1

INTRODUCTION

Among the two most marked characteristics of English historical scholarship are its fixation with the adversary system – its desire to see the past as a debate between two 'sides', and to echo this in the rivalry of scholarly interpretations – and the principle of specialisation, which implicitly shapes so many of the historian's results by confining him to a predetermined chronological field. So great is the volume of new publication that for any century it seems we can only keep up with it by swimming with the current, accepting and working within the framework of explanation which that scholarship contains. Yet it would be useful, and sometimes it seems possible, to stand back from a debate in an attempt to undo the effects of both these imperatives, and to look within a wider perspective at those cross-currents of influence and argument that sway the course of historical scholarship. 'When our ideas on some large historical theme are in a state of disorder', wrote Sir Herbert Butterfield, 'we may find it useful to make ourselves acquainted with the history of the historiography of that particular subject.'[1] Studies of this kind are more commonly undertaken for past generations of historians, as in such deservedly famous works as Sir Herbert's cited below, D. C. Douglas' *English Scholars, 1660–1730* (1939, 1951), J. G. A. Pocock's *The Ancient Constitution and the Feudal Law* (1957), or John Burrow's *A Liberal Descent: Victorian Historians and the English Past* (1981). Here I have sought to pursue these questions in more recent years.

Although the result is much wider both in chronological and thematic range, this book began as an attempt to trace the course of debate over the last decade in two usually quite separate areas: the reigns of the first two Stuarts and the first two Hanoverians. It was conceived as a response to a valuable survey by Professor J. H. Hexter of the impact on scholarship in the former field of the recent writings of Professor Conrad Russell.[2]

[1] Herbert Butterfield, *George III and the Historians* (London, 1957), p. 9.
[2] J. H. Hexter, 'The Early Stuarts and Parliament: Old Hat and the *Nouvelle Vague*', *Parliamentary History* 1 (1982), 181–215.

Hexter's informality and erudition excused his marshalling of his colleagues into schools or parties; his helpful precedent is my excuse for asking similarly irreverent questions of the eighteenth century also.

I have borrowed but adapted some of Hexter's terminology, and should explain my terms. By the 'Old Guard' I mean that cohort of scholars whose minds were formed in the matrix of inter-war Marxism, and their later heirs. The 'Old Hat' school in these pages indicates those Whig or liberal historians, in the tradition of S. R. Gardiner and G. M. Trevelyan, Wallace Notestein and Sir David Lindsay Keir, who rearranged English political history into a benign and teleological pattern – the unfolding of parliamentary liberties, the rule of law, and representative institutions. By the 'Class of '68' I mean those writers whose world view took shape in euphoric approval of the radicalism and unrest of the late 1960s and early 1970s. 'Revisionist' and 'revisionism' are labels I inherit from early-Stuart scholarship in the last decade, and use with more reluctance: each generation revises the views of its predecessor, and soon post-revisionists, and *their* successors, will emphasise the inadequacy of those categories. Yet these are shorthand terms only: little of the argument developed in this book depends on the infallibility of these categories. Nor do I wish to imprison particular individuals within them: my ambition, on the contrary, is to abet their escape.

The generation effect, then, is one preoccupation of my discussion. Another is the effect of those divisions which have grown up within the historical community. This book arose from the belief that the unit of production (to borrow an economists' expression) which sets the boundaries of historical enquiry and influences its content is often larger than the professional historian's avowed 'period' or the range of the student's textbook. In this case, I wish to suggest that the debate between different cohorts of historians, well known in the period 1603–42[3] and little known in the years 1714–60, is part of a wider movement which embraces both centuries. If those debates are to be clearly understood by the student and profitably prosecuted by the researcher, the interdependence of academic arguments in the two areas must be highlighted, and the apparent isolation of neighbouring groups of historians and of students must be broken down. This study is a small contribution to that process.

If these two debates can be related more closely, there may be a consequence for our perception of English history which is both foreseen and intended in this book. The traditional strategy has posited a decisive

[3] Early-Stuart historiography until the mid 1970s may be traced in R. C. Richardson, *The Debate on the English Revolution* (London, 1977); his helpful study was written before the appearance of Professor Russell's more recent work, which is the starting point for the present book.

break at the end of the seventeenth century: 1688 is often taken as the symbolic date. The 'eighteenth century' is consequently coupled to an industrial–democratic engine of change and drawn off into 'the future'. The suggestion here, on the contrary, is that such linkages are very greatly weakened by our new understanding of the nature and chronology of economic growth, and by a proper appreciation of the events of 1828–32. If so, we must explain eighteenth-century English politics and society as a development, not a revolutionary discarding, of their traditional forms in the seventeenth century. Revolution must be located in the realms of politics and ideology, religion and social institutions, and such a redefinition emphasises the large degree of continuity in even these forms. Yet to seek to relate eighteenth-century England to its seventeenth-century origins rather than to its nineteenth-century outcome runs counter to most traditions of English scholarship, and many of the shortcomings of this book will stem from the novelty of the attempt.

Such an enterprise, it is suggested, calls into question a number of historians' long-familiar explanatory categories. Scepticism rather than a new credulity is our aim, however. The elaboration and refinement of definitions is not the business of the historian. We can safely leave it to social scientists to build models of institutions or processes (capitalism, class, party, revolution) and, if they wish, to carry their models back into the past in a search for phenomena which might seem to fit them. The historian should prefer to work more closely with his material and to be more responsive to the content of the categories employed in past time. In contrasting 'revolution' and 'rebellion' in the title of this book I am referring to modern debates: I am not suggesting that those terms carried their present meanings in the seventeenth or eighteenth centuries. Until after 1789 the term 'revolution' indeed often signified a reversion to a previous pattern, as a wheel comes full circle. Clarendon reserved the title 'revolution' for the events of 1660, not 1642; Whigs used the same term of 1688 to signify the repair of the constitution after what they claimed had been James II's innovatory tyranny; and this usage survives in Dr Johnson's *Dictionary*.[4]

Our own meanings are somewhat different. Without wishing to rest much weight on definitions, it might be suggested that we still understand

[4] The various and accumulating meanings of the two words can best be traced in the *Oxford English Dictionary* and its *Supplement*. It is clear that in and before the eighteenth century 'revolution' (apart from its meaning 'a return to origins') was merely used in a sense synonymous with 'successful rebellion', i.e. a 'complete overthrow of the established government'. Not until the nineteenth century did 'revolution' take on the social-structural meanings we now principally give it. Despite the writings of such pioneers as James Harrington (1611–77) or Gregory King (1648–1712), we should not exaggerate the willingness or ability of most seventeenth- and eighteenth-century Englishmen to think about their society in structural terms.

'rebellion' to mean a fundamental challenge to the title to legitimacy of political institutions, often (in the past) a religious title. But this concept now tends to become subsumed in that of 'revolution', since we often take a revolution to be a successful rebellion; and by 'revolution' we now understand, in addition to the political aspects, a fundamental challenge to the legitimacy of social structures, including patterns of hierarchy or stratification, and titles to economic ownership or control. Too much recent writing, it is suggested, rests on this anachronistic sense of 'revolution': the revisionists' preferred category is 'rebellion', and it is one which deserves more exploration.[5]

So to distinguish these two explanatory categories helps us to disengage ourselves from the assumption that revolutions are always 'forward-looking', that they embody the progressive aspirations of 'rising' social classes to speed up developments being impeded by 'the forces of reaction'. Rebellion is a concept more evidently devoid of such implications; it helps our appreciation that many conflicts (like the Civil War or 1688) can better be described as reactions against innovations, a deeply rooted resistance to undesired change. I am not claiming that no revolutions, in the social-structural sense, ever occurred in early-modern societies (though that is a possibility); I am suggesting that we should be far more cautious in applying that category. These modern usages, of course, are not watertight: past events have a way of spilling over into both. The object of any attention to definitions in this book is not to encourage readers to rest content with a new label but to use labels more critically, as a help to understanding the concrete and unique detail of the events themselves.

This study, then, attempts to trace (and, indeed, to reorganise more purposefully) the scholarship of the last decade concerned with England's experience of political and social change over two centuries. It attempts to link both the conceptual formulas and the frameworks of explanation which have been applied in curiously similar ways to those episodes conventionally designated 'revolutions': the Civil War, the events of 1688, of 1776 in America, the 'Industrial Revolution', and the quasi-

[5] For a profound and scholarly non-Marxist enquiry into the status and function of the concept of revolution as applied to early-modern Europe see Perez Zagorin, *Rebels and Rulers, 1500–1660* (2 vols., Cambridge, 1982), vol. 1, pp. 3–57. Yet his broadly drawn definition of 'revolution' (vol. 1, pp. 17, 24) subsumes 'rebellion' and confuses social-structural change with discontinuities in the realms of ideology and religion, distinctions which I wish to emphasise. Surveying Britain, France, Spain, the Netherlands and Germany, Professor Zagorin develops a fourfold typology – 'agrarian rebellion', 'provincial rebellion', 'urban rebellion' and 'revolutionary civil war'. Applying the last category to the 'English Revolution' of 1640–60 (vol. 2, pp. 130–86), Zagorin rejects some of the arguments of the revisionist historians which are discussed and partly accepted in the present book. Although I dissent from several aspects of his thesis, Zagorin's able and wide-ranging comparative study is essential reading on the subject.

revolutionary or silently revolutionary upheaval in which the 1832 Reform Act is related to all of the foregoing and to post-1815 'radicalism'.

Such a study, spanning two centuries, must necessarily be a schematic one, and to a large degree a study of the historiography. But lest it should be thought that I am merely postponing 'modern times' and keeping alive 'the world we have lost' until 1789 or 1832, it should be remembered that the same process of revision and reinterpretation has been achieved by François Furet for that most paradigmatic of all revolutions, the French Revolution of 1789. As John Roberts emphasised,

> Time and again, the accumulating conclusions of the best scholarly research not merely fail to uphold, but actually undermine, the received conceptual framework within which *bienpensant* narrative of the Revolution is written. That pervading conceptual structure is, in broad terms, progressive and left wing. It has often been summed up as Marxist, or at least *marxisant*, and such terms are understandable – though (as M. Furet from time to time turns from his own argument to show) Marx and Engels themselves provide precious little support for it. Essentially, it is a structure suffused with a commemorative or even celebratory approach to the history of the Revolution. It rests on the presentation of the Revolution as a unitary expression of a supra-historical process of world significance. The Revolution is seen by it as the emergence of a liberating force, organically related to all subsequent 'progressive' social 'movements'. Historians of the Revolution have gone over again and again a narrative which illustrates this basic scenario, stressing the rupture with the past and strengthening the illusion of change. Unfortunately, at the same time solid scholarship was making such views increasingly untenable.[6]

If even the French Revolution now defies incorporation in the *marxisant* schema, we need to look afresh, and to look synoptically, at those English episodes that have been explained as the precursors or counterparts of 1789.

[6] John M. Roberts in *History*, 68 (1983), 168–9, reviewing François Furet, *Interpreting the French Revolution* (Cambridge, 1981).

2

A DISCOURSE ON METHOD

PERIODISATION: THE EFFECTS OF DIVISION

Few people are more isolated from each other than near neighbours, and among the most effective barriers to historical research are those unwritten rules which have grown up to inhibit scholars from questioning the received orthodoxy in adjacent areas. The result has been the survival of some curiously obsolete opinions. Many historians of seventeenth-century England still seem to believe that Sir Lewis Namier or J. H. Plumb legitimately reigns over the succeeding century; eighteenth-century historians still too often assume a model of the preceding century drawn from the Marxist Old Guard, with perhaps a glance at the somewhat muted critique of it offered by the Old Hat liberals. Even the best scholars sometimes seem up to a quarter of a century in arrears in their understanding of the course of scholarship in adjoining periods which have been designated 'someone else's'.

This does not mean that they are immune from influence, however. Wider movements in opinion show a remarkable ability to produce curiously similar phenomena in apparently unconnected areas of enquiry, and one such coincidence is the occasion of this survey. It took as its starting point the realisation that two debates had been proceeding together, not quite on parallel lines but with strong affinities and similarities, for a decade or more: the reinterpretations of the parliamentary history of the reigns of the first two Stuarts and of the first two Hanoverians. To a small degree, they stimulated and linked up with scholarship in other decades; but the isolation of different groups of historians gave them each a unity and autonomy. Moreover, those two debates, it will be argued, were essentially the same debate. They have, indeed, so far owed little or nothing to an explicit understanding of each other: Conrad Russell's research on the 1620s and Anthony Fletcher's on the 1640s disclosed no recognition of the importance of the eighteenth-century work of the History

of Parliament Trust,[1] and when I wrote on the 1750s[2] I failed to see the full significance of the work of the early-Stuart historians discussed here – much to my own loss.[3] But related problems had presented themselves in each area: the attempted answers proved to be strikingly similar, and the orthodoxies which came under attack were essentially the same. The two debates could still learn much from each other, and it is the modest desire to link these two areas more closely which prompts this survey rather than any intention to solve – in so short a book as this – the outstanding problems of either.

By enclosing the common field of English history with those fences termed 'centuries', academic entrepreneurs of a bygone generation prescribed a particular type of agriculture as well as a new pattern of ownership. They did so by preventing the emergence of the English ancien regime, 1660–1832, as an area of study in its own right. It was not only that that area was underpopulated; the numerical dominance of scholars in the 1500–1660 and post-1832 fields,[4] and their high academic calibre, had a disastrous effect on the perceived *substance* of the history of 1660–1832. In one Old Guard model, the years 1660–1760 became an unpleasant hangover after the euphoric revolution of the 1640s, a time when promising libertarian developments were forced 'underground' by oligarchic repression. At the other end of the scale, the combination of Industry, Democracy and Liberalism seemed so appropriate to an equally reified nineteenth century that the period after 1760 was reconstructed as a lead-in to those phenomena which reached maturity only in a later age. The whole experience of England's ancien regime was thus made to seem a temporary aberration from trends naturally successful before 1660 and after 1832.

A periodisation in terms of 'centuries', by fragmenting the continuum 1660–1832, emphasised transitions rather than continuities, cutting his-

[1] Namier's research institution, launched in 1951, has produced: Sir Lewis Namier and John Brooke (eds.), *The History of Parliament: The House of Commons 1754–1790* (3 vols., London, 1964); Romney Sedgwick (ed.), *The House of Commons 1715–1754* (2 vols., London, 1970); P. W. Hasler (ed.), *The House of Commons 1558–1603* (3 vols., London, 1981); S. T. Bindoff (ed.), *The House of Commons 1509–1558* (3 vols., London, 1982); B. D. Henning (ed.), *The House of Commons 1660–1690* (3 vols., London, 1983).

[2] Conrad Russell, *Parliaments and English Politics 1621–1629* (Oxford, 1979); Anthony Fletcher, *The Outbreak of the English Civil War* (London, 1981); J. C. D. Clark, *The Dynamics of Change: The Crisis of the 1750s and English Party Systems* (Cambridge, 1982).

[3] Historians have a fair excuse: we need to see each period in the past in its own terms, without papering over the gaps in our knowledge by the devices of hindsight and foresight. But it is also legitimate to use our results to generate a wider perspective, as the historians discussed in this book have done in other writings.

[4] Taking the community of professional historians as a whole, my impressionistic estimate is that for every ten scholars working on the years 1500–1660 there are three for 1660–1832 and thirty for post-1832. And the dividing dates are very sharp: in recent decades only a handful of historians have researched and written on either side of the years 1660 and 1832. Even today, very few do so.

torians off from their neighbours, and this isolation tended to increase as scholars' attention receded from the 'edges' of their subjects. Even quite recently, an outside observer might have been forgiven for supposing that the parliamentary history of 'the nineteenth century' meant mainly Disraelian and Gladstonian manoeuvres in the years between the Second and Third Reform Bills; that of 'the eighteenth century' chiefly meant the conflicts between the accession of George III in 1760 and the crisis of 1782–4; and that of 'the seventeenth century' was largely concerned with the decades immediately preceding the outbreak of the Civil War and with the Interregnum: interest flagged after 1660.

This weighting inevitably had its effect on the substance of the story. Attention was focussed in the seventeenth century on the causes of revolution (historians usually saw their task as the search for 'origins' rather than the balanced depiction of states of existence). Equally, the 'Whig' agenda written by Lord John Russell's generation in the 1830s and 40s for long focussed attention on the apparent stirrings of reform from the 1760s. The years *c.* 1714–60 were *terra incognita*; and in both centuries, new, interesting and dynamic phenomena were rearranged into the pattern of a crescendo. The events of 1642 and 1832 performed a similar role in each scenario.

Nor was there agreement on what 'the eighteenth century' meant: 1700–1800 raised too many problems, and the general answer was 1714–1815; yet the period after 1789 was usually given only perfunctory attention[5] and tacitly abandoned to nineteenth-century historians, reaching back to depict the origins of their world. If 'the eighteenth century' meant, in practice, 1714–89 or even 1760–84, it is less surprising that students concluded that little of importance can have transpired in a period which allowed itself so pusillanimously to be truncated. The vast majority of students, both at school and college level, studied 'Tudors and Stuarts' or 'moderns', and this neglect was by far the most important reason for the amiable tranquillity of eighteenth-century studies compared with the din of academic battle in the seventeenth century and the nineteenth. In particular, the early Hanoverian era was desperately underpopulated: if the units of study were 'the English Revolution [of the 1640s]' and 'Industrial Revolution to the present', then 1714–60 was marginal to everyone's interests.

Only if the unit of study were the English ancien regime, 1660–1832, did the reigns of the first two Georges stand out as conceptually *central* to the viability of the social and political order which prevailed between the Restoration and the Reform Bill. Conversely, the rewriting of the parlia-

[5] An honourable recent exception is Ian R. Christie, *Wars and Revolutions: Britain 1760–1815* (London, 1982), which deals in detail with *c.* 1789–1815.

mentary history of the reigns of George I and George II – an enterprise begun as scholarship for scholarship's sake – led to results far more extensive than its initiators ever expected. It led, first, to a fundamental challenge to a ruling orthodoxy; and, second, to a realisation that the challenge posed was the same challenge that was being made in the early seventeenth century also. This book began therefore as an account of a war on two fronts: a large and widely reported battle in a heavily populated area of scholarship, 1603–42, and a small and initially neglected engagement in a thinly populated area, 1714–60. Yet if there was a great imbalance in the forces involved, in strategic terms these two theatres of war proved to be of equal importance.

REVISIONISM VERSUS ORTHODOXY

When, at last, even the sluggish waters of eighteenth-century scholarship were ruffled by revisionist breezes, the form taken by the arguments in that century had much in common with the structure of the revisionist case in the seventeenth. Partly this was due to the survival in both centuries of an 'Old Hat' liberal account of beneficent constitutional evolution, but the more important reason was the similarity of the models the Old Guard had constructed in each period, and to which authors heavily influenced by 1930s Marxism (if only to the extent of a shared economic reductionism) had given expression.

By the early 1970s, this combined orthodoxy had hardened to such an extent that a certain asperity, even aggressiveness, was found necessary by its critics in both centuries. Many senior scholars evidently felt fragile in 'the new, idol-smashing atmosphere of research on early Stuart politics'.[6] Revisionists in that era, as in the later one, were explicit in announcing that they aimed to revise 'conventional views' and 'received opinions', rejecting 'the present orthodox answer' and 'the traditional interpretation' in favour of a wholly new account of the structure and close texture of parliamentary politics.[7]

There seems no doubt that the Stuart establishment, whether Marxist Old Guard or liberal Old Hat, reacted to such temerity with an indignation matched only by the Hanoverian establishment in their reaction to similar suggestions in the later field. Professor Hexter, in a remarkable peroration, seemed to suggest that the revisionist interpretation of early-Stuart Parliaments was not merely an 'intellectual defect' but 'a moral one' because it

[6] The phrase of Theodore K. Rabb, 'Revisionism Revised: The Role of the Commons', *P&P* 92 (1981), 55.
[7] Cf. J. H. Hexter, 'Power Struggle, Parliament, and Liberty in Early Stuart England', *JMH* 50 (1978), 4–5.

did not attend to 'the central importance of liberty and the rule of law in the ordering of human affairs'.[8] If, as Professor Hirst suggested, the *major* concern of revisionist historians has been 'to deny the existence of conflict over issues of principle',[9] it seemed a short step to regarding such historical arguments as themselves *unprincipled*.

Since (as is argued in chapter 3) this orthodox model took a similar form in both the Stuart and Hanoverian periods, economic-reductionist and teleological in respect of political change, it prompted its critics also to adopt similar methodological weapons. The revisionists' 'rallying cry is the declaration that hindsight is dangerous', objected Professor Hirst: it led to the heretical view that 'the causes of the breakdown which led to civil war were essentially short-term', to a renewed attention to the role of chance, accident and disaster, and to a focus on day-to-day manoeuvre for explanations of these things rather than on grand rhetorical announcements of principle lifted out of their specific tactical context and strung together as a spurious chain of causality. Detailed studies of precisely defined tracts of parliamentary history were all very well, warned Hirst, yet 'historians who ignore what comes before and afterwards can fall victim to a myopia as damaging as that suffered by the most teleological Whigs'.[10]

Myopia, teleology and anachronism were the key charges bandied about in all revisionist debates;[11] but they were deliberate practices, not thoughtless slips. What critics denounced as sins, believers defended as self-evident truths. 'Great events do not necessarily have great causes, though it is natural for historians to seek them', noted Anthony Fletcher, explaining the rationale of his detailed and rigorous account of the events of 1641–2. Such care was necessary in the elucidation of this crisis, since

> misunderstanding is of its essence. Men's actual intentions must be distinguished from their assumed and alleged intentions. The political debate was conducted in emotional and often highly dramatized terms ... When war came there were some who were able to articulate the principles and convictions for which they fought. Yet even they, let alone the many who found it impossible to attach themselves steadfastly to one side or the other, had not sought war. This was a war that nobody

[8] *Ibid.*, pp. 48–50.
[9] Derek Hirst, 'Revisionism Revised: The Place of Principle', *P&P* 92 (1981), 79.
[10] Hirst, 'The Place of Principle', pp. 79–80.
[11] Equally, they can be found in analogous debates over the French Revolution, as in François Furet's critique of 'the old tautological proof that deduces causes from results'. Sometimes the method is concealed beneath a category: 'the dominant "concept" of today's historiography of the Revolution, "bourgeois revolution", seems to me to be used less as a concept than as a mask concealing precisely these two presuppositions: that of the inevitability of the event and that of a radical break in time'. Furet, *Interpreting the French Revolution* (Cambridge, 1981), pp. 19, 102.