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Review Article

Coping with Revisionism in Early Stuart History*

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The faint-hearted have never found a safe refuge in the study of early modern England. Acrimonious debate and furious rebuttals rather than polite applause and ready acceptance often greeted the first histories of early Stuart England, and the passage of time has done little to diminish the capacity of this material to set normally sedate scholars by the ears. It was, for example, a Victorian Liberal's analysis of the English Reformation that led a High Churchman like Edward Freeman to express the hope, "May I live to disembowel James Anthony Froude."¹ Admittedly the reputation for academic bloodletting among early modernists has been exaggerated. Nevertheless, the fact remains that the often prolonged periods of quiet amiability have regularly been punctuated by controversies of remarkable length and vehemence. It is worth remembering that R. H. Tawney's celebrated rule that "an erring colleague is not an Amalakite to be smitten hip and thigh" was more of a pious wish than an accurate description of his colleagues' behavior during a discussion of the economic fortunes of the gentry.² While these periodic rows have traditionally attracted a wide audience, the attraction was due as much, if not more, to the uncompromising standards of the debate as to the sheer spectacle. Few other occasions call together so many established stars and talented tyros, and no others make them so readily dispense

* The books under review are Mark Gould, *Revolutions in the Development of Capitalism: The Coming of the English Revolution* (Berkeley and Los Angeles: University of California Press, 1987), pp. xxvii + 508; Michael B. Young, *Servility and Service: The Life and Work of Sir John Coke*, Royal Historical Society Studies in History Series, no. 45 (Woodbridge, Suffolk: Boydell Press, for the Royal Historical Society, 1986), pp. xii + 297; Esther Cope, *Politics without Parliaments* (London: Allen & Unwin, 1987), pp. xiii + 252; J. C. D. Clark, *Rebellion and Revolution: State and Society in England in the Seventeenth and Eighteenth Centuries* (Cambridge: Cambridge University Press, 1986), pp. x + 182; Mark A. Kishlansky, *Parliamentary Selection: Social and Political Choice in Early Modern England* (Cambridge: Cambridge University Press, 1986), pp. xiii + 258; Ann Hughes, *Politics, Society, and Civil War in Warwickshire, 1620-1660* (Cambridge: Cambridge University Press, 1987), pp. xvi + 392; Johann Sommerville, *Politics and Ideology in England, 1603-1640* (London: Longman, 1986), pp. x + 254; and Richard Cust, *The Forced Loan and English Politics, 1626-1628* (Oxford: Oxford University Press, 1987), pp. x + 358.

¹ J. P. Kenyon, *The History Men: The Historical Profession since the Renaissance* (Pittsburgh, 1983), p. 118.

² R. H. Tawney, "The Rise of the Gentry: A Postscript," *Economic Historical Review*, 2d ser., 7 (1954): 97.

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with their gloves; deference hardly seems in order when the future course of an entire field hangs in the balance. Consequently those who maintain that spirited arguments make for better history feel quite at home among scholars of early modern England.

Given this background, the news of yet another historiographical fracas is neither surprising nor inconsequential. This time, in the apt phrase of one of the chief protagonists, the point at issue is nothing less than "the traditional blood-sport of English historians, the origins of the civil war." In the journals the signs of an outbreak of hostilities are unmistakable; Lawrence Stone's curt dismissal of his opponents as "antiquarian empiricists" simply brought on taunts about the inability of the Whiggish "Old Guard" to "answer evidence with evidence." By 1983 the situation had become so uncertain that a reviewer only half-facetiously proposed that since the entire field was "a minefield," those publishing in it "deserve awards for gallantry."³ This unsettling situation dates from the mid-1970s when a group of scholars began an intensive examination of the Whiggish interpretation of early Stuart politics. The initial results were startling. Confrontation, which Whigs had seen as a dominant factor of early Stuart political life, was in fact something contemporaries abhorred; consequently consensus, not conflict, was the ruling political paradigm. Conflict of course occasionally occurred, but it was due to bitter factional rivalries at Court and in the shires, not to any profound ideological divide. After all, the ease and frequency with which the "oppositionists," so celebrated in the Whiggish canon, moved into royal service suggests that their articulation of antigovernment rhetoric was largely an exercise in self-promotion. Such cynical bids for advancement were only to be expected when the vision of most Parliament men and their constituents scarcely extended beyond the county border; these were men intent on fleeing from all contact with the central government, not on taking it over. In terms of visceral impact, however, nothing in the revisionist reinterpretation could compare with the new description of Parliament; what had long served as the centerpiece in the Whiggish celebration of liberty and freedom was in fact pronounced to be a weak, impotent body saved from extinction in the late 1630s only by the timely efforts of some Scots well off the Westminster stage.⁴

³ Conrad Russell, *Parliaments and English Politics, 1621–1629* (Oxford, 1979), p. 4; Lawrence Stone, "The Revival of Narrative," *Past and Present*, no. 85 (1979), p. 20; Kevin Sharpe, "An Unwanted Civil War?" *New York Review of Books* (December 3, 1982), p. 45; and Christopher Haigh, review of *Puritanism and Theatre*, by Margot Heinemann, *English Historical Review* 98 (1983): 194.

⁴ For the work of the initial revisionists, see Conrad Russell, "Parliamentary History in Perspective, 1603–1629," *History* 61 (1976): 1–27, and *Parliaments and English Politics, 1621–1629*; Kevin Sharpe, "Parliamentary History, 1603–1629: In or Out of Perspective," in *Faction and Parliament* (Oxford, 1978), pp. 1–42; John Morrill, *The Revolt of the Provinces* (London, 1976); and Mark Kishlansky, "The Emergence of Adversary Politics in the Long Parliament," *Journal of Modern History* 49 (1977): 617–40, "The Army and the Levellers: The Roads to Putney," *Historical Journal* 22 (1979): 795–824, "Consensus Politics and the Structure of the Debate at Putney," *Journal of British Studies* 20 (1981): 50–69, and *The Rise of the New Modern Army* (Cambridge, 1979). For the best survey of the debate, see Howard Tomlinson,

Given the magnitude of this challenge, champions were understandably quick to emerge in defense of the old orthodoxy. Unfortunately the early counterattacks were often more noticeable for their élan than for their effectiveness.⁵ Their difficulties stemmed from the revisionist insistence on selecting terrain for the conflict that highlighted the weakness of their opponents. The hallmark of the new school was its reliance on the close contextual study of events and its exultation in sources, the more arcane the better. It naturally followed that only criticism directed at the evidence could possibly be relevant. Anything else, from furious theoretical assaults to skillful recapitulations of the Whiggish interpretation, was beneath comment; indeed they only served to prove that the old theory was a Victorian anachronism based on a flawed reading of a few documents, and printed ones at that. It must be conceded that the merits of the early Whiggish paladins, considerable though they were, did not include the ability to cite chapter and verse from obscure manuscripts scattered across county record offices and buried in the recesses of the British Library. Consequently the revisionist insistence on just the facts has resulted in the repeated postponement of the long-awaited battle royal over revisionism; a generation of scholars has been too busy in the archives.

In the meanwhile, although the debate itself had moved in a curious direction, the compulsion to publish has remained as primordial as it has ever been, and a number of talented scholars have faced the unenviable task of coping with a major controversy that had hung fire. Of all the possible responses, the most obvious is to ignore the question. Admittedly some footnotes in Mark Gould's *Revolutions in the Development of Capitalism* acknowledge the historiographical conflict, but his text is oblivious to the furor. In a far-reaching work examining "the genesis of revolution within . . . the seventeenth century social structure," such an aloof attitude is eminently understandable (p. xiii). In all areas of academe the enthusiasm for interdisciplinary studies remains high, especially for ideal matchups like history and sociology. Hence Gould, who had laudably attempted such a synthesis in early Stuart history, can with some justice regard the current debate as inconsequential. The same logic compels historians to read his book with considerable care and attention.

Regrettably, they are likely to come away disappointed and certain to be confused. Fair warning is given in the introductory remark that "some readers may find it difficult reading," and any belief in the author's unnecessary modesty vanishes after learning that the book can be read in three different sequences (p. xiv). Some of the roughest sailing comes in the opening chapters when Gould presents a structural analysis of the "manufacturing social formation" of early

"The Causes of War: A Historiographical Survey," in *Before the Civil War*, ed. H. Tomlinson (London, 1983), pp. 7–26.

⁵ For the notable interventions, see Theodore K. Rabb, "Revisionism Revised: The Role of the Commons," pp. 55–78; Derek Hirst, "Revisionism Revised: The Place of Principle," pp. 79–99; and Christopher Hill, "Parliament and People in Early Seventeenth Century England," pp. 100–124, all in *Past and Present*, no. 92 (1981). See also J. H. Hexter, "Power Struggle, Parliament and Liberty in Early Stuart England," *Journal of Modern History* 50 (1978): 1–50, and "The Early Stuarts and Parliament: Old Hat and the Nouvelle Vague," *Parliamentary History* 1 (1982): 181–215.

seventeenth-century England. Unfortunately only those initiated into the mysteries of systems analysis will be able to make heads or tails of much of this; the less adept historians will follow Gould's advice and proceed to the more straightforward historical chapters. Yet here too a thick sociological jargon obscures the argument. The reader can only clutch the list of abbreviations in order to sort out the difference between concepts like LBi, "legitimizing belief for integrative disorder," and LBg, "legitimizing belief for political disorder." And even this knowledge fails to explain the importance of the numerous charts and graphs. If Gould's fondness for "theoretical argot" does not exceed the tolerance of most historians, then his subdivision of the 1640s into REVOF, REVOG, REVON, and REVOV certainly will (p. 231). In short, Gould should be applauded for undertaking such an ambitious book, and he may well have much to say. All the more reason to lament that he and his publisher did not take greater pains in rendering his ideas into clearer English.

Admittedly, underneath this verbiage there are numerous names that historians will immediately recognize—their own. To his credit Gould has diligently surveyed the secondary authorities, but this mastery was achieved only at the price of almost totally excluding primary sources. This practice, which may well be acceptable in the social sciences, trips off methodological warning bells among historians. It is all the more alarming since his citations of secondary authorities often lump together improbable bedfellows. Thus Wallace Notestein and Conrad Russell are called on to support a proposition about "the Country's ability to seize the initiative against the king, [and] to formulate programs seeking to redress grievances"; yet in fact Russell has explicitly denied this point (p. 210). Gould's general insensitivity to far more subtle historiographical nuances reveals itself more disturbingly in his general thesis that the 1640s witnessed a bourgeois revolution; while it is of course possible to make such an argument, Gould can scarcely be said to advance it when he effectively ignores the sharp criticism of this mode that has led to its near total disappearance from recent history. Sad to say the major accomplishment of the book is to illustrate that barriers between sociology and history remain frustratingly high.

The tactic of largely ignoring historiographical turmoil need not produce irrelevant reading. Michael Young has admirably proved this fact in his biography of Sir John Coke, Charles I's long-suffering Secretary of State. As his earlier articles testify, Young is far from shy about intervening in debates, but in *Servility and Service* he has rightly stuck close to his subject.⁶ For all the general popularity of biographies, the bleak fact of the matter is that the comparative rarity of an extensive collection of personal papers has limited the usefulness of this genre in early modern English history. Secretary Coke, however, is one of the major exceptions; through his papers we can follow several decades of administrative and factional politics at Whitehall in extraordinary detail. Indeed, for much of the

⁶ Michael Young, "Illusions of Grandeur and Reform at the Jacobean Court: Cranfield and the Ordinance," *Historical Journal* 22 (1979): 53–73, "The Origins of the Petition of Right Further Reconsidered," *Historical Journal* 27 (1984): 449–52, and "Buckingham, War and Parliament: Revisionism Gone Too Far," *Parliamentary History* 4 (1985): 45–69.

1620s and 1630s the Coke Manuscripts represent a large segment of the State Paper Domestic series, which until Young's book we were never fully aware we were missing. Given the importance of these materials, the publication of *Servility and Service* is especially timely, for after lying for over three centuries in Coke's Derbyshire country house, inaccessible to all but the most dogged scholars, the Coke Manuscripts have recently been deposited in the British Library. Young consequently has provided an invaluable entrée to a newly opened manuscript collection of fundamental importance to early seventeenth-century scholars.

Young has also done much to expand our understanding of the period. One of Russell's most arresting observations stressed the fact that many of Buckingham's supporters in the 1620s were not stalwart royalists in the 1640s; Young at once amply confirms and explains this phenomenon.⁷ Coke's budding administrative career ended almost before it had begun in 1604, when the Howards and Sir Robert Cecil bested Coke's patron, Sir Fulke Greville. Afterward Coke had fourteen years to meditate on the nature of factional politics. The time for retribution eventually came when Greville and Coke latched onto the coattails of George Villiers in his meteoric rise at Court. A brisk succession of appointments ended in 1626 with Coke as Secretary of State and effectively in charge of a wartime navy. Such was his ability to move paper that the return of peace in 1630 could not dislodge him; by that time, his industry had earned him a secure place in the Caroline regime and even the admiration of "Thorough" ministers like Laud and Strafford. Eventually in 1640 old age and court rivals forced the septuagenarian into retirement. Young's study reveals much about the motivation and functioning of early Stuart factions. Coke had entered the Court in the company of Greville and Robert Naunton, both of whom supported the Elizabethan model of domestic and particularly parliamentary harmony and of unceasing war against Spain and Rome. These sentiments not only illuminate Buckingham's ideological taste in clients, but they also render quite comprehensible Coke's willingness to pursue an accommodation with the Commons in the late 1620s when many of his colleagues had already given up hope. Not surprisingly he ended his career alarmed by the pro-Catholic and antiparliamentary aspects of the "Personal Rule"; thus Charles's faithful bureaucrat rebuked his son for failing to register a public vote *against* Strafford and prayed in 1643 "for the prosperity of the Parliament, wherein consisteth the welfare of this church and state" (p. 270). After all, there were few people better able to tell what would follow a royal victory than Charles's former Secretary of State. Young deserves high praise for illuminating some of the murkier areas of the early Stuart regime as well as for producing one of the finer biographies of early modern Englishmen.

Another conventional means of maneuvering through troubled historiographical waters is to acknowledge the dispute and cautiously to steer between the two extremes. Esther Cope has presented a striking example of this tactic in *Politics without Parliament*, a study of the domestic response to the pivotal but poorly understood period of Charles I's "personal rule" in the 1630s. On one hand, Cope maintains that "England was not on a high road to revolution or civil war

⁷ Russell, *Parliaments and English Politics, 1621–1629*, pp. 435–36.

during the 1630s'' (p. 220). Charles's dissolution of the turbulent 1629 session effectively lowered the realm's political temperature, and only at the end of the following decade did the Scottish rebellion bring matters back to a rolling boil. Such issues as there were in the 1630s, furthermore, were not primarily constitutional. This line of argument would raise few revisionist eyebrows. Yet at the same time, Cope also stresses that the Personal Rule was far from an idyllic period of harmony. The decade-long hiatus in parliamentary sessions, however much it lowered domestic tension, did not resign the populace to prerogative rule. Barred from their traditional forum in the Parliament house, critics of royal policies simply adopted more circumspect modes of expression. Thus Cope is careful not to allow "compliance with commands" in controversial matters like ship money and Laudian church reforms to overshadow "grumbling and resentment," which "can fester so that a regime's stability can be more apparent than real" (p. 118). In the end, however, the careful assessment of both sides produces an unexpectedly weak conclusion; neither school, "neither localism or constitutionalism," adequately explains what was happening in the 1630s (p. 215).

Such a hesitant conclusion is inherent in the policy of weaving between one school and another. Unfortunately this failing is only symptomatic of the book's difficulties. Ironically, the depth of Cope's research hobbles *Politics without Parliament*. Although the book is intended as a preliminary survey based on selected archival gleanings, only the truly exceptional sentence ends without its own, often detailed, footnote. One irresistible conclusion emerges from reading such a comparatively brief work into which so much material has been packed. Her argument would have been much more forceful, and the text more interesting, if she had been able to deploy her evidence to better advantage. Terse references to libels against Laud and popular verses on Parliament men in 1640 are no substitute for a more ample discussion of these matters (pp. 143 and 186). A more crippling problem is methodological. It is perfectly valid to focus on the reactions of those outside the Court to events at Whitehall, but it becomes a decidedly awkward brief when there has been no thorough study of the policies themselves, either their development or their implementation in the intense factional struggles of the Caroline court. Consequently *Politics without Parliaments* at times resembles a detailed account of reactions to events that are themselves distressingly vague. Such an imbalance also means that it is all too easy to slip into a highly polarized view in which the ties between Court and country are obscured in favor of emphasizing their conflicts. These problems notwithstanding, Esther Cope deserves high commendations for a daring reconnaissance into an important period of history that has been strangely neglected. Future scholars may well find themselves qualifying her work, but they will all have to acknowledge their debt to her pioneering efforts.

Such a scrupulously correct approach to the controversy is far from the only course of action. It is also possible to accept the initial revisionist position as the obvious point of departure for further reinterpretations of the old Whiggish canon; both Mark Kishlansky and Jonathan Clark have demonstrated the merits of this tactic. As befits a charter member of the revisionist group, Kishlansky in *Parliamentary Selection* is consistently provocative in his "social history" of

seventeenth-century elections. The course of the century, he argues, witnessed a sharp shift from social “selection” to a more openly political election; a system based on consensus and obsessed with the maintenance of community order ultimately gave way in the 1640s to another glorying in direct confrontation and driven by propaganda, religion, and patronage. The results of this transformation were as striking as the shift itself, for it engendered nothing less than modern “participatory democracy” (p. 230).

Thanks to crisp prose and a clear argument, Kishlansky’s study makes compelling reading. Perhaps the greatest discovery in *Parliamentary Selection* is the deftness of Kishlansky’s pen. As his predecessors had found, elections naturally make lively reading; his numerous vignettes ably continue that tradition and are alone worth the price of the book. Certainly few of his readers are likely to forget Sir “Timber” Temple and the 1685 Buckingham election. Furthermore, Kishlansky’s dexterous ability to straddle the great divide of 1660, a feat that few other scholars have attempted, only enhances the attraction of *Parliamentary Selection*. Students of the early Stuart century will find his report on the second half of the century fascinating, but they will naturally be most interested in his analysis of the first half. Kishlansky has “a surprising sight” in store for them (p. 230). The dramatic contested elections before the outbreak of the Civil War that have bulked so large in earlier studies scarcely appear at all, aside from a detailed case study of the 1614 Somersetshire election. Instead Kishlansky has rescued from historical oblivion the uncontested “selections,” which were the result of meetings at the hustings on the majority of occasions. He has skillfully reconstructed the contemporary concern with honor, reputation, and public order, all of which made the thought of a contested election and of a division within the community anathema. Kishlansky’s greatest achievement arguably lies precisely in his ability to recover this political world in which a happy outcome necessarily meant an uncontested election.

At bottom in the revisionist temperament lies a desire to make scholars think again—and deeply—about matters that have hitherto seemed all too clear-cut, and by this standard Kishlansky has scored a brilliant success. The fact remains, however, that while he has moved the question of election and selection into the historiographical spotlight, he has not, at least as yet, clinched the argument. In spite of an eloquent plea to accord equal time to “the resiliency of political values and practices and the transforming power of radical ideas and methods,” the former is much more evident than the latter in his discussion of early Stuart England (p. 106). The earlier Whiggish imbalance in the other direction doubtless accounts for his emphasis on “selections.” Nonetheless his indifference to almost all the celebrated, even if uncharacteristic, contested elections before 1640 is baffling. After all, without a careful examination of these exceptions to the rule, he cannot effectively dismiss contested elections as mistakes, fostered by faulty communications and originating in “squabbles over rates, markets and outlivers” rather than “any burgeoning political consciousness” (pp. 31–32; see also pp. 16, 65, 74, and 109). As the introduction makes clear, this problem stemmed from his reluctance to correct colleagues, especially deceased ones like Sir John Neale. In a field celebrated for its displays of ill temper, such polite restraint is

exemplary. On the other hand, a frontal assault on an issue of fundamental importance like elections is perhaps not the best place to administer an object lesson in good manners. Thus while Kishlansky pursues earlier scholars of elections through the footnotes, he signally fails to close with them in any overall assessment in the text itself, and as a result his overall argument remains only a tantalizing one. He has done enough, however, to ensure that future scholars will attend to *Parliamentary Selection* with considerable care.

Kishlansky's approach to the revisionist furor is far from unique. To date, Conrad Russell has declined to respond to his critics, but after J. H. Hexter in 1982 subjected revisionism to a characteristically vigorous pounding for the second time in four years, a champion for the new contenders emerged from an unlikely corner of eighteenth-century history.⁸ Having proven that he can write ponderous tomes with the best scholars, Jonathan Clark's goal in *Rebellion and Revolution* is a brisk and amusing, albeit partisan, interpretation of the recent quarrels of early modern English historians.⁹ He may not persuade too many of the unconverted, but he will certainly prompt his readers to ponder larger questions that transcend narrow fields of research. It is also comforting to learn that Hexter's unbuttoned style of discourse is not lacking for adept disciples on the other side of the Atlantic.

Clark's intervention is solid evidence of the rapidity and enthusiasm with which some in adjacent fields have subscribed to the revisionist creed, and the purpose of *Rebellion and Revolution* is to provide a non-Whiggish sketch of seventeenth- and eighteenth-century historiography. Since Clark is probably correct that most scholars operate on dated views of adjacent fields, students of the early seventeenth century will appreciate his gloss on the recent disputes among their colleagues in late seventeenth- and eighteenth-century history. They will not, however, be apt to learn much new about their own disputes. Nevertheless it is certainly curious to ponder his main argument that the quarrel over revisionism is essentially a quarrel between generations; the revisionist iconoclasts of the late 1970s, he maintains, succeeded in uniting hitherto antithetical "Old Hat" liberals of Hexter's ilk with the Marxist "Old Guard" of Christopher Hill and Lawrence Stone. Although exceptions constantly come to mind, there is something in his suggestion, which later scholars of twentieth-century intellectuals will doubtless develop further. In the meantime what does emerge from his generational analysis with dazzling clarity is the modern political agenda that at least some scholars have read into revisionism. The distribution of Clark's censure, for example, can scarcely be termed even-handed; while he can be gracious toward Hexter, he has almost no time for Hill, Stone, and Brewer. Little wonder then that Clark's delight is scarcely hidden with the prospect that "the fate which then [in the early twentieth century] overtook liberalism is now overtaking socialism" (p. 20).

⁸ Hexter, "Power Struggle, Parliament and Liberty in Early Stuart England," and "The Early Stuarts and Parliament: Old Hat and the Nouvelle Vague."

⁹ J. C. D. Clark, *The Dynamics of Change: The Crisis of the 1750s and English Party Systems* (Cambridge, 1982), and *English Society, 1688–1832: Ideology, Social Structure and Political Practice during the Ancien Régime* (Cambridge, 1985).

Those alarmed by Clark's enthusiasm for parallels can find some comfort in the uneasiness that some revisionists doubtless feel over being linked to the Oxford Movement and Cardinal Newman, whose Eighteen Theses on Liberalism Clark has thoughtfully reprinted (pp. 102–3 and 172–73).

Rebellion and Revolution also illustrates the danger of writing “a contribution to a living debate” (p. ix). The obvious assumption behind his wide-ranging discussion is that the major historiographical landmarks that the revisionists recently have erected would stand as monolithically as the old Whiggish totems had. The failure of the initial neo-Whig counterattacks to land any telling blows likely accounted for his confidence, as did his generational argument that the young had abandoned the history as well as the politics of their elders. Unfortunately, had he waited only a year or two, he may well have been less wedded to the belief that the future belongs to revisionism. Without question, the shock waves of the late seventies have left behind radically different concerns. No one soon is blithely going to disregard the revisionist challenge; but it is just as evident that the revisionist methods, in particular the insistence on a firm contextual understanding and the close study of documents, could ultimately be used to qualify, if not to subvert, the new prescription.

One of the shakiest parts of the revisionist case is its reliance on the “localist” school. Russell's *Parliaments and English Politics* had scarcely come off the presses before Clive Hughes and Ann Hughes lodged searching objections to the entire “provincial” model.¹⁰ The effect of these telling opening salvos can be seen in *Rebellion and Revolution* where Clark attempted to untangle revisionism from excessive dependence on the localist model (p. 57). Anxiety on this account was in order, for Ann Hughes's initial criticisms have been greatly amplified in *Politics, Society, and Civil War in Warwickshire, 1620–1660*. Even worse news for her opponents is the fact that her conclusions are defended with an extraordinary display of detailed manuscript research; casual challenges are unlikely to a work that carefully announces that an important document, once in the wooden chest by the dining door of a local manor house, has migrated to the chest by the drawing room window (p. 127). Like others of the postrevisionist generation, Hughes has learned the importance of an impeccable set of sources. *Politics, Society, and Civil War* is all the more exciting because she knows what to do with this mass of information. With considerable skill Hughes repeatedly maneuvers around the many traps for the unwary scholar. Hence, unlike some of her predecessors, she never forgets that her brief is to uncover why the Civil War occurred; after noting that most of the elite sought neutrality, Hughes emphasizes that “what finally remains surprising is not that a majority preferred peace to war . . . but that so many, albeit a minority, were prepared to take up arms for what they believed in; and that several, albeit a smaller minority, were, in a hierarchical, deferential society,

¹⁰ Clive Holmes, “The County Community in Stuart Historiography,” *Journal of British History* 19 (1980): 54–73; and Ann Hughes, “Militancy and Localism: Warwickshire Politics and Westminster Politics,” *Transactions of the Royal Historical Society*, 5th ser., 31 (1981): 51–68.

prepared to fight the supreme authority in the land” (p. 167). In such able hands there is little danger of the trees obscuring the forest.

One of the book’s major attractions is its systematic analysis of the influential “localist” thesis of the “county community,” which Alan Everitt first advanced over twenty years ago in a study of Kent.¹¹ Her conclusion is arresting: there was indeed a county community of the Warwickshire gentry, but it only emerged in the 1650s. Before that date, what may well describe the situation in Kent is, at best, of limited usefulness in a Midland county like Warwickshire. The political boundaries of the shire contained two distinct regions with different economies and social structures as well as a major anomaly, the “county” of Coventry. Not surprisingly, members of such a fractured county community appear to have regarded the county line as little more than an artificial delineation on a map. It logically follows, then, that “Warwickshire, or part of it, was a specific part of England, not a unit independent of or opposed to the nation state” (p. 44). Furthermore, Warwickshire politics before the Civil War were never the story of the struggle between the Lord Lieutenant, the local agent of a centralizing Crown, and a bench of intensely parochial Justices of the Peace; in fact, both worked together efficiently and generally balanced the demands of the county and the country. Political crises therefore split the county community down the middle. Consequently the Civil War was not thrust on the unwilling shire in 1642; rather, it logically developed from the power struggle of two magnates, one with close ties to Court and the other a dissident “oppositionist” peer.

Equally provocative is her detailed analysis of the impact of the Civil War. Although most recent interpretations have downplayed the role of class tension, she points out that Parliament triumphed in Warwickshire thanks to Lord Brooke’s ability to mobilize the “lower orders” against the bulk of their social superiors. After Brooke’s death in 1643, a committee of godly men who were “unfamiliar and suspect” to the gentry ruled the county for the next two decades (p. 179). This development in turn was largely responsible for the belated emergence of a “county community” as the gentry eventually closed ranks against the upstarts. Her analysis, which repeatedly illustrates the limitations of many common assumptions about the Interregnum, underscores the need for further detailed work in this area. Little evidence supports the charges either that the Warwickshire committeemen abused their power for personal gain or that in the 1650s Whitehall tightened its grip on the provinces; in fact, after 1653, there was no phalanx of soldiers to cow the local populace. Admittedly the JPs in the 1640s and 1650s were eager to promote a reformation of manners, but they were no more zealous than their prewar predecessors. Indeed, when dealing with bastardy, the Cromwellian magistrates, while still stern with the crime, appear to have been more sympathetic to the often abused mothers (pp. 288–89). Much of Hughes’s most penetrating work concerns her discussion of the county’s religious politics. Thanks to an unenthusiastic bishop and a “godly” resident peer, Laudianism

¹¹ Alan Everitt, *The Community of Kent and the Great Rebellion* (Leicester, 1966), and *The Local Community and the Great Rebellion* (London, 1969).

made little headway, and only a minority of “prayer-book” Anglicans existed before the 1640s. Yet having won the initial struggle against Arminianism, the godly soon found themselves cast in the awkward role of repressive conservatives when faced with Ranters and Quakers. The bitterest of defeats came after 1660 when many of this religious elite were purged and others after much soul-searching opened the Book of Common Prayer, which they had closed sixteen years earlier. Hence anyone who wants to understand the tumult of the mid-seventeenth century can do no better than to read *Politics, Society, and Civil War* with great care.

The difficulties the revisionist thesis has suddenly encountered do not stem simply from a few cantankerous scholars of provincial England; an even more compromising challenge has recently arisen to the entire notion of consensus, the veritable keystone of the new interpretation. Plainly an attack from this quarter was unexpected; otherwise Clark would likely have been less vocal in his plea for the closer integration of “parliamentary history and the history of political ideas” (p. 106). As it is, his call could have done admirable service on the dustjacket of Johann Sommerville’s *Politics and Ideology in England, 1603–1640*. Perhaps the most striking first impression of this work is the authority and facility with which Sommerville makes his case. The successful unraveling of this complex question requires a mastery of recondite sources and a willingness (all too uncommon in the late twentieth century) to wade through lengthy Latin treatises on law and theology. Sommerville nonetheless wears his erudition with ease, and a topic that could have been buried underneath opaque prose has found an exceptionally lucid, and indeed often witty, exposition.

The many merits of *Ideology and Politics* are apt to be lost, at least initially, on revisionists. With the emergence of the revisionist emphasis on the inherent political harmony of early Stuart England, the old Whiggish concern with the long-standing tension between “divine right” monarchs and the devotees of the “ancient constitution” has become decidedly passé. Sommerville demonstrates, however, that this tension cannot casually be dismissed as the product of overheated Whiggish imaginations. Rhetoric about the necessity of consensus did indeed come readily to many contemporaries, but Sommerville stresses that such language celebrated a pious wish rather than reality. The fact of the matter was that well before the British problem erupted in the late 1630s, distinct ideological divisions had already rent the political nation; “there was no unity on the questions of the nature and limitations of royal authority, the relationship between the law and the king, and the role of Parliament in church affairs” (p. 4). The first half of the study is essentially a gloss on this text. Contemporaries argued about the merits of “at least” three distinct theories (p. 108). Out of the crisis over the Elizabethan church emerged the notion that “the Queen . . . was bound by no purely human laws” (p. 12), and her Stuart successors further encouraged the elaboration of this absolutist rhetoric. Whitehall, moreover, had no monopoly on ideological innovation; English adherents of “contract theory” deployed a heady brew of medieval conciliarists, Jesuit theologians, and Calvinist divines in support of their case for a sovereign checked by the laws of the land and ultimately by the threat of deposition. Others, especially among the common lawyers,

opposed the development of absolutism on thoroughly English grounds; any expansion of royal powers could only come at the expense of Sir John Fortescue's celebrated notion of England's "regimen politicum et regale" (p. 88). With mouthpieces for the "ancient constitution" like Sir Edward Coke, the advocates of "divine right" never had to worry about the lack of a good argument.

It could be argued that this ideological debate had no real effect on the course of political events. However, this argument can only be made because earlier scholars were either reluctant or unable to place these ideologies in their political context. In the last half of *Politics and Ideology* Sommerville begins the task of correcting this omission with a telling sketch of the political impact of these rival ideologies. While revisionists accord primacy to the disruptive role of faction, parochialism, and cynical careerism, Sommerville illustrates how these factors were linked to, and often dominated by, ideological concerns. All students of the period would do well to consider carefully his observation that "if we distinguish too rigidly between ideas and interests we are in danger of missing the point that interests themselves are shaped by ideas" (p. 232). To be sure, there was no inevitable "high road to civil war" between 1603 and 1640. Yet this fact cannot justly be employed to deny the importance of a series of incidents in these years which regularly focused a generation's attention on the fundamental liberties of Englishmen. While other scholars are busy developing new and ever more immediate causes of the Civil War, Sommerville boldly bucks this trend; at least ideologically, "the war did have long-term origins" (p. 5). After following his analysis of crises over Cowell's *Interpreter*, the 1621 Protestation, Sibthorp and Maynwaring, the Petition of Right, and Laud's revolution within the Church, it is hard to reject this conclusion out of hand. Although his examination of these events is cursory rather than definitive, he has advanced more than enough evidence to prevent the wholesale Namierization of early Stuart politics.

Quite simply, *Ideology and Politics* is a striking work. All those interested in the field must become well acquainted with Sommerville's book, which represents the most significant single challenge to the revisionist thesis. The mere fact that this study has finally superseded Margaret Judson's old workhorse of 1949, *The Crisis of the Constitution*, simply reveals the magnitude of Sommerville's achievement. It will as well likely have an extraordinary shelf life; long after the current furor over Parliament dies down, undergraduates and their teachers will still read it as the most convenient point of departure into the neglected ideological dimension of early seventeenth-century England.

Given the revisionist insistence on context, it is possible to argue that Sommerville's terse one hundred pages got it all wrong. In *The Forced Loan and English Politics, 1626–1628*, Richard Cust reveals the futility of such a response. At first glance the vital importance of Cust's study is not readily apparent. When compared with Kishlansky, Clark, and Sommerville, whose eyes rarely wander from the current controversy, Cust initially seems low-key; without any fanfare he simply analyzes the forced loan project and the domestic reaction to it. Yet the modest packaging of *The Forced Loan and English Politics* should not deceive readers. Any resolution to dismiss his work quickly cannot survive a perusal of the manuscript bibliography. A *select* list draws on documents in almost forty local

record offices and country houses across the realm. In addition his grasp of the extensive records of the central government, and especially the awkward financial documents, is comprehensive. What Cust has to say, therefore, is nothing if not authoritative.

It is also fascinating. The effect of reading Cust immediately after *Ideology and Politics* is almost electric, for many of the points that Sommerville discussed in general find particular illustration in Cust's detailed analysis of the forced loan project, the first major attempt to collect extraparliamentary taxation. Although the judges had after considerable controversy approved the levy, many contemporaries regarded it as a direct violation of a series of English laws stretching back to the Magna Carta, and the 1628 Parliament eventually forced Charles to agree not to repeat the project. The royal attempt to bypass parliamentary control of extraordinary taxation thus introduced a generation of taxpayers to the ideological questions that Sommerville has sketched out. Such is Cust's skill that we can follow the response of the political nation from privy councillors to obscure freeholders and tenants.

Revisionists have suggested that the entire incident has been far too overblown. Far from attempting to dispense with Parliament, Charles vowed that the loan was simply an emergency measure and would produce more, not fewer, parliaments. Such objections as there were seem to have been focused on ratings disputes rather than constitutional issues. Given this response the levy understandably was a financial success, raising a quarter of a million pounds or almost as much as five subsidies in 1628 were to bring in. After Cust's study, however, such an interpretation becomes quite dubious. The decision in 1626 to pursue "new counsels" of extraparliamentary taxation, Cust emphasizes, sharply divided Charles's councillors into "hardliners" and "moderates." This debate over policy furthermore was conducted in language that might have been lifted directly from *Ideology and Politics*. The hard-liners were openly antithetical to Parliament, the capstone of the "ancient constitution," and enthusiastic about "divine right" theory; some went so far as to describe the Magna Carta as "a chain to bind the King from doing anything and a key to admit the vassal to everything," while others characterized the opponents of the loan as "King-haters" (pp. 216 and 21). Moderates on the other hand stressed the benefits of clinging to the "ancient constitution" rather than embarking on "somewhat of a new world" of unfettered prerogative rule (p. 71). Charles I himself resolved the dispute by committing his full authority to the controversial project; thus "hardly anything could have been more calculated to alarm the sensibilities of ordinary Englishmen and upset the delicate balance of faith and trust on which the constitution rested" (p. 89). The political nation mirrored the tensions within the council; while some local governors saw in sedulous collection of the loan a means of currying royal favor, others openly protested its legality; for their scruples, some found themselves imprisoned and even purged from the bench. So disconcerting was the initial opposition that the government soon found it wise to make financial concessions to local communities. One of Cust's more startling findings is the stark fact that only 184,000 out of the 267,000 pounds, less than 70 percent of the total, found its way into the Exchequer (p. 92); hence what has been billed a victory for an

aggressive royal government in face of a compliant populace becomes instead a rather hollow triumph of creative bookkeeping.

Cust's study also affords a splendid opportunity to examine how this debate about principles affected the lower orders. In Westminster some five hundred subsidymen greeted the first mention of the loan with cries of "a Parliament, a Parliament," and since others, particularly in the Midlands, echoed this sentiment, several magnates were apprehensive about the spread of "the Northampton and Warwickshire infection" into their own counties (pp. 96 and 121). Admittedly the combination of stern punishments for refusers and generous concessions to the obedient eventually forced the majority into compliance. Nonetheless even the most skillful application of the carrot and the stick failed to persuade a significant minority. In some cases, the lower orders followed the clear lead of the prominent local gentleman; this much could support the case of the political naïveté of the nonelite, especially in the countryside. But Cust has found other, more ominous, correlations to recusancy. In some cases, godly ministers and their congregations stood out against the loan in defiance of local superiors. In others, the inhabitants of wood-pasture districts were apt to be more difficult than their neighbors in more hierarchical open field areas; hence the broad distinctions between "chalk" and "cheese," which loom so large in David Underdown's analysis of political allegiance in the 1640s, has some relevance in the 1620s.¹² These points allow Cust to advance an arresting conclusion: the reaction of the lower orders to the loan provides "an important corrective to those who might assume that ordinary English men and women were unconcerned at the broad issues of contemporary politics or incapable of action to affect these" (p. 306). In short, *The Forced Loan and English Politics* is a classic example of how an exhaustive study of an event can cast a penetrating new light on an entire period.

For all of their merits, Hughes, Sommerville, and Cust have not put paid to the revisionist interpretation; Kishlansky and Clark, to name only two, are plainly not going to allow the match to go by default. But these books collectively indicate that the long-delayed resolution of the furor is at last imminent. And as the advocates of revisionism prepare to respond to this belated challenge, they can derive a good deal of satisfaction from the knowledge that, whatever the ultimate outcome, they have at the very least forced a most distinguished body of scholars to abandon the pursuit of grand overarching theories and instead to ponder the facts. If nothing else, the frontal challenge to the Whiggish canon has forced scholars to examine their basic premises and their evidence very closely. It has also spawned a generation of scholars who now are able to lodge objections that revisionists have to take seriously; given the primacy that revisionists accord evidence, they cannot ignore Hughes, Sommerville, and Cust as cavalierly as they did earlier objections. Consequently, once the polemical dust has settled, there will be few who doubt that the study of early Stuart England has not profited from extended contemplation over how to cope with revisionism.

¹² David Underdown, *Riot, Revel and Rebellion: Popular Politics and Culture in England, 1603–1660* (Oxford, 1985).