The early modern history of Lancashire and Cheshire offers a range of exciting possibilities for the enterprising historian. Indeed, even though regional history has been accepted (and even respected) as a branch of the historical sciences for the past generation, it is remarkable how little research has been undertaken in most counties and how much more remains to be done. In the case of Lancashire, one almost senses that the history of the county pre-1780 has been written off as uninteresting. Twenty years ago there was a great deal of work being done on early modern Lancashire, out of which two major monographs emerged (Haigh on the Reformation and Blackwood on the gentry). In the early 1990s students seemed to be drawn to Cheshire, with theses by Hindle and Thornton now making their way into print. This makes the point that the history of a county is written by individuals, and small numbers of individuals too. Hence there is room for more workers on the history of both counties.

1 I ought to explain that I have made no attempt to include every recent writer in this discussion and so offer my apologies to those omitted. I have deliberately not included theses except in a few cases where I know the process of turning the thesis into a book is well advanced. And I have concentrated on Lancashire as the county I know best. The paper was delivered in October 1996: I have referred to publications crossing my desk up to Easter 1997. For a full annual survey of the literature, I recommend Northern History. In writing this paper I was grateful for the comments of Andy Gritt and Dr Suzanne Schwarz.
It necessarily follows that the history which needs to be written in the next generation is conditioned by the achievements of past generations. And in this respect the coverage of the two counties is curiously uneven. Both counties have the great advantage of the recent Phillips and Smith contribution to the Longman Regional History of England series.² This summarizes what is known; the acute historian can discern from a reading of text and citations where the gaps in our knowledge lie. Cheshire has the disadvantage of no completed Victoria County History, or rather has the advantage of having one under way where Lancashire suffers the disadvantage of a first-generation V.C.H. account of the county. The topographical volumes of V.C.H. Cheshire have yet to appear, but they will surely reflect the preoccupations and sensitivities of historians at the end of the twentieth century rather than those of the late nineteenth. One would like to see V.C.H. Lancashire redone to reflect the same contemporary preoccupations, but the financial means will almost certainly never be found. And it could well be argued that if money or willing hands were available, there are higher-profile ways of spending it than on the detailed parochial histories which remain at the core of the V.C.H.

There are other ways in which the achievement of the past impinges on the future. One area in which a great deal of labour remains to be invested is simply that of record publication. On medieval records, it seems to me that the North-West has an enviable record of publication; on early modern records less so. There is a mass of standard historical sources (the Poll Tax, the Lay Subsidies and Hearth Taxes, or the Feet of Fines) which remains to be published. Relatively little of the two counties’ quarter sessions records has been put into print and virtually nothing from the early modern palatinate records in the Public Record Office. Nor has a great deal been published out of the Duchy of Lancaster archive, and one would welcome some modern editions of court rolls or estate documents—say a volume of records relating to the pre-1640 estates of the earls of Derby. Amongst

the northern towns, one hears of reports of forthcoming editions of Liverpool records, but Chester surely calls for a determined campaign of publication so we can see the records as a whole and not simply in Morris's thematic re-arrangement. Amongst the smaller towns, the earlier (mid- and late seventeenth-century) civic records of Lancaster and Preston deserve editions. The two major antiquarian collections, the Randle Holme family for Cheshire and Christopher Towneley for Lancashire, require published descriptions and selective publication. Of course, the publication of record texts by university historians is presently rather unfashionable and one hears that record societies do not always like to accept proposals for the publication of 'lists', but this is a key investment for the future.

Another area which remains substantially unexplored by early modernists is the records of the palatinates which, since the mid-nineteenth century, have been lodged in the P.R.O. The character of this material is familiar to the medievalist but perhaps because the records are in form medieval, and inaccessible to boot (in both language and location), early modernists have been slower to draw from this particular well. There would appear to be a mass of material here for the historian interested in crime and the application of the criminal law; there are also considerable numbers of equity proceedings which continue into the nineteenth century. The palatinate equity pleadings for Lancashire covering 1650–1700 were described in detail as a freelance exercise by the late Angela Barlow, whose list can be seen at Kew: there is no longer any excuse for not using them. A similar archive, the equity pleadings of the Duchy of Lancaster, was listed to 1603 by the Record Commissioners in the early nineteenth century, but again the material runs on through the seventeenth and eighteenth centuries and its proper description will only be achieved by someone from outside the Public Record Office taking the initiative. There are treasures to be discovered in

3 This list, which is all that has yet appeared of a larger project curtailed by death, covers PL 6/19–46 (bills) with cross-references to PL 7 (answers) and PL 10 (depositions). The list has a comprehensive index of places and persons.
that archive too. There is work to be done on the vitality of the Duchy and Palatinate administrations, the identity of their office-holders and the way in which the jurisdictions were used and perceived by the inhabitants of Lancashire and Cheshire. We should not judge their importance by the fact that they were largely moribund by the early nineteenth century.  

A final matter which colours the sort of history which has been written is the lack of state-funded archaeology in the two counties. There is now an account of the state of archaeology in Lancashire—*The Archaeology of Lancashire: Present State and Future Priorities*—which offers the interesting figure that between 1938 and 1972 there were only four state-sponsored rescue excavations in the modern counties of Lancashire, Greater Manchester, and Merseyside compared to forty-three in North Yorkshire: ‘From the viewpoint of central government funding of site investigations before 1973, these three counties were the least investigated in England’. A search of the annual reviews of excavations and other work in *Medieval Archaeology* and *Post-Medieval Archaeology* reveals a continuing dearth of archaeological work in the county, and this is confirmed by the relevant chapters of *The Archaeology of Lancashire*. A great deal of historical research has been undertaken elsewhere by archaeologists (or historians working with archaeologists) to contextualize the findings of excavations and to offer guidance as to where limited archaeological resources should be invested or which areas should be given statutory protection. One specific piece of archaeologically inspired (and funded) history which might be mentioned in this context is Dr Henry Summerson’s monumental *Medieval Carlisle*, on which Lancastrians should look with ill-concealed jealousy. The first point I want to make is that the lack of archaeological

investigation in Lancashire has not stimulated corresponding historical research in the ways it has in other counties, perhaps most notably West Yorkshire. Secondly, archaeology has served to enrich historical understanding through its appreciation of the landscape and the need to look at developments in both rural and urban society in spatial terms. Landscape archaeology or landscape history is relatively underdeveloped in Lancashire at least (although the work of Mary Atkin, Mary Higham, and Angus Winchester deserves commendation). Much more can surely be done on the reconstruction of the physical fabrics of towns: whilst very little survives, it may be possible to draw on maps, nineteenth-century drawings, and the early photographic record to offer comments on at least the external appearance of buildings in towns like Lancaster, Preston, and Wigan and, by using inventories, fill these buildings with people and their possessions. Likewise the study of vernacular architecture ought to contain much mileage: Sarah Pearson’s terrific *Rural Houses of the Lancashire Pennines, 1560–1760* (1985) shows what could be done, whilst Lancashire and Cheshire historians could also model their work on P. Stamper, *Historic Parks and Gardens of Shropshire* (1996).

In the remainder of this paper I propose to discuss six areas of research, quickly review the modern literature on each, and then make some suggestions as to what might profitably be done. Some of the suggestions are doubtless already being acted upon; others may be contained in the historian’s equivalent of the deep-frozen body awaiting resuscitation, the unpublished doctoral thesis. In both these instances I offer my apologies.

(i) Agriculture and Rural Society. The first place to which the student of rural society would turn for an account of the agriculture of any English county would turn for an account of the agriculture of any English county would be the discussions of


8 For a telling example of how this can be done, see P. Holdsworth, ‘Layout of the town’, in Michael Morris (ed.), *The Archaeology of Greater Manchester, I: Medieval Manchester* (1983).
regional farming practices in the *Agrarian History of England and Wales*. The accounts in both volume IV (by Joan Thirsk) and V (i) (by David Hey) are relatively poor. The best single account any aspect of farming in the North-West is Foster’s recent account of Cheshire cheese production. A paper by King on the fairs of Blackburn hundred is actually much more about agriculture than its title might lead the reader to suppose. On rural society, one suspects a preference for work in the Tupling tradition of research on the Pennines: Ironfield on Chipping, and Swain on the eastern end of Blackburn hundred.9

There has been a little recent work on landowners and their estates: the earls of Derby by Coward, late sixteenth-century gentry in south-west Lancashire by Hollinshead, and mid-seventeenth century Lancashire gentry by Blackwood.10 Foster has also published an account of estates in Cheshire in the 1740s, but there is little more in print for north-western estates after 1660.11 There is a decidedly odd article on the inheritance practices of the Cornwall-Legh family of High Legh, Cheshire.12

Rather like record publications, estate studies might seem to be old-fashioned, and yet we need many more of them. The

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Duchy of Lancaster estate holds enormous possibilities for the researcher with access to London. We need more accounts of lowland estates before 1660 and highland estates after 1660. Enclosure also remains a central area: we need more studies of field systems in both counties, together with accounts of the dynamics of both highland and lowland enclosure of subdivided arable and common. Someone ought to work on the drainage of the coastal fringe of Lancashire and its financing.

In terms of rural society, perhaps the greatest need is for studies mixing agriculture with industrial activity. The whole character of dual economies in Lancashire is worthy of much more discussion than it has received so far, and these would inevitably lead into questions of farming type and character on the one hand and the development of rural industrial employments on the other. The real gem, though, would be a full study of the reaction and response of Lancashire agriculture to industrialization and urbanization after 1700. The standard contemporary account by Holt (1795) reveals a highly sophisticated commercial system of agriculture in southern Lancashire: how recent were these changes and what form had they taken?

(ii) Urban Society. My overall impression is that North-Western towns are not well documented. Preston, for instance, has virtually no borough archive surviving from before 1660 and Lancaster is not dissimilar. Other towns have only poor records because they failed to achieve borough status during the Middle Ages and remained unincorporated until a late date. Poor records make for unappetizing fare, but again, much less has been written than one might expect. The chapter on medieval towns in *The Archaeology of Lancashire* (there is no chapter on post-medieval towns, itself a significant omission) reveals the poverty of archaeological understanding and offers its own research agenda. The historians have contributed four books of note: Willan’s *Elizabethan Manchester*, the collaborative history of Lancaster edited by White, Hunt’s history of Preston, and the rather narrower discussion of the slave trade in Lancaster by Elder.13

 Appropriately enough, much of the energy of urban historians has been absorbed by Chester: amongst other writings there are articles by Kermode and Alldridge (drawn from an unpublished thesis) and a short monograph on trade by Woodward; but much of the earlier writing on Chester will doubtless be superseded by *V.C.H. Cheshire: The City of Chester*, now soon to appear. But where are the studies of Liverpool and Manchester in the century after 1650 or 1660, or of the second-rank Lancashire towns such as Bolton and Wigan? It may not be possible to offer terribly satisfactory accounts of any Lancashire town in the sixteenth or early seventeenth century, but there do seem to be abundant opportunities for urban historians in Lancashire, whether looking at urban self-government and politics, the emergence of mercantile élites, their consumption and political attitudes, as well as for more traditional accounts of trade after 1660.

(iii) Social History . . . and Local Government. Connecting the two may seem pretty preposterous, but the shape of research in Lancashire actually makes the pairing sensible. There are two important areas in which work done nationally needs to be tested locally, and here local historians, especially those with a bent for counting, have a role to play. The first is the work of marriage seasonality undertaken by Kussmaul. In essence Kussmaul showed that different types of economy displayed distinct marriage seasonalities, and that it was therefore possible to work backwards from what could be easily discovered and categorized, marriage seasonality, to determine the character of local economies. She regards parishes which display no seasonal peak in marriages (that is, without a tendency for marriage to occur in either the spring or the autumn) as reflecting the presence of rural industry. In broad terms this appears to work:

it is possible to identify industrializing parishes which had lost a spring or autumn peak and where marriages were distributed across the year. In her sample Kussmaul has a mere ten parishes or 5.3 per cent of all Lancashire parishes in her observation. The seasonality of these parishes is assessed over three different periods, 1561–1640, 1661–1740, and 1741–1820, giving thirty observations. Of these, twenty-one are her X category, without a peak season for marriage, and so indicating rural industry. But the parishes she examined are by no means industrial. If the Kussmaul method works, then it may well be a wonderful method of tracking the development of industry within Lancashire.\footnote{A. Kussmaul, \textit{A General View of the Rural Economy of England, 1538–1840} (1990).} But be warned: an undergraduate project at the University of Central Lancashire was able to show that the apparently flat distribution of marriage seasonality in an east Lancashire parish actually concealed peaked distributions belonging to different occupational groups. There is room for someone to explore these issues within either county.

The second area concerns recently published work by Adair on the pattern of bastardy within pre-industrial England. The broad English pattern is well known after the work of Laslett: over the seventeenth century the level of bastard-bearing rose from the last third of the sixteenth century to a peak about 1610–20, then fell to its lowest recorded level during the Interregnum. In the North-West Adair found that the level of illegitimacy was consistently higher than in the country as a whole, confirming some contemporary comment. Moreover, bastard-bearing was possibly socially different from the pattern established for south-eastern England, where illegitimacy was increasingly the misfortune of the poor. Swain has already pointed to the high levels of acknowledged illegitimate children in the wills of yeomen and others from the parish of Colne. Both the demographic pattern and the social context appear to be different in the North-West, and here there is room for the enterprising historian to discover the chronological and geographical edges of the phenomenon and much more besides.\footnote{Richard Adair, \textit{Courtship, Illegitimacy and Marriage in Early Modern England} (1996); Swain, \textit{Industry before the Industrial Revolution}, pp. 25–7.}
This leads very directly to the larger study of the relationship of people to the state, an area which can be divided conveniently into two categories. The first is the way in which the English state treated subordinate jurisdictions during the process of state formation and 'centralization'. This forms a theme of Thornton's work on Cheshire, of which a couple of foretastes have already appeared and on which a book, extending the Cheshire work to look at other autonomous jurisdictions, can be expected. A second area of study is the way in which the state, through its local agencies, tried to reform individual activity, especially between about 1560 and 1640 (a movement loosely called 'the reformation of manners'). Areas which received especial attention include extra-marital (or pre-marital) sexual activity, drunkenness, alehouse frequenting and 'sports', especially such social recreations as piping or dancing on a Sunday after (or even during) church services. The redefinition of such activities as criminous, and their persecution, was a complex and (as yet) poorly understood process, but whilst in one respect it is a dimension of social history, in another it is an aspect of local government. Hindle has recently written about the struggle to suppress the Little Budworth (Cheshire) wakes in 1596, an offcut from a larger forthcoming book on *The State and Social Change* which again will draw heavily on Cheshire. Cust and Lake have described one Cheshire gentleman, Sir Richard Grosvenor, who was in the forefront of the campaign to establish a more perfect morality, and Cust has now edited a selection of his surviving papers. King has discussed the campaign to suppress alehouses in Lancashire in the seventeenth century. Quintrell, Wilkinson, and others have all discussed other aspects of the local government of Lancashire and the activity of the commission of the peace, particularly in


the half century before the Civil War. Morrill has written both generally on local government in Cheshire and more specifically on the county during the Civil War. All this material needs to be gathered together and placed in a larger context of change in the half century or so before the Civil War. To some degree this will be done by Hindle in his forthcoming book, but in Lancashire the issue needs to be read in terms of a conflict between alternative visions of society constructed along religious lines, divisions which appear most plainly in the agitation against the Lancashire Book of Sports in 1616–17.

Later seventeenth-century county government—and its social dimensions—appears to be less well explored in both Lancashire and Cheshire and here again there are clear opportunities for research, some of which I shall review further in section (v). Poor relief has been little explored to date but I will merely observe that Dr Steve King of the University of Central Lancashire is engaged in work which may well fill this gap. I might add that I am bored with the Lancashire witches (on which see a recent article by Swain): despite the richness of the material contained in Pott’s Wonderful Discovery (1613), there seems to be too little to offer a really satisfying account of an ultimately puzzling episode. Perhaps someone should try.

(iv) Religion. The early modern history of Lancashire has spawned one great book, Haigh’s Reformation and Resistance in Tudor Lancashire (1975). This is probably the one work of early modern Lancashire history which people without any


21 For which see the recent account by K. L. Parker, The English Sabbath (1988), ch. 5.

interest in the county are compelled to read. Haigh’s studies of the diocese of Chester also remain authoritative; there is too the fuller and more widely ranging V.C.H. account by Lander and Thacker. But early seventeenth-century Lancashire has not attracted anything like the same attention. Hilton’s book on the Lancashire Catholics is simply not in the same league as Haigh and, in any case, the point about Haigh is that he is interested in the emergence of and relations between two religious communities. Denominational history is not the same at all. Yet most of the writing which comes to mind is denominational: Blackwood on plebeian Catholics, Bossy on eighteenth-century Lancashire Catholics, soon to be amplified by Dr Marie Rowland’s study of the Catholic community seen through the 1767 census, and Morgan on Quakers. There is a little on the Established Church in Cheshire in the eighteenth century, but generally the Established Church seems to have been overlooked, and the history of Catholicism needs to be reintegrated into a fuller account of the religious life of Lancashire and the tensions between the communities in the county.

(v) Political Society. The reformation of manners and the conflicts it engendered within Lancashire society is obviously highly political, but here I want to reserve consideration of the political to the post-Restoration period. Lancashire, more than Cheshire, has seen some interesting work here by Glassey (on the development of parties) and Mullett (on the politics of

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individual towns). But someone needs to tackle the politics of Manchester, Liverpool, and the county, and must do so in awareness of a development at the national level which needs to be embraced and applied to Lancashire. This is the new appreciation that Jacobitism was a political movement of coherence and substance rather than a series of dining clubs. The key work here is probably P. K. Monod’s *Jacobitism and the English People* (1989), which contains a great deal of Lancashire material. Hence there is a need to look in greater depth at what might anachronistically be called the political Right, and tie the development of gentry faction and party to the external crises of the later seventeenth century: the Restoration itself, the Exclusion Crisis, the Glorious Revolution. And one of the things which Monod teaches us is that Jacobitism was ‘popular’: it was not merely a matter for the gentry but was attractive to the man in the street, and so Jacobitism ought to lead the ambitious historian into such difficult questions as the religious and political culture and preferences of the wider society. There is a book to be written with a title like *Religion, Politics and Political Opposition in Lancashire, 1660–1745*, which would tie together a number of different strands of Lancashire history: borough and county politics, the slide into political and occasionally armed opposition, and the religious fractures of the county. It would be a very useful book and, potentially, a very great book.

(vi) Industrialization. I have already suggested that there is a tendency to see industrialization beginning about (if not actually on) 1 January 1780. Industrialization is seen to be

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about the shift to the factory and the beginning of a proletarian wage-labour force. Yet no one would disagree that this was merely a stage in a much more extended process. There is remarkably little modern writing on the late seventeenth- and eighteenth-century development of industry in Lancashire. The recent collection of essays edited by Mary Rose claims to be *The Lancashire Cotton Industry: a History since 1700* (1996), but the eighteenth century is weakly represented. There are exceptions: Swain on industry (and much more) in Blackburn hundred before 1640, a paper by Schwarz on Blackburn hundred c. 1660–1760 and another by Walton relating Lancashire developments to the proto-industrialization debate, and, of course, Langton’s book on coal mining in the south-west of the county. But one of the most striking features of Lancashire history is the continued dominance of Wadsworth and Mann’s *The Cotton Trade and Industrial Lancashire* (1931); another is the lack of writing about the plurality of Lancashire’s industrial trades. We have, for instance, no account of the metalware industry in south Lancashire. There seem to be endless opportunities for historians to explore the developing industrial economy of the county before 1780.

Rather than puzzle over why so little has been done, it would be better to remark that both Lancashire and Cheshire—in common with most English counties—present an enormous range of possibilities for the enquiring historian, whether postgraduate, academic, or unsalaried. I have suggested where some big books could usefully be written, but anyone attempting a monograph, whether on Lancashire agriculture during the eighteenth century, the county before the Civil War, its political and religious history 1660–1745, or eighteenth-century industrialization, will have to base his or her work on that of others. All history writing is ultimately collaborative; and so the local historian, interested in a single place, has a major role to play in both the achievement and success of these works.

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Big books, though, take a long time to gestate and when they appear they tend to be inaccessible—whether through their cost or the character of the writing—to the lay public. So I end by making a slightly different proposal. There is room for a more accessible series of studies of the county, probably organized chronologically, which might consist of collections of commissioned essays on the model of the Rose collection on the cotton industry or single-authored monographs. Cheshire is ahead of Lancashire on this; Lincolnshire has a very successful history of the county in about a dozen volumes. The aim of such a series would be not only to sponsor new work, but also to present that work in a forum in which it might be read not just by undergraduates, professionals, and the committed local historian, but equally by that interested, sometimes well-informed audience that we as historians crave but do remarkably little to encourage. Of course, such a project would, of necessity, be a long-term one; but such an objective might concentrate minds, stimulate research, and so lead to a golden age in Lancashire history.