THE PURPOSE of this review article is to try to provide some guidance for New Zealand sixth-form teachers involved with the University Entrance topic on Great Britain, 1832-1868. The syllabus directs the teacher to cover the following problems:

'Industrialisation and urbanisation; economic growth and economic fluctuations; the situation established by the 1832 Reform Act; social and administrative reform from the 1830s to the 1860s; the rise, achievements and fall of Peel; the occasions, goals and results of agitation; the revival and results of political reform in the 1860s; party government and party organisation' (University Grants Committee Handbook, 1975, p. 158).

This is certainly an ambitious programme but it is one which provides an opportunity for the teacher and the students to familiarize themselves with the sorts of issues which are dominant in modern historical writing. It is, therefore, somewhat disappointing that the topic is one of the least popular, being heavily beaten in the popularity contest by those topics covering what was often the major element in Stage III university courses in the 1940s and 1950s: diplomacy and nationalism in the nineteenth and early twentieth centuries. This may not be the best way to preserve a diet of serious history against the intellectual porridge known as social studies. It also seems strange that more interest cannot be generated in the very society from which New Zealand's own governing traditions emerge.

The present discussion of those traditions in their original, early Victorian British context is marked by a great, not to say, bewildering variety of interpretations as well as by a rather self-conscious concern with method. The volume of work being done is grotesque in its largeness, causing two prominent historians to suggest that more researchers should move into the (relatively) unploughed fields of the eighteenth century. Thus no attempt is made here to cover everything published in the last decade, nor everything of importance (teachers who are not familiar with it may find the bibliography published annually in Victorian Studies exceptionally useful). In particular, discussion of the 1867 Reform Act and parliamentary party politics has been excluded, the first because little
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has happened since a sudden flurry of work in 1966–67, the second because most of the work has been fairly dull and straightforward.

Dull and straightforward are scarcely the words to use to describe some of the general studies which have appeared in recent years. The most notable exercise in providing a new interpretation is Harold Perkin’s *The Origins of Modern English Society, 1780–1880.* Perkin argues that these years saw a ‘more than industrial revolution’ with social causes and social antecedents. This led, through the emergence of social classes in the late 1810s, to the rise of a ‘viable class society’, in the third quarter of the nineteenth century in which classes had become ‘institutionalised’ (largely by the agency of organized religion). Rather than class war Britain saw a war of class ideals in which the entrepreneurial ideal emerged triumphant. The interpretation stated so baldly seems a little strained but there is a wealth of interest in this book which is perhaps safer in the hands of the teacher than of the student. Less idiosyncratic but no less interesting is J. F. C. Harrison’s *The Early Victorians, 1832–51,* one of a new series on British social history. Harrison covers the basic questions of wealth and poverty, social values and social attitudes with insight and humanity. It is largely ‘literary’ history, but none the worse for that. Its equally valuable companion volume, Geoffrey Best’s *Mid-Victorian Britain, 1851–75,* should be read by all interested in the nature of history. Professor Best positively leaps out of every page — witty, charming, chatty and immensely knowledgeable — to grab the reader. Best’s own original mentor, the late G. S. R. Kitson Clark, has also provided an excellent introduction to Victorian Britain, *An Expanding Society: Britain 1830–1900.* Notice should also be taken of R. K. Webb’s *Modern England* which covers a much broader period but for that very reason may be of use to those who feel a lack of confidence in their training in modern British history. Lastly, the economic background is soundly described in Peter Mathias’s *The First Industrial Nation.*

Mathias needs little supplementation from the most recent researches into the purely economic aspects of the period for there seems to be no great controversy or revisionist work on the essentials of the mid-nineteenth century economy. Matters are different, however, when we turn our attention to social structure. Already Harrison and Best are looking somewhat outmoded. Recent publications in this area are notable for their self-consciousness of one sort or another. John Foster in his *Class Struggle and the Industrial Revolution: Early industrial capitalism in three English towns* seems at times to be waving the red flag simply to annoy the bulls among us. Who can resist temptation of this sort? Foster’s is a brilliantly bad book. On the basis of the experience of Oldham (with brief forays to South Shields and Northampton) Foster sees the emergence of a revolutionary working-class consciousness from trade union and earlier radical experience in the 1830s and 1840s. This consciousness was perverted by the fiendishly clever bourgeoisie who used the rise of a labour aristocracy, Irish immigration, and economic changes to usher in a long
period of bourgeois-dominated class collaboration. A wide range of material is used, often in a highly original fashion, to sustain the argument. Yet the argument in fact collapses, both because of the dubious use of the empirical data and because of the insistent dogmatism. The working classes suffered enough in the nineteenth century without being posthumously lectured to by late twentieth-century Marxists on what their 'true consciousness' should have been (and so palpably was not in most cases). The ideology of some 'proletarian vanguard' cannot be delineated by reference to the ideas of John Fielden. The statistics purporting to demonstrate the relation of poverty to family life-cycles are scarcely justified and border on the spurious. It is perhaps significant that one diagram (p. 83) demonstrates that a large cotton mill in Oldham accepted a massive profit squeeze before reducing its wages bill — and restored that bill as soon as profits began to expand again. This hardly looks like part of a bourgeois plot to immiserate the workers. Foster would have done well to follow up his own remark that 'the employers themselves were trapped within [the economic] process' (p. 82). He might have found a greater pluralism of attitudes and experiences than is dreamt of in his philosophy.

A much sounder work informed by similar ideological preconceptions is Gareth Stedman Jones's *Outcast London*. This is a valuable study of poverty in Victorian London perhaps a shade too impressionistic and marred by a tendency to confuse the poor with the whole working class and by a totally unnecessary attack on the 'bureaucratic machine politics of Herbert Morrison's London Labour party' at the very end. The discussions of the casual labour market and the housing of the poor are worth particular attention. The housing of the poor and attempts to deal with the problem are dealt with by Enid Gauldie on a national scale. Both these works (despite the numerous statistical appendices and graphs in *Outcast London*) are not as well bodied-out as they might be quantitatively, a charge which cannot be levelled at Alan Armstrong's *Stability and Change in an English County Town: A social study of York 1801-51* and the collection of essays edited by E. A. Wrigley under the title *Nineteenth-century society: essays in the use of quantitative methods for the study of social data*. The latter is in some respects yet another of those increasingly tedious statements of intent to revolutionize the world of history which issue from time to time from the Cambridge population group and associates. Yet it provides insight into the newer research methods as well as data to flesh out the rather bare bones of books like Best's and Harrison's. The same could be said of Armstrong's study which also shows there is some danger of quantitative research into social history becoming a sort of high-powered antiquarianism in which bare facts are interesting in themselves and not necessarily in relation to each other. It is a little alarming to find such matters as education, religious life and poor law policy dismissed as 'traditional' social history' (p. xix), especially as Armstrong's own brief discussion of education is insufficiently critical.
of the reliability of the sources. These topics could well have been discussed in terms of their connection with the more purely demographic factors that Armstrong devotes most space to. Yet again, selective quarrying from this book should help the teacher to avoid talking in only the vaguest generalities about social structures.

Possibly the most important (and certainly the most self-conscious methodologically) of these recent studies of social structure is Michael Anderson’s *Family Structure in Nineteenth Century Lancashire.* Anderson is a sociologist with historical interests. This means that two of the four sections of his book are almost incomprehensible, being written in that strange sub-language sociologese (‘situations where *alter*’s socialisation is outside one’s own control or where his present relationships are not encapsulated by a close-knit network on which he is dependent and of which ego is a powerful member’). This sort of thing should not prevent one from recognizing the originality and importance of Anderson’s work—he is one of the first serious investigations of the family in an industrial revolution setting. The romantic notion that industrialization replaced loving extended families in which burdens were shared by rather mean and nasty nuclear families is finally buried (if only students learn that they will be saved from repeating some of the silly things modern undergraduates are prone to say). In fact it seems likely that various forms of the extended family were more common in the mid-nineteenth century than earlier (Foster makes a similar point). Anderson refuses to idealize this fact. His book is based upon a theory of calculative exchanges within extended family and (to a lesser extent) neighbourhood groupings. We come much closer in this work to the everyday life of the ordinary people than in half a dozen others which loudly advertize their commitment, humanity, and historical imagination.

Unfortunately, there is no satisfactory recent overview of the kinds of problems dealt with by Foster, Anderson and the others (beyond the relevant chapters in the general texts). Malcolm Thomis’s *The Town Labourer and the Industrial Revolution* has its virtues but does not get to grips with modern techniques and modern scholarship and spends too much time on industrial and political action and reaction (it reads, and perhaps was intended to read, like a safer version of the Hammonds’ *Town Labourer*). Equally, there is no satisfactory full-scale study of the most important popular radical movement of the period, Chartism. Instead, there is J. T. Ward’s *Chartism.* If most books on popular radicalism in the late eighteenth and nineteenth centuries have suffered from an excess of commitment to the radical cause Ward has singlehandedly tried to correct the balance. And failed. Four chapters—half the book—are wasted on the origins of the movement (‘Antecedents’, ‘Background’, ‘Foundations’, ‘Emergence’), wasted because we learn nothing from them except Ward’s own lack of sympathy with nearly all radical endeavours since 1780. The whole book is rather like a negative photo-copy of Foster’s; black becomes white and white, black. The various shades of grey that should colour
most history books are absent. The teacher looking for an introduction to Chartism would do better to consult Dorothy Thompson’s *The Early Chartists* (the introduction emphasises the rational and articulate aspects of the movement. This is followed by a selection of documents), Alex Wilson’s essay, or even F. C. Mather’s little pamphlet.20

That there is a need for a new general work on Chartism is amply demonstrated by some of the more specialized publications on the subject. Brian Harrison and Patricia Hollis in their study of Robert Lowery argue that the placing of Chartism solely within the context of history of the British labour movement distorts its variety — especially the possibility of links with middle-class radicals and eventual assimilation by the Liberal party.21 D. J. Rowe goes even further by trying to demonstrate that London Chartism was to a large extent middle-class.22 Elsewhere he has explained the supposed apathy of the London working class towards Chartism by reference to the lack of a recent radical tradition, the lack of any unifying local interest in the great metropolis, the dispersion of the trades, the absence of industrialization and the trade cycle, and the failure of the educated artisans to devise a programme of mass appeal.23 This approach has been disputed by Iorwerth Prothero on a number of counts. Firstly, Prothero justifiably points out that it was only in the early stages that Chartism was weak in London — in the 1840s the movement was more active there than in many other parts of the country, the peak of London Chartism coming in 1842 rather than 1839. Prothero also emphasizes the role of the less respectable trades and of leaders other than G. J. Harney and William Lovett who are usually seen as the two representative and polar figures in London.24 However, in a rather indirect fashion Weisser has questioned the proletarian nature of London Chartism.25 Clearly the nature of Chartism in London needs fuller study.26

On a more national scale J. H. Treble has confirmed what was already suspected, that the prominent role of Irishmen like Feargus O’Connor and Bronterre O’Brien (the subject of an excellent biography)27 in the movement is unrepresentative of general Irish attitudes. Irish immigrants by and large stood aloof from Chartism.28 Finally, mention should be made of three books which bear upon the origins of Chartism. Patricia Hollis and Joel Wiener have, with somewhat different emphases, studied the radical newspapers and periodicals of the early 1830s and the campaign to free them from the constraints of the newspaper tax. Middle and working class elements were both important in this campaign and shared certain hopes and assumptions concerning the power of knowledge to free men from their illusions and prejudices. Hollis in particular stresses the emergence of a new ideology (which she calls working class) providing a more rounded political, economic, and social theory than the old radicalism, though, rather confusingly, she also stresses the links between middle and working class radical attitudes and activities.29 Edsall takes up one aspect of ideology in his study of the movement which opposed the introduction of the new poor law, arguing that it had a reasonable degree
of success in the north of England in delaying implementation of the 1834 act and, indeed, in modifying central poor law policy by 1844. But he also shows the failure of the law’s Tory-radical opponents to develop any realistic alternative.  

There has been a significant amount of work done on wider aspects of poor law policy. J. R. Poynter has studied attitudes to the old poor law in the forty years before 1834 and concluded that Malthusian-influenced abolitionist sentiment reached its peak in the late 1810s and was of little influence in the crucial period 1832–34. J. D. Marshall has surveyed, with admirable brevity and skill, developments from 1795–1834. S. G. and E. O. A. Checkland have produced an edition of the Poor Law Report of 1834 with a longish introduction which tries a little too hard to rescue the commissioners from the enormous condescension of welfare statist posterity. The main controversy in this field, however, has arisen out of the attempt by an American historian, Brundage, to show that the new Poor Law strengthened rather than weakened the hand of the old, amateur gentry poor law authorities (i.e. that there was no switch to middle-class professionalism) and that the purpose of the Act was to favour the formation of local deference communities on a traditional model. This intrinsically unlikely theory has been shown to be actually untrue by Dunkley. Lastly, Eric Midwinter and others have shown that there were great continuities in the administration of the poor law before and after 1834. In particular, outdoor relief continued to be the major form of relief and there were no significant economies made (rather the reverse). Whatever the intentions of the Act its results, at least in the short term (and ignoring the reaction it aroused), were quite minimal.  

The study of the poor law in its various aspects — the formulation of policy, reform, reaction, and implementation — is but one part of a wider topic which has been under scrutiny in recent years due to the pioneer efforts of Professor Oliver MacDonagh in the 1950s, the growth of (central) government. Lubenow has attempted something like a general survey for the period from 1833 to 1848, covering the poor law, public health, factory legislation, and railway legislation. His thesis seems to be that the main support for government intervention came from those imbued with what he calls an ‘incrementalist’ philosophy which favoured experimentation and pragmatic solutions to pressing problems, the opponents were largely influenced by an ‘organic’, ‘traditionalist’ view of society and politics which looked to supposedly ancient liberties and constitutional arrangements. He is concerned to emphasize that the reforms of the period did not really lead to centralization, but rather to central supervision of local administration. On both counts the argument is overdrawn. The ideological division is an odd one and leaves Lubenow rather baffled by the fact that the same people could support or oppose centralization depending on the area of policy under debate (e.g. see pp. 133, 171). Had he started with this fact he might have discerned quite different underlying divisions. As to the limits of centralization, Lubenow
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tends to set up straw men to knock down (e.g. on p. 104 we are told the 1848 Public Health Act was not ‘a collectivist measure nationalising the the health services of England in a tightly organised and centralised system’). Of course central authorities continued to work through local administration (as they often still do); the crucial point is that there were now central authorities. One such was in education (from 1839) and the growth of the Education Department has been subject to a sound analysis by J. S. Hurt.33

The other major enterprise on the topic of government growth has been the collection of essays edited by Gillian Sutherland.34 In an unusual essay Finer describes the methods by which Benthamite ideas were transmitted within and around the civil service while Ryan looks at J. S. Mill’s views of the bureaucracy. Jenifer Hart disposes of the notion that the Northcote-Trevelyan Report was concerned to provide jobs for the middle-class boys (rather it was concerned with efficiency and raising the calibre of the civil servants) while Donajgrodzki shows that Trevelyan’s notions of efficiency warped his view of the achievements of the Home Office officials in the period 1822–48. Five other essays round out this collection which points to the need for a work of synthesis. Arthur J. Taylor’s Laissez-faire and State Intervention in Nineteenth-century Britain is too brief and begs too many issues.35 We must wait for Professor MacDonagh’s book covering the years 1830–70 which is due to go to press soon.36

Much of the impetus for the growth of government (using that phrase in its broadest sense) came from the activities of what today would be called pressure groups. These, too, have received their share of attention. J. T. Ward has repeated his earlier conclusions about the factory movement, but on a smaller scale and in a clearer fashion. In the same collection of essays C. H. Hume has written a brief survey of the public health movement which mainly serves to emphasize the need for a much broader study (which may well turn out to be by Dr F. B. Smith of the Australian National University who is working in this area).37 Michael Cullen has described the men and motives underlying the interest in social investigations, relating them to pressure for reforms in education and public health.38 Of particular value is a collection of essays edited by Patricia Hollis.39 Hollis’s own introduction provides a stimulating overview of the whole problem of pressure group activity, concentrating on the way in which the role of pressure groups was legitimized within the political system. The other essays cover the anti-slavery movement (before and after 1833), the philosophic radicals, the middle-class political radicals, land reform, the Liberation Society (anti-state church), David Urquhart and the Foreign Affairs Committees, the Administrative Reform Association, plus biographical studies of William Lovett, Shaftesbury, and Edward Baines. The book concludes with Brian Harrison’s discussions of state intervention and moral reform. Harrison has emerged as the leading figure in this field and in a number of works (notably Drink and the Victorians) he has argued that the Victorian preoccupation with moral reform needs
to be integrated with philanthropic and other social reform activities into a wider picture if we are to understand the Victorians properly. Drink and the Victorians itself is a very large and at times difficult book which traces the history of the temperance movement from its beginnings in the anti-spirits campaign, through teetotalism, to prohibition. It is impossible to summarize briefly but two arguments contained within it might be noted: the temperance movement was a factor in gaining acceptance for the notion that government had a role to play in the creation of a humane and decent society; and the movement was another one of those which provided opportunities for cooperation between middle-class and ‘respectable’ working class activists thus gaining for the latter a place within the social and political system.

The political system created by the Reform Acts of 1832 and the motives behind those Acts seemed once to have been completely and finally described by Norman Gash. But the proliferation of historians seeking to distinguish themselves has meant that no orthodoxy can now last for long. Professor D. C. Moore has won a name for himself by examining what he regards as the sociological premises of the Acts and the succeeding political system. In a number of articles he has argued that the Acts were not meant as a (largely forced) concession to the new middle class but as a cure for the ills of the old system. Those ills were caused largely by such factors as urban penetration of counties which led to the individual electorates losing their coherency and legitimacy as deference communities bound together by a common interest (such as agriculture or coalmining). The Acts, Moore argues, were intended to create new and stable deference communities by insulating the counties from the urban elements and separately enfranchising legitimate urban interests. This thesis has been subject to considerable criticism, notably by E. P. Hennock and John Cannon. Much of the argument has centred on the different provisions made in the various Bills (and drafts thereof) concerning the town dweller who qualified for a county but not a borough franchise as well as on the various statements made at the time by politicians. Perhaps three points need to be borne in mind in approaching this debate. Firstly, the Bills and final Acts represent a great deal of political compromise and political social, and economic ignorance. To expect them to proceed from a single social theory is ridiculous. Secondly, the statements of politicians (especially ministers) need to be interpreted in the light of a remark made a few years later by Peel to Gladstone when the latter had to speak in defence of a government measure: ‘be long and diffuse, it is all important in the House of Commons to state your case in many different ways, so as to produce an effect on men of many ways of thinking’. Thirdly, the dichotomy that Moore sees between concession and cure is a false one in some ways. There seems little doubt that men like Lord Grey thought they were making a concession to the middle classes which would cure the diseases in the body politic.

If Gash’s view of the motives underlying the Acts seems to have
survived largely intact (though Cannon has a few choice words to say about some of his more metaphysical statements) the same cannot be entirely said for his view of the structure of politics after 1832. The historian who has most directly attacked Gash in this area is R. W. Davis. Largely basing himself on a study of certain Buckinghamshire electorates Davis argues that after 1832 the power of landlords to determine votes and control members of Parliament was significantly diminished and that the political system was now more responsive to public opinion (at least the opinion of the enfranchised public). What we call deference early Victorians called influence and there was a general agreement that influence had a role as long as it was ‘legitimate’, that is derived from natural ties of association, leadership and dependence (often the great landlord would expect the elector to give one vote for his nominee but left the other to be used freely). But the use of fear and coercion was normally regarded as illegitimate.47

These arguments are developed somewhat further in an interesting study of north-east England by T. J. Nossiter. Nossiter sees three major factors operating in electoral politics. The first was the politics of individualism or principle. But there was also the politics of the market where votes were given in anticipation of or gratitude for some reward. This could involve crude bribery but was more likely to be based on employment, services, or patronage. Finally, there was the politics of influence in which the voter’s frame of reference was his local community and his deferential role within it.48 Nossiter thus updates Gash and places slightly less emphasis on the importance of public opinion than Davis (though the distinctions are all ones of degree, not kind). Closer still to the Gash model is Olney’s study of Lincolnshire. But even in such a heavily rural county as Lincolnshire there were limits to the power of influence which itself was not in any case a one-way process. Those who were influenced tended to ensure that they made gains of some sort thereby.49 In the new urban seats, by and large, the power of influence was particularly weak. D. Fraser (Leeds) and D. G. Wright (Bradford) have shown the importance of political divisions based on religious and social affiliations and the crucial part played by party organization (in terms of voter registration and de-registration) in deciding who won.50

Many of these themes are taken up in the most unusual of the recent works on the political system, J. R. Vincent’s *Pollbooks: How Victorians Voted*.51 Vincent is not the first historian to use the numerous pollbooks which, before 1872, give the votes of each elector (and often the occupation of the elector as well) but he has made more of them than any previous worker. It is perhaps unfortunate that it should be Vincent who did so for, as his earlier book on the formation of the Liberal party had shown, he is perversely determined at times to differ from all previous commentators.52 But two facts do seem to emerge from his analysis. One is that there was a very high degree of party voting in early and mid-Victorian politics (even when the parties at Westminster were in a state of flux). The second
is that the pollbooks do not support any simple social explanation of political behaviour. The relation of occupation to voting was a complex one (Vincent provides his own rather strange interpretation). This may most appropriately be seen as a reflection of a complex social structure which at best is only crudely described by the traditional three class model\textsuperscript{53} and of a great and changing variety of attitudes, opinions, causes, and beliefs. As Patricia Hollis has put it, ‘Victorian political life was engagingly pluralist’.\textsuperscript{54}

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\section*{NOTES}


2 \textit{Victorian Studies} is published by the Program for Victorian Studies, Indiana University.


6 Melbourne, 1967.


12 ibid., p. 349.


16 London, 1974. It does, however, provide a useful introduction to the problems considered for those unfamiliar with them.

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For the debates in Cabinet see John Milton-Smith, 'Earl Grey’s Cabinet and the Objects of Parliamentary Reform', Historical Journal, XV (1972), 55–74.


