot rounds off her text with an epilogue in which she underlines the impact of each of these phenomena on family life, demonstrates the complex interconnections that link them and points out the difficulties involved in defining family types and dominant models in a world where the breadth of individual experience defies simplification or averaging. Indeed, one of the main strengths of this synthesis is the emphasis Elliot places on complexity and contradiction in all matters relating to 'the family' in both the past and the present.

Elliot's well-organized, unbiased text provides an excellent introduction to the subject of gender, family and society for undergraduates in sociology and a range of other disciplines. The strength of its feminist scholarship in particular should appeal to anyone interested in women's studies. However, with 38 tables and figures presenting data from Britain, various Western European nations and the USA, it also constitutes a valuable source book for general or specialist readers with an interest in recent changes in family life. The book is well-written and accessible (although references to 'eufunctionality', 'ideational change' and the 'ineluctable processes of ageing' may distress some readers), and should interest a wide audience.

TIM STRETTON
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Gerard J. DeGroot, *Blighty: British society in the era of the Great War*. (London and New York: Longman, 1996.) Pages xiv + 356. £14.99.

Initially intended as an 'up-to-date and affordable text on British society during the Great War', DeGroot's study not only provides an extremely useful synthesis of recent scholarship on the impact of the First World War upon Britain, but fully engages in the ongoing debate concerning the nature of this impact. In the change versus continuity argument, DeGroot comes down firmly in the continuity camp, seeing the war not as 'a deluge which swept all before it, but at best a winter storm which swelled the rivers of change' (p. 291). He makes a compelling case for the influence of deep-seated forces of conservatism on both the management of a nation at war and the recovery from its impact. The government, and society as a whole, endeavoured (not always wisely, and with very dubious success) to minimize the effects on civilian life of the conduct of a new kind of warfare ('Business as Usual'). Indeed, the war itself was seen as returning the nation to simple sound values, a conventional military campaign against an enemy 'out there' replacing the ambiguous and problematic prewar struggles of class, gender and the colonized. Afterwards there was 'a desire to reconstruct according to cherished patterns' (p. 311).

He is justly sceptical of received myths of the war. He takes a cynical view about the alleged loss to the nation of the 'Lost Generation' of the ruling elite. He points out that although certainly some individuals of great promise were lost, the governing elite as a whole was based on social rank rather than merit, and that the nation still had vast untapped resources of talent, both among men who did not belong to this elite and among women. He also debunks the idea that Britain only just missed a left-wing revolution as a result of the war, emphasizing the profound conservatism, sullen deference and patriotism which pervaded the working classes. He even inverts the traditional 'lions led by donkeys' cliché, suggesting that the working-class soldier was 'a beast of burden, a man caged by a life of drudgery, squalor, powerlessness and social stasis' (p. 173). The paranoia and hysteria aroused by fears of alien spies and saboteurs and by 'conchies', DeGroot suggests, indicates how solid and pervasive support for the war was.

There is an excellent chapter on 'Houses, homes, and health'. Unlike many historians who have emphasized the entry of women into new spheres during the war, DeGroot points out that for most women their task was still to keep 'home fires burning' in the face of food shortages, rent increases, delinquency among the young and all the other problems brought by war. The married woman was often in a no-win situation, urged to work for the war effort yet blamed for neglecting home and children, with few allowances being made for the double burden, and suffering the incalculable emotional strain of anxiety for loved ones in the forces (though some women, as Leonora Eyles reported a few years later in *The woman in the little house*, were 'bloomin' glad the old Kaiser went potty'). Health improved, partly, he suggests, because the absence of husbands at the front meant more food to go round the rest of the family. On questions of nutrition and rationing it would have been nice to have had some discussion of the role of nutritional scientists in forming policy and determining the composition of the approved loaf (DeGroot claims that this was 'as digestible as a howitzer shell and probably as lethal' (p. 89), something that can surely only be ascertained by making up the recommended recipe).

He follows an orthodoxy already being queried about the place of women and feminism in the postwar era. Whether feminism died with the grant of the limited suffrage or whether it mutated and dispersed among diverse campaigns is a hotly debated issue, as is the received assumption that surviving feminists can be neatly classifiable into Old (equal rights) feminists, and New (special needs of women) feminists – a view which overestimates prewar and underestimates postwar feminist cohesiveness. And while there was certainly a postwar rush into matrimony, there are strong arguments that the dynamics of marriage in the 1920s were significantly different from those of the 1900s.

In spite of these quibbles, this well-written and very readable book can be highly recommended. It also provides a substantial bibliography incorporating contemporary or retrospective accounts by participants and observers alongside recent scholarship.

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P. Skold, *The two faces of smallpox: a disease and its prevention in eighteenth- and nineteenth-century Sweden*. Report no. 12 from the Demographic Data Base, Umea University (Umea, 1996.) Pages 654. £40.00.

The subject of Peter Skold's study is germane to historical demography and epidemiology since both the disease and the country in question are of central interest to those disciplines. Smallpox's position in disease history is in many ways unique, but its character is paradoxical, and it is this quality which underlies Skold's title. Perhaps the most feared and destructive of infections, after plague, smallpox could leave its survivors literally marked for life or – given its role in precipitating mortality from other conditions – for early death. Yet it was the one major killing infection which pre-scientific, indeed folk, medicine, could prevent,

and smallpox had already been transformed from a ubiquitous disease of childhood to a rare adult infection before the advent of germ theory. The extinction of the disease – probably the twentieth century's sole permanent and complete triumph over infection – was, moreover, accomplished with the eighteenth-century technique of vaccination: scientific medicine was never able to develop an effective drug treatment. Smallpox is, historically, at once the most visible and the most mysterious of entities – most visible in that its symptoms were clearly recognizable to contemporaries – and it is one of the few terms from the 'pre-scientific' symptomatic gnosology which can be identified straightforwardly with a disease known to twentieth-century medicine; most mysterious because its prevalence and age-incidence varied over time and space in ways which are hard to understand.

Sweden is of particular importance in this context (as it is in the study of population history generally) since it possesses a uniquely detailed and extensive series of population registers from the middle of the eighteenth century. It is this material that forms the core of Skold's study. After extensive introductory and methodological passages the volume falls into three main sections. The first is concerned with quantitative demographic and epidemiological data on a national and regional scale, with some local studies, and the remaining two cover the progress and effects of inoculation and vaccination respectively. Smallpox was evidently – in effect – a universal disease of childhood in eighteenth-century Sweden. In England, by contrast, the occurrence of many adult casualties demonstrates that infection was widely avoided in earlier life, despite this country's denser population and settlement network. Furthermore, the rarity of adult smallpox deaths when Swedish registers begin shows that the disease had evidently become ubiquitous decades before, yet this process attracted relatively little comment from contemporaries.

Inoculation was practised widely in eighteenth-century England – with effects which remain controversial to this day – but in Sweden its story is largely one of a dog which didn't bark. It was little practised outside the elite, and divisions within the medical profession were partly responsible for this. But things fell out very differently where vaccination was concerned. Under the aegis of the national Medical Board a network of 2–3,000 vaccinators was established – many of them lower-level ecclesiastical personnel – and, backed by legal compulsion from 1816, a high level of coverage was evidently attained. Unlike inoculation, vaccination gave only temporary protection, but the need for re-vaccination was not immediately recognized – indeed the Medical Board at first suppressed evidence of the need for re-vaccination – and, outside the military, an effective programme seems to have awaited the closing decade of the nineteenth century. In spite of this, however, vaccination in Sweden seems to have been very effective in reducing mortality from the disease.

The two faces of smallpox runs to more than 600 pages, and the Swedish idiom may make it heavy going for Anglo-American readers, but the material it contains will make it essential for anyone seriously interested in the historical demography and epidemiology of smallpox. In some other respects, however, it is a little disappointing, particularly for the non-Swedish reader, since the historical context

of contemporary Swedish society is sketched only very lightly. It is also very much 'history from above'. The reader learns little, for instance, of the nature and function of the 'church assistants' – unsung foot-soldiers in the war against smallpox. Nor, apart from a few references to 'popular fatalism', is there much discussion of the meanings which smallpox and its preventive measures had for the mass of the Swedish population in these centuries.

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Richard J. Evans, *Rituals of retribution: capital punishment in Germany 1600–1987*. (Oxford: Oxford University Press, 1996.) Pages xxxii + 1014. £55.00.

No one could accuse Richard Evans of shrinking before the morbid side of Germany's past. Following his earlier book on cholera in nineteenth-century Hamburg, here is another mammoth, pathbreaking study – over 900 pages of text – with death as its focus. This is a marvellously written exploration of the changing position of capital punishment in German society and politics, of official and popular attitudes towards execution, of the repeated efforts to reform criminal law and achieve abolition, of the procedures following judicial condemnation of the accused, and of the mechanics of punishment, including a social history of the executioners and their craft. In spite of its length (and weight), the book is difficult to put down. Evans keeps his story moving forward at a brisk tempo, combining subtle analysis with an eye for pointed, sometimes horrifying, detail. The subject is treated solemnly, but it shocks despite the effort to avoid a sensationalist approach to the material.

Evans begins with the ritualized executions of the seventeenth and eighteenth centuries, where the public executioner, customarily recruited from the ranks of dishonourable and polluting trades such as town knackers, stood on the margins of society, patrolling the border between culture and nature, and 'acting as the conduit through which the excrement of the social body flowed into the world beyond' (p. 64). Evans argues that early modern executions were not simply manifestations of the absolutist power of the state, but were negotiated events with community sanction, expressions ultimately of a divine not a secular will. Around 1800, this public sanction of executions itself became an object of elite concern. Several factors help explain the shift towards imprisonment as the major medium of punishment: a new fear of the crowd during the revolutionary era; the search for an effective deterrent against crime; the rise of reform ideology; the declining importance of honour so that penalties of dishonour lost their meaning; the redefinition of the female as weak and emotional, so that infanticide, one of the major reasons for putting women to the axe and wheel, was no longer demonized; and increasing bourgeois sensibilities about blood, gore and the public display of corpses.

Two of Evans's arguments emerge early. Firstly, Elias's idea that executions played a role in 'conscience forming' in the 'civilising process' is dismissed as unsupported by the evidence. Law and order, not the avoidance of cruelty, was the

reformers' principle objective. Secondly, Foucault's view of the new penal reforms as a 'totalizing system' is rejected in favour of regarding reform as a product of 'pressure from below'. Evans is not always clear about this point, and his statements about public and private spheres and about the respective significance of authority and popular pressure in determining the course of capital punishment sometimes tend to contradiction (compare, for instance, pages 147 and 149). What is clear is that he believes that the secularization and de-ritualizing of executions during the nineteenth century was a gradual and contested process in which the new disciplinary discourse was never more than partially realized.

One of the strongest themes to emerge is the demonstrably close relationship between capital punishment and the contemporary political context. Evans collects an impressive array of statistics for several German states which show an inverse relationship between the number of executions and the rise of liberalism from the 1860s, and again during the 1920s. The violent birth of the Weimar Republic, however, left a legacy which compromised all political parties in their approach to capital punishment. Efforts to obtain abolition were further hindered by the politicization of criminal biology, by the astonishing sequence of 'sex murders' during the period and by the continued public interest in executions fuelled by the popular press. During the Nazi era the boundary between judicial execution and mass extermination became totally obscured. However, Evans finds irony in the fact that after the war the Occupying Powers executed criminals at a similar rate to that of the previous regime, reappointed Nazi executioners and even made similar racial judgements about the Polish 'displaced persons' who appeared before their courts for murder and robbery. The abolition of capital punishment in the constitution of the Federal Republic came about less by design than by the accident of political party configurations, while in the GDR show trials and executions in the 1950s, albeit on a limited scale, served to restore the customary links in Germany history between political ideology and capital punishment.

Evans succeeds in bringing fresh insights to an area of modern history which has already attracted considerable attention. Unlike other works in this field, the victims are not the main focus of his attention, nor does he attempt to examine in a German context Linebaugh's view of capital punishment as a means of protecting property. Evans's perspective might be described as liberal. It is certainly abolitionist, and in his concluding pages he drafts a powerful argument against justification of the death penalty on grounds of either deterrence or retribution. The lesson of history is that the state which kills its citizens is degraded by the act of killing. Politicians, as well as historians, should read this compelling book and take note.

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Raimo Pullat (ed.), *Die Nachlaßverzeichnisse der deutschen Kaufleute in Tallinn 1702–1750*. (Tallinn: Estopol, 1997.) Pages 560. DM 100.

The city of Tallinn (Reval) in modern Estonia was a major seaport and a military base of considerable strategic importance in the Baltic during the eighteenth century. Like other Baltic trading towns, its population reflected the political and economic currents of a region where social and national distinctions were often mutually reinforcing. Though a cosmopolitan city linked to European and world markets, Tallinn's population was internally stratified as each migrant group remained associated with particular occupations and levels of education. This fine edition, by the eminent Baltic historian Raimo Pullat, of German traders' probate inventories provides an illuminating insight into a slice of this rich and diverse culture.

The collection reproduces the full original German text of 47 inventories for the period 1702 to 1750, drawn from the 500 which survive the eighteenth century. Those selected thus date from Tallinn's period of great upheaval. The city suffered the effects of the 1697 famine, the 1710–1711 plague and the Great Northern War (1700–1721) which saw its transfer from Swedish to Russian control. All the inventories are those of the lesser German traders who, though belonging to the city's social elite, were nonetheless outside the magistracy dominated by large-scale German merchants. The careful transcription sticks faithfully to the original and is well supported by a useful bibliography and a short introduction. The latter provides a brief analytical overview of all 500 surviving inventories, arranged by social and economic status and giving a glimpse into what is obviously a rich archival source.

Pullat has already published extensively on Estonian urban and demographic history and intends this to be the first in a series of edited volumes to encompass all the surviving inventories. Given the differing nature of wills and inventories as historical sources, it is to be hoped that the former will also be included in future volumes. This book nonetheless represents a significant source for social and economic history and the history of everyday life, and is of particular value to those interested in the cultural and economic connections across the eastern Baltic.

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Dauvit Broun, Richard R. Finlay and Michael Lynch (eds.), *Image and identity: the making and re-making of Scotland through the ages*. (Edinburgh: John Donald, 1998.) Pages iv + 236. £16.00 paperback.

This generally worthwhile volume comprises eleven substantive chapters, mostly written by younger scholars of Scottish history. The volume came out of a conference which was organized by the Scottish History Department of Edinburgh University in January 1995. The central theme is that national identity is a Protean phenomenon, subject to invention and re-invention, and that 'Scotland' or being Scottish has meant different things at different periods from the twelfth to the

nineteenth century (when the discussion stops). The authors variously consider how and when identity was created, what (if anything) constituted it, what apparent dualities or contradictions tempered its impact, and what its overall effect was.

Most of the contributions focus on geographical dimensions of belonging and being different: the local, regional, or national. It is only in the last three chapters that different perceptions based on social status and gender are considered systematically. As is always the case with edited volumes, the quality of the contributions varies. Some of the chapters have the distinguishing features of the conference papers they once were. Some seek (with varying degrees of success) to provide an overview whereas others offer a specific angle on an aspect of identity. For example, Finlay unfortunately fails to achieve the bold synthesis to which his chapter aspires. Cameron, Morton, and Young provide the most interesting analyses, dealing successfully with both facts and concepts. It is no accident that these are also the chapters which have the largest comparative components. In the case of Morton (stream of consciousness) and MacDonald (sociological and geographical theory) the writing style obscures the message. The medieval chapters are, as one might expect, source-led but are sensible as far as they go.

While the chapters generally cohere around the theme of the nature of identity or identities and how they vary over time and space, the impact of the volume could have been enhanced by a fuller introduction. Some theoretical framework would have been helpful here – for example, that provided in Benedict Anderson's *Imagined communities* – even if, ultimately, it is the shared view of most of the contributors that those who try to weave the complex strands of history into twentieth-century simplicities are doomed to fail. The introduction we have is a mere three pages long and simply sketches the main points of the contributions without adding further interpretation. Editing and production standards are sometimes unacceptably poor: at least three different fonts have been used in the book, while the footnotes and index bear the marks of an only partly successful attempt to handle submissions compiled with different software. Finally, there are many typographical errors and other evidence of careless proof-reading. Regarding the Ossian myth, MacPherson apparently 'attempted to mold the residues of Gaelic culture into the heroic epic which might appeal to a gentile audience' (p. 148).

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