Introduction

The state of the historiography

When George Eliot wrote *Silas Marner*, she was acutely aware of the regional differences in religious cultures through which Silas moved. Even people 'whose lives have been made various by learning', she wrote, find it hard to maintain their beliefs when they are transported into a new region, 'where the beings around them know nothing of their history, and share none of their ideas . . . in which the past becomes dreamy because its symbols have all vanished'. In Silas' move from a northern, strongly Nonconformist chapel setting – its familiar phrases like an 'amulet worn on the heart . . . the fostering home of his religious emotions' – to the large Anglican church of Raveloe and its associated culture, Eliot captured one of the fundamental regional contrasts of her time. Silas, she wrote, was vaguely conscious that 'each territory was inhabited and ruled by its own divinities': by its own 'native gods', whose influence was locally contained and not transferable. In the consequent disassociation of Silas from religious belief, a response to this regional transition and confrontation with people of differing views, she defined a fundamental cause of religious disillusionment.[1]

This was a subtle and sensitive lesson from a novelist of great intuition. We shall need to keep it in mind. For in her preoccupation with these themes, and in her awareness of regional contrasts and their effects, George Eliot was articulating thoughts which are now remote from the minds of many historians. It is often customary to begin academic books by stating the scholarly gaps that one's work tries to fill, and it is appropriate to do that here, albeit in a more austere style than that penned by George Eliot. This academic problem is easily stated. By comparison with many other countries, particularly with France

[1] George Eliot, *Silas Marner* (1861, Harmondsworth 1969 edn), ch. 2, pp. 62–4. The quotation is given more fully on p. v of this book.

and America, the understanding of English and Welsh religious regions is often crude and limited.[2] The major religious denominations in England have been described at a basic county level, but they have not been analysed in a more detailed way for the whole of England and Wales.[3] There have been many regional historical studies; but in these a well-judged national picture of religion has been forgone in the usual closeness of local focus. The major religious sources that lend themselves to such analysis have not been studied in any nationally comprehensive way.

Inadequate understanding of spatial patterns of religion has constrained many areas of knowledge, and has lost us many of the insights which were visible to George Eliot. Some of these should be mentioned. Assessments of the role of religion in politics, for example, have not paid much attention to region, despite the acknowledged primacy of religious influences upon political parties

[2] For examples of the many French and American studies on this subject, see G. le Bras, *Etudes de Sociologie Religieuse* (Paris, 1956), 2 vols.; F. Boulard and G. le Bras, *Carte Religieuse de la Rurale* (Paris, 1952); F. Boulard, *An Introduction to Religious Sociology* (1960); X. de Planhol and P. Claval, *An Historical Geography of France* (Cambridge, 1994); P. Deffontaines, *Géographie et Religions* (Paris, 1948); R. D. Gastil, *Cultural Regions of the United States* (Washington, 1975); E. S. Gaustad, *Historical Atlas of Religion in America* (New York, 1976); J. R. Shortridge, 'Religion', in J. F. Rooney, W. Zelinsky and D. R. Louder (eds.), *This Remarkable Continent: an Atlas of United States and Canadian Society and Cultures* (Texas, 1982). A sense of the scope of this subject in other countries can be gleaned from the huge bibliography in C. R. Park, *Sacred Worlds: an Introduction to Geography and Religion* (1994), pp. 288–312; G. J. Levine, 'On the geography of religion', *Transactions of the Institute of British Geographers*, 11 (1986), 428–40.

[3] The most notable discussion has been J. D. Gay, *The Geography of Religion in England* (1971). Our national work differs from his in a number of ways. Computerised methods were not available to him, and this limited what he could achieve. He did not include Wales. With a few exceptions (e.g. Lancashire), his data were described at county level, and therefore his maps were much less detailed than our own. Nevertheless, there is much of enduring value in his work, notably on broadly drawn geographical patterns. He also used more modern data, like the Newman Demographic Survey (which collected Mass attendance figures for 1958–62), or denominational marriage data for the early 1960s. *Ibid*., pp. 95, 284, maps 19–20. He covered groups like the Jews, and 'quasi-Christian groups and eastern religions'. *Ibid*., chs. 10–11. In our opening chapters, the aim is to complement his findings with much greater resolution, rather than re-tread ground that he covered; while in later chapters this book's approach becomes very different. There are also some maps of 1851 data in H. McLeod, *Religion and Society in England, 1850–1914* (1996), pp. 29, 33, 63; and his 'Religion', in J. Langton and R. J. Morris (eds.), *Atlas of Industrializing Britain, 1780–1914* (1986), pp. 213–15. See also Park, *Sacred Worlds*, pp. 70–5, as based on Gay. For a more regional study, the approach of which prefigures this book, see K. D. M. Snell, *Church and Chapel in*

and elections prior to the early twentieth century.[4] Compared with many other European countries, the cartography of electoral sociology in the nineteenth century has almost never been related to that of the religious denominations.

There have been renowned debates about the effects of Methodism on political behaviour, from Halévy to Eric Hobsbawm, E. P. Thompson and others,[5] or the roles of religion in fostering innovation, entrepreneurship and industrialisation.[6] These ought to have had an analytically regional focus, relating political action or entrepreneurship closely to patterns of religious affiliation. Yet such debates proceeded with little spatial or geographical sense of where the denominations were sited, or of how strong they were in applicable areas.

There is in Britain poor spatial understanding of popular religion, 'zones' of religious practice, areas of 'dechristianisation', and of cultural and political 'frontiers' defined via religion.[7] Nor has study of regional or occupational cultures connected much with regional patterns of religion, except at the most local of levels. Questions about

the North Midlands: Religious Observance in the Nineteenth Century (Leicester, 1991).

[4] See for example K. D. Wald, *Crosses on the Ballot: Patterns of British Voter Alignment since 1885* (Princeton, 1983), pp. 10–18. Such a statement is most pertinent to historiography on the period before about 1885, although some would apply it later too.

[5] E. Halévy, *A History of the English People in the Nineteenth Century: vol. 1: England in 1815* (1913, 1970 edn); E. J. Hobsbawm, 'Methodism and the threat of revolution', *History Today*, 7 (1957), also in his *Labouring Men* (1964); E. P. Thompson, *The Making of the English Working Class* (1963, Harmondsworth, 1975 edn), ch. 11. Hobsbawm pointed in general terms to the regional coexistence of Methodism with political radicalism, but this historiography did not much advance understanding of how local geographies of Methodist denominations related to regional socio-economic and political conditions. Compare D. Hempton, *Methodism and Politics in British Society, 1750–1850* (1984, 1987 edn), p. 236: 'The most satisfactory way of analysing the relationship between Methodism and politics in English society c. 1750–1850 is to root Methodism as firmly as possible in its religious, social, geographical and chronological context.'

[6] One summary was M. W. Flinn, *The Origins of the Industrial Revolution* (1966, 1976 edn), pp. 81–90, a text that took up some of the ideas of Tawney, T. S. Ashton, Hagen, McClelland or Kindleberger, to review possible links between certain Nonconformist denominations and industrialisation. This remains among the best treatments of the theme in economic historiography. Even so, Flinn's discussion of possible educational and attitudinal influences of dissenters upon economic growth lacked geographical specificity. A similar point could be made about many works which discuss the possible economic influences of Puritanism.

[7] Compare M. Vovelle, *Ideologies and Mentalities* (Cambridge, 1990), pp. 113, 159–62, on France.

the longer-term continuity of such cultural regions and patterns are not often raised.[8] The important issue of whether industrialisation fragmented and diversified the range and cohesiveness of regional cultures is poorly addressed in general,[9] and lacks connection with religious history. This is despite the marked proliferation of denominations during industrialisation, and the strongly regional identities of Roman Catholicism, Wesleyan and Primitive Methodism, Bible Christianity and many others. It is also despite the obvious relevance of this issue, like that of occupational cultures, to arguments about 'the making of the English working class'.

Requisite economic histories of the Anglican and other churches might have made much clearer the regional strengths and weaknesses of the respective churches. Yet the modern economic history of religion remains almost non-existent as a subject: most economic historians studying the period after about 1660 have an avid propensity to ignore anything religious, and the disciplinary allure of economics rather than history has brought little profit in this quarter.[10]

Such neglect is less apparent in demographic study – so distinguished in recent English historiography. This subject has had to consider religious contexts. Nonconformity had a major effect upon parish registration, especially after about 1780. Parochial Non-

[8] One exception here (on a rather earlier period) has been M. Spufford (ed.), *The World of Rural Dissenters, 1520–1725* (Cambridge, 1995).

[9] For discussion of this issue, see J. Langton, 'The Industrial Revolution and the regional geography of England', *Transactions of the Institute of British Geographers*, 9 (1984), 145–67; K. D. M. Snell (ed.), *The Regional Novel in Britain and Ireland, 1800–1990* (Cambridge, 1998), ch. 1, and K. D. M. Snell, *The Bibliography of Regional Fiction in Britain and Ireland, 1800–2000* (forthcoming), introduction.

[10] This neglect is remarkable when one considers the resourcefulness of historians on so many other issues. Aspects of the economic history of the church in the eighteenth and nineteenth centuries are covered in a few books like G. F. A. Best, *Temporal Pillars: Queen Anne's Bounty, the Ecclesiastical Commissioners and the Church of England* (Cambridge, 1964); E. J. Evans, *The Contentious Tithe: the Tithe Problem and English Agriculture, 1750–1850* (1976); F. Heal and R. O'Day (eds.), *Princes and Paupers in the English Church, 1500–1800* (Leicester, 1981); R. J. P. Kain and H. C. Prince, *The Tithe Surveys of England and Wales* (Cambridge, 1985); P. Virgin, *The Church in an Age of Negligence* (1988); and (for an earlier period) C. Hill, *Economic Problems of the Church* (Oxford, 1956). There are a number of usually very local articles, especially on tithe, often written from standpoints within agricultural history. This historiographical oversight contrasts markedly with voluminous contemporary evidence and publications, and is despite the many subjects open to study: such as tithe, charities, ecclesiastical landowning, enclosure and the clergy, glebe farming, pew rents, clerical fees, the economic effects of church building, Queen Anne's Bounty, or the Ecclesiastical Commission and financial reorganisation.

conformity, and its wider geography, have evident relevance for demographic sources. It is less often observed, however, that parish registers are an *Anglican* source. Their quality is likely to be highest where there was strong Anglican control or monopoly, rather than in regions where Nonconformity was more influential. We shall see that certain regions, and types of parishes, favoured the Anglican Church (southern and south midland counties, lowland areas, nucleated parishes, those with concentrated landownership, perhaps those with low demographic growth, and so on). Rather different regions and parishes often proved more hospitable to Nonconformity, especially to 'new dissent' (upland settlements, industrial areas, those which were 'open' in settlement, with scattered landownership, often with rapid population growth rates, areas of reclaimed or marginal agricultural land, and the like). Demographers who apply searching criteria to choose the best parish registers may easily alight upon Anglican monopolised parishes and areas to study, running a risk of becoming victims of their own assiduity and care. Such areas may share certain socio-economic, demographic and other historical attributes favourable to the Anglican Church, but these were not necessarily representative of other important regions, notably those which had fostered strong Nonconformity. Such possible connections need to be suggested, even though they almost certainly do not unsettle results from the widely distributed parishes used by leading English historical demographers. For those parishes frequently contained more Nonconformists than was ideal for the purposes of vital registration and family reconstitution; they had wide regional representativeness; the demographically reconstituted parishes were larger than average; the Anglican church comprised the major part of the population during the parish-registration era; and the Anglican data were reassuringly tested in many ways against figures from early civil registration.[11] Other such considerations could be added in defence of the Cambridge demographic findings, but the import of religious regions for this most advanced field of English historiography should be clear.

Within religious history itself, the geography of religion should be

[11] The main English parishes studied by the Cambridge demographers are mapped in E. A. Wrigley and R. S. Schofield, *The Population History of England, 1541–1871: a Reconstruction* (1981), pp. 40–2, and E. A. Wrigley, R. S. Davies, J. E. Oeppen and R. S. Schofield, *English Population History from Family Reconstitution, 1580–1837* (Cambridge, 1997), p. 31.

fundamental to understanding issues like church governance, schism and denominational formation, church and chapel building and the spread of architectural styles, religious education, charity and welfare, the evolution and influence of circuit systems, the biographies of religious leaders, regional cultural influences and biases affecting religious doctrine, popular religion, the urban or rural bases of denominations, and many other such matters. However, one often finds such subjects discussed with limited awareness of regional location. And denominational histories frequently prefer to imply wide affiliation and to concentrate on mobile personalities; an understandable stress is sometimes placed on expansive universality rather than the local church, and this is commonly linked with theological universalism. From such historical writing, converting the particular to the general, regional structures can often emerge in an impressionistic form only.

Issues of religious geography therefore occur across many areas of historical enquiry. These go well beyond the immediate history of religion itself, where they bear on virtually all aspects of denominational history. Despite this, it appears that secularised academic minds, limited spatial thinking, a predilection for national rather than regional or local description, and the fragmentation of historical specialisms have minimised awareness of religious regions and their importance. We are in danger of losing the sensitive regional knowledge and sense of difference that structured books like *Silas Marner*.

If we lean back from such reflections, and think instead of technical expertise and method, another point would be widely acknowledged. As far as method is concerned, historical studies of religion linger behind many other areas of social scientific and historical enquiry. There are salient exceptions,[12] but as a specialism amenable to quan-

[12] R. Currie, *Methodism Divided: a Study in the Sociology of Ecumenicalism* (1968); Gay, *Geography of Religion*; A. Everitt, *The Pattern of Rural Dissent: the Nineteenth Century* (Leicester, 1972); A. D. Gilbert, *Religion and Society in Industrial England: Church, Chapel and Social Change, 1740–1914* (1976, Harlow, 1984 edn); H. McLeod, *Class and Religion in the Late Victorian City* (1974); R. Currie, A. D. Gilbert and L. Horsley, *Churches and Churchgoers: Patterns of Church Growth in the British Isles since 1700* (Oxford, 1977); N. Yates, 'Urban church attendance and the use of statistical evidence, 1850–1900', in D. Baker (ed.), *Studies in Church History, 16: the Church in Town and Countryside* (Oxford, 1979); B. I. Coleman, *The Church of England in the Mid-Nineteenth Century: a Social Geography* (1980); A. M. Urdank, *Religion and Society in a Cotswold Vale: Nailsworth, Gloucestershire, 1780–1865* (Berkeley, 1990); Snell, *Church and Chapel in the North Midlands*. There has been

titative and related methods this large subject seems diffident and undeveloped. Orthodoxies have been little examined and refined by such methods. This is despite the fact that the historiography of religion overflows with arguments and views, expressed through literary or impressionistic statements, that are nevertheless of an essentially quantitative nature. Methodological innovation has slipped between the disciplinary isolation of a few interested geographers, and the scepticism of some religiously committed historians towards the secular bias of religious sociology and its methods. Quantitative approaches in much religious historiography have been limited, definitional precision has often been lacking, and variables have sometimes been inadequately constructed or handled. What some measures may indicate about the nature of religious provision or attendance has sometimes been insufficiently explained. The historiography contains many articles and editorial introductions providing valuable assessments of major sources *as sources* (those of 1676, 1715, 1829, 1851 and so on). But there have not been the intensive ensuing research projects and analyses that are plainly justified. Three decades ago, one author commented critically that 'The history of the empirical investigation into religion in this country over the last hundred years is littered with examples of dogmatic and general conclusions based on very shaky evidence.'[13] One would not word this in such strong terms now, but the sentiment might still be endorsed.

Research aims and methods of this book

Seeing the historiography from such perspectives, and with these points in mind, it seemed that the most creative way forward was to adopt the following main priorities:

(i) To computerise the published 1851 *Census of Religious Worship*, correct those registration-district data for omissions, test their reliability, develop further measures of denominational strength from the data, and map those comprehensively

growing use of quantifiable evidence in religious history, producing fascinating work like Urdank's book; but the generalisations made by R. A. Soloway back in 1972 remain valid: this has still not developed into any significant broader analytical advance. R. A. Soloway, 'Church and society; recent trends in nineteenth-century religious history', *Journal of British Studies*, 11 (1972), 152.

[13] Gay, *Geography of Religion*, p. 22.

for England and Wales.[14] This would allow far more refined cartographic understanding and analysis of religious regions, and would permit many questions and debates about the extent, siting and reciprocity of denominations to be resolved.

(ii) To construct a series of closely related parish-level datasets, allowing analysis and mapping via computer cartography, of the 1851 *Census of Religious Worship* data on denominational provision, free and appropriated sittings, attendances, Sunday school attendances and related information. Even with a small team of researchers this was evidently too large a task to be done for the entire country. It was decided instead that fifteen counties would be selected as representing certain key features of the national geography of religion, informed by the registration-district analyses.

(iii) To compare the 1851 data with earlier sources, particularly the Compton Census of 1676, and (by way of a check on the mid nineteenth-century data) with the 1829 returns of non-Anglican places of worship. Much data from those earlier sources would also need to be computerised. This was likely to be a complex matter, given evidential and design differences between the historical sources. So a further aim was to create methodologies that enabled longitudinal and latitudinal study of these data.

(iv) To relate the religious and cultural data of 1676 and 1851 to many socio-economic variables, to answer questions about the local contexts, influences and regional cultures affecting denominational geographies and religious 'pluralism'. This was clearly best done at parish level.

(v) To analyse in their own right the socio-economic data that was being used, and to develop arguments or models of local/regional contexts and parochial divisions, incorporating cultural, religious, demographic and economic characteristics. The need here was not to advance deterministic arguments for their own sake, but rather to explore the adequacy of deterministic and contextual considerations affecting religious strength and siting, and to show precisely how significant or

[14] Coverage was considered of Scotland as well, but (as discussed in chapter 1) the survival of the Scottish data is poor compared with England and Wales. For this reason Scotland was omitted.

insignificant they may have been in different areas. The potential contribution of a more quantitative approach to such on-going debates was self-evident, allowing many historical questions to be resolved with much more precision.

Foremost among a very large set of research questions, it was hoped to assess how durable over time the geography of the major denominations had been, how they reciprocated or undercut each other regionally, what was the role of Sunday schools, what was the denominational significance of 'free and appropriated' sittings, and how important were social controls as exercised particularly through landowning patterns. A related aim was to consider where and how 'secularisation' (defined by falling church attendances) became apparent, and what its regional dimensions were. It was hoped to test and develop some of the rather ahistorical theories of religion in the social sciences, notably theories of 'secularisation', using the rich veins of computed data being created.

In short, a firmer sense of the regional features of religious history was felt necessary to extend the historiography of religion, to augment historical awareness of cultural regions (and the role of religion in their origins and persistence), and to enhance understanding of the importance of religion for related issues. We hope that this book, and the huge datasets constructed over many years for it (now made available to the research community),[15] will address these research priorities and extend understanding of these subjects.

As will become clear, this research has been conducted in a technically more sophisticated way than previous British studies. This will bring the history of religion to the fore of current techniques and methods. No closed or tight definition of the 'geography of religion' is adopted in this book, for the self-containment of disciplinary areas is most unhelpful. The approach is inter-disciplinary: very historical, and 'geographical' in its quantification and stress on spatial and regional understanding in the history of religion. Some readers from particular disciplines may encounter unfamiliar approaches and methods; but they can be reassured that many steps were taken to keep the text approachable, readable, and within reach of any modern student trained in history or the social sciences.

[15] The data collected for this book are being deposited with the ESRC Data Archive at the University of Essex.

There are gains and losses in pushing ahead in this way. A priority for this research is the view that many features of the religious and cultural regions of the Victorian period have yet to be disclosed in an objective manner. There is also a growing sense that the many research subjects that bear on 'cultural regions' – dialect, the English, Welsh and Gaelic languages, political behaviour, patterns of folklore, regional fiction, surname distributions, migration fields, vernacular architecture and the like – should in due course be inter-related via broader syntheses, if not via group projects. This requires careful work within each field that lays an appropriate groundwork for this; and, to aid objectivity and comparison, much of that groundwork needs to be of a quantitative and geographical character. As public discussion focuses ever more intently upon the distinctiveness of parts of the British Isles, upon national and regional assemblies, upon regional voting patterns, upon the real or supposed identities of different areas, upon the evolution and drawing of cultural boundaries, and other such questions, it is crucial for modern British 'society' (if decentralisation is to mean anything positive) that the historical subjects be properly researched. The writing of religious history has sometimes been thought a reclusive and self-indulgent pursuit of dwindling contemporary significance – but the study of religious geographies, and the cultural and political regions associated with them, now have an increasingly obvious relevance for very prominent modern issues.

Such research is probably best conducted via the relatively impartial quantitative methods adopted here. There are losses involved in making less use of the rich literary evidence that has traditionally attracted religious historians, even though such omissions can be justified by pointing to the profusion of excellent and highly readable work already based upon such documentation. No historians would claim that the approaches adopted in this book are sufficient in themselves. However, given the priorities outlined above, few would dispute that there are considerable gains in taking religious historiography along this way in a more thorough manner.

Accordingly, the religious data for twenty-seven denominations from all 624 registration districts of the published 1851 Religious Census for England and Wales were computerised. Those data were corrected for omissions (as described in appendix B), new measures were formed to describe denominational coverage (see appendix C),