John A. Phillips as a Historian*

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The late John Phillips broke new ground both in terms of what he had to say and the methods he adopted. A 'revisionist' in the truest sense, over a period of some 20 years he was enthusiastically engaged in a number of leading (and often heated) debates on both sides of the Atlantic. His contributions to discussions about the nature of the English political system during the 'long' eighteenth century and the impact of the 1832 Reform Act will already be familiar to many readers of Parliamentary History. His work on electoral politics, in particular, opened up challenging new perspectives on the traditional themes of change or continuity, the full implications of which have far from run their course. What is probably less widely known is the way in which Phillips was also embroiled in debates about historical methodology, most notably concerning the use (and misuse) of quantitative techniques in historical analysis, but also more latterly, as a staunch defender of traditional empirical history against what he regarded as the excesses of postmodernism and the so-called 'linguistic turn'. Both types of contribution were often intermingled in his historical writings and, as befits such a talented interdisciplinarian, his scholarship is best appreciated when both are considered in tandem.

Phillips' first book, Electoral Behavior in Unreformed England. Plumpers, Splitters and Straights (1982), added significantly to a growing body of literature challenging long-established interpretations of eighteenth-century politics, not least those put forward by Sir Lewis Namier and his acolytes.1 Building on his 1976 Iowa doctorate and subsequent articles, Phillips joined the likes of John Brewer, Richard Davis, Donald Ginter and Frank O'Gorman in portraying a political system under George III in which parties, ideology, and popular participation genuinely mattered.2 (Namier, of course, had dismissed all three as irrelevant and explained national political developments almost entirely in terms of the personal ambitions of the aristocratic elite.) At the core of his work, in what fast became his trademark approach, lay a detailed analysis of voting behaviour as recorded in poll books, supported by lively anecdotes and descriptions from contemporary sources. Specific attention was paid to the four relatively large towns of Lewes, Maidstone, Northampton and Norwich at eight general elections between 1761 and 1802, but

* Full details of his publications will be found in the bibliography on pp. xiii-xvi.


material was also introduced from Bristol, Chester, Coventry, Liverpool, Newcastle, Nottingham, Reading and Northumberland.

His conclusions dispelled any lingering doubts. The extent of corruption and control in the electoral system had been vastly exaggerated. Patronage and ‘deference voting’ were rarely sufficient to win both seats (most constituencies at this time elected two M.P.s). The voters themselves were not only more representative of the adult male population than had previously been thought, but also more likely to turn out in high numbers. Above all, political issues – whether local or national or a quirky blend of both – were far more significant determinant of voting habits than had hitherto been allowed. Electors, in other words, were politically principled, even if their political leaders were not. Large numbers of those who had signed petitions respecting the John Wilkes episode of 1769, the American colonial crisis of 1775-6, or the Fox coalition of 1784, for example, had adopted remarkably consistent political stances in subsequent elections. Driving his point home, Phillips noted a substantial increase in partisanship within his four boroughs and speculated that over half of the borough electorate may have become similarly politicized by the close of the eighteenth century.

The political conflicts he described, however, were far removed from the class-based Marxian ones made so fashionable by E. P. Thompson’s *The Making of the English Working Class* (1963). In an important challenge to another major school of historical writing, Phillips found little evidence that occupation or socio-economic profile influenced voting behaviour. The world of the sociologists and historians of the left, with their belief in an emerging class consciousness driven by industrialization and the French revolution, was barely recognizable in his constituencies. In Norwich, for example, the same occupational groups repeatedly voted for the Whigs in the Northern ward and for the Tories in the Conisford ward during the 1790s. Using one innovative statistical measure after another, Phillips was unable to find any significant correlation between socio-economic status and political affiliation. Religion, on the other hand, could explain a great deal. Norwich’s Whig northern ward was dominated by dissenters; its Tory Conisford ward by Anglicans. A similar link between denomination and party was evident in Northampton and, to a lesser extent, Maidstone. Boroughs with active dissenting interests also had far more contested elections than those that were uniformly Anglican. It was the rapid spread of dissent, Phillips suggested, that provided the key to the rapid politicization of the urban electorate. Late eighteenth-century popular politics, he conjectured, was in essence ‘religiously-colored’ and only gave way to ‘class-dominated’ politics at a much later date.

Comments by reviewers included ‘brilliant’, ‘outstanding’, and ‘one of the most significant works since those of Sir Lewis Namier’. But there was also some criticism, of course. In particular, concerns were raised about how typical his four

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boroughs were. Phillips was scrupulous in his attention to detail and keenly aware of local idiosyncrasy, yet his more generalised speculations appeared to throw all caution to the wind. What is telling, however, is how many of his ‘hunches’ about the electorate as a whole were in due course corroborated. Subsequent studies, notably O’Gorman’s *Voters, Patrons, and Parties* (1989), confirmed the broad thrust of most of his assertions. Many of his passing comments showed equal foresight. His remarks about the neglected role of municipal politics in shaping partisanship, for example, were amply borne out in later work. Phillips could be impetuous, but his instincts as a historian were spot on.

One key element missing at the time was an appreciation of the scale of Phillips’ technical achievements. Most reviews inevitably focused on his conclusions rather than his methods. The late 1970s, it is worth remembering, was a pre-PC world, in which all but the most simple data processing required highly specialised knowledge and costly equipment. To assist him in his analysis of over 14,000 voters in his four boroughs, Phillips had enlisted the programming skills of Roy Weaver at the University of London Computer Centre, then one of the best (and fastest) facilities available. Building on the pioneering studies of William A. Speck, whose computer analysis of voting had produced important findings for Queen Anne’s reign, they constructed a nominal-record-linkage database, in which information about individual electors was matched up to allow their voting habits to be followed from one election to another. The matching up process alone represented something of a milestone. Numerous problems associated with similar names, inconsistent spellings, voter mobility, and different ways of listing occupations and addresses – to name but a few – were overcome, and all without having recourse to the behaviour at the poll itself to clarify the voter’s identity. Once the data had been linked, a primitive sort of search engine or ‘query language’ was then used to examine the statistical relationships between the different types of behaviour on offer. These could range from simple comparisons of party support to complex strings of questions. How many party supporters at two successive elections had previously cast one of their two votes for the opposite party, in a ‘split-vote’? Were whig supporters at three successive elections more likely to maintain their partisanship than their Tory equivalents? The permutations that could be explored were, quite literally, almost endless. But they all shared one common characteristic. This was the ability to examine and compare the cohesion of voting trends from an individual-level perspective over time.

The methodological differences between such ‘longitudinal’ assessments and conventional ‘static’ measures, such the level of split-voting in successive elections, could not have been more fundamental as far as Phillips was concerned.

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Reviewers of *Electoral Behavior* quite correctly pointed out that in some of the boroughs he had considered, the level of partisan voting actually appeared to decline. In Maidstone, for example, the level of split voting was 23.4 per cent in 1796 and 59.1 per cent in 1802. Surely this was incompatible with an argument for increased partisanship? Well, not if Phillips’ methods are considered more fully. Take a situation in which an elector votes for the whigs at one election and the tories at another. In a conventional ‘static’ assessment of partisanship, the voter obviously would be included in the percentage of partisans at the first election. But he would also count among the partisans at the next poll, even though he had changed sides. Trends in the level of partisan voting in this ‘static’ type of analysis include even the most fickle and inconsistent voters. ‘Static’ snapshots of partisanship and split-voting ignore ‘floating voters’, who keep on switching parties, and alterations taking place in the consistency of voting behaviour. As Phillips himself later put it, ‘what individual voters did across elections is not evident from these figures. Even worse, increased political coherence at single elections may well have been unrelated to political behavior over the longer term.’

It was in the study of long-term trends, especially, that Phillips’ approach opened up a new dimension on the late eighteenth-century electorate. In the case of Maidstone, for example, he identified a decisive shift away from ‘floating votes’ and their replacement by more fixed forms of partisanship among a hard-core of voters at successive elections, including the two mentioned above.

Of course Phillips was perfectly aware of how these two different approaches could throw up seemingly mixed results, and with typical diligence continued to utilise both methods in all his subsequent work. In later studies, however, the sophistication of his ‘longitudinal’ approach advanced considerably. One important development was his ability to combine nominal-record-linkage with random sampling techniques. This had at first proved too much for his computers, but in a paper with the rather alarming title ‘Achieving a Critical Mass While Avoiding an Explosion’, he outlined a novel solution which was later used to great effect. Armed with new methods and greater computing power, and with the assistance of Charles Wetherell at the Laboratory for Historical Research, University of California, Phillips began to extend his inquiries into the Reform Act era. Amongst other things, this enabled him to confront head on D. C. Moore’s compelling theory about ‘deference voting’ and the extent to which the 1832 Reform Act had increased, and was intended to increase, aristocratic control over the electorate.

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9 See, for example, *Albion*, XV (1983), 245-47.
At the same time, he was able to probe far more deeply into late eighteenth-century popular politics by examining the hitherto neglected role of municipal elections. His timing could not have been better. J. C. D. Clark's *English Society* (1985), with its 'revisionist' portrayal of the eighteenth century as a theocratic 'ancien régime', had re-ignited the whole debate about the extent of popular involvement in English politics. Clark had dismissed recent 'participatory models' of the electorate as flawed and, sounding much like the Namierites, had argued that popular politics
was insignificant before 1832. With typical resolve Phillips responded with what he (privately) termed, ‘a little Jonathan Clark bashing’. His groundbreaking ‘longitudinal’ analysis of Maidstone’s parliamentary and municipal electors revealed the fundamental importance of political principles not only in national elections, but also in local council contests during the last quarter of the eighteenth century.

These twin themes of reform and municipal politics were to dominate Phillips’ output in the 1990s. The need for more intensive research had become apparent both from his own initial forays and the publication in 1989 of O’Gorman’s widely acclaimed study of the unreformed electorate, which had raised significant doubts about the status of the electoral reforms of the 1830s. In his second book, *The Great Reform Bill in the Boroughs. English Electoral Behaviour 1818-1841* (1992), Phillips adopted the obvious (but as yet untried) approach of comparing voting behaviour in the decades immediately before and after 1832. His methods were by now familiar – traditional, frequently lively, historical narrative combined with quantitative analysis – but the scale of this project, involving some 25,000 voters in eight boroughs at nine general elections, as one reviewer noted, ‘in advance of anything before attempted’. Never one to be historiographically straight-jacketed, Phillips seemed reluctant to plump for either of the conventional ‘change’ or ‘continuity’ arguments long associated with the debate about 1832, to the evident frustration of some reviewers and historians since. Demonstrating typical respect for idiosyncrasy, he insisted that the Reform Act had played out differently in different boroughs. In Lewes and Great Yarmouth there was little change. Here reform occurred in a milieu which was already highly politicized and participatory. The cooler political climates of Shrewsbury and Northampton, on the other hand, were transformed, but not so much by the act itself as by the popular agitation surrounding its tortuous passage through parliament. In Bristol, Colchester and Maidstone, meanwhile, strong local traditions of partisanship became far more nationally-oriented and likely to be repeated in successive contests. Only Beverley remained non-partisan, but not for long. In an important harbinger of future research, Phillips traced Beverley’s ‘political awakening’ not to the Reform Act itself, but to its often overlooked corollary, the Municipal Corporations Act of 1835.

There is no doubt that these variations in the scale and pace of politicization between 1818 and 1841 complicated Phillips’ overall claim that the ‘Great’ Reform Act ultimately did matter and had ‘justified its epithet by altering

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England’s political environment profoundly. A less scrupulous historian might have played them down in favour of a cleaner (and punchier) argument. But that was not his way. Instead, Phillips set about extending his analysis backwards into the mid eighteenth-century and forwards into the mid nineteenth in order to provide the ‘broad-brush’ kind of perspective on 1832 that was now clearly needed and his reviewers called for. The resulting lead article in the American Historical Review (with follow-up commentary) unequivocally affirmed the ‘critical, indeed watershed, role of the Great Reform Act’. It was (and remains) the largest ‘longitudinal’ assessment yet undertaken, examining the political behaviour of almost 40,000 voters in 13 boroughs between 1760 and 1868. And as such it made abundantly clear the key methodological differences between ‘longitudinal’ and ‘static’ studies, such as the analysis of split-voting used by the political scientist Gary Cox. What these and other ‘static’ measures failed to reveal, Phillips demonstrated, was the extent to which Reform had eradicated haphazard forms of political behaviour and replaced them with long-term party attachments. A decisive shift to inter-election partisan loyalty occurred after 1832, which he argued marked ‘the death of localized politics and the birth of modern party voting’.

Taking another cue from his second book, Phillips also began to inquire more closely into the effects of municipal reform. This was an area that had barely been touched on, at least from a political standpoint, since the early twentieth century. In two seminal articles Phillips applied his ‘longitudinal’ investigations of voting to the new town council elections introduced in 1835. Using previously overlooked municipal poll books, Phillips unearthed clear evidence that the politicizing effects of reform had not been limited to the parliamentary electorate alone. Against all expectations, and in spite of an extremely confusing voting system, the new municipal voters also began to adopt the same kind of long-term party loyalties. Even more surprising, those that shared both franchises overwhelmingly carried over their party loyalties from one type of election to the other, apparently approaching their local political choices in essentially national party terms. In the second article, which appeared posthumously, Phillips astutely observed that these developments had been neither intended nor foreseen by the

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19 Phillips, Reform Bill in the Boroughs, p. 300.
whig government that had introduced the new system. In his closing remarks he posed a typically tantalizing question: 'How could this almost entirely unintended consequence of the Municipal Corporations Act have happened so quickly? A major part of the answer lies in the nature of the voting process itself at the council elections, but that complicated issue must be left for another venue.'

Sadly, it is the current volume that now provides the venue for this intended sequel, which was in the final stages of preparation at the time of his tragic death. His article has been re-arranged and updated to take account of subsequent publications, some of which he himself generously helped to initiate, and was the most complete chapter of a draft book discovered among his papers entitled *Crushing the Close Corporations. The Politics of Municipal Reform*. Like his previous monographs, this combined the findings of earlier articles with new material. Of particular note in this respect was an investigation of socio-economic variations within the new municipal wards and, in a departure from his usual constituency focus, a lively account of the Municipal Reform Bill's stormy passage through the house of lords, in which the apocalyptic forebodings of the tory opposition were set against the backdrop of popular unease at the appearance of Halley's comet. Many of his conclusions about the importance of municipal reform and his broader 'hunches' about the politicization of local administration have, of course, now been confirmed and extended by more recent studies of the 1830s. But some questions still remain, not least concerning the extent to which the developments of the reform period survived in later decades. Phillips himself was acutely aware of this problem and in the conclusion to his second book had noted that on the evidence of Great Yarmouth's 1848 by-election, at least, the changes of the 1830s seemed to have been rather short-lived. Not for the first time, however, he did what few historians are inclined to do and later alerted his readers to the fact that he had been 'wrong' about this. It may well turn out to be the case that the new forms of political behaviour he discovered survived and prospered where they might have been least expected to - in the still largely uncharted territory of mid-Victorian municipal elections. One thing, at least, is certain. In the study of this key component of English popular politics, which he almost single-handedly rescued from neglect, his influence will continue to be seen clearly for many years to come.

There are, of course, many other aspects to his legacy besides electoral politics. All his work managed to combine well-crafted prose with innovative quantitative techniques, often so effortlessly that his technical achievements were hidden from view. Few of his admirers will need reminding of his description of the Bristol reform riots or his gambit of opening a piece with a pertinent anecdote about an

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individual voter or politician. Phillips clearly worked harder than most to achieve a fine balance between traditional historical narrative and quantitative social-scientific history, or QUASSH as it is called by its proponents. Leading by example, he was a firm advocate of the need for higher standards of mathematical literacy in the historical profession and could always be relied upon to spot any sloppy statistics when reviewing. But at the same time he was no apologist for historical analyses at the other extreme, based on purely abstract statistical modelling and theorizing. With a foot firmly in both camps, as it were, he was uniquely placed to edit *Computing Parliamentary History, George III to Victoria* (1994), a pioneering collection of essays demonstrating how some of the latest computing techniques were being used to re-examine important events in late eighteenth- and nineteenth-century British politics. The lengthy debate surrounding quantitative methodologies in history, he argued in his introductory survey, was now effectively over. The sophistication of QUASSH had been so transformed by recent improvements in computing that research was now possible in areas that only a few years ago had seemed unthinkable. And as computing continued to advance so too would QUASSH’s potential. As such, Phillips believed that it provided a much needed counter-weight to some of the more extreme influences of deconstruction and post-modernism then sweeping the humanities. ‘While some entire academic disciplines appear to be foundering in an impossibly heavy sea of relativistic neologizing and word play being passed off as scholarship’, he declared, ‘QUASSH might be seen as just more evidence of the nearly universal respect among historians for the solid, usually archival, evidence that provides the firm foundation for good historical scholarship.’

As one of a handful of historians for whom the ‘science of history’ was truly a practical reality, Phillips clearly found the anti-empiricism of the new cultural history difficult to stomach. Yet he remained sensitive to the fact that when stripped of its post-modernist gloss, many an example of this genre revealed a solid archival base. Those who resisted the temptation to succumb to the latest fads sweeping the profession, however, earned his praise, along with a prediction that their work would prove more durable as a result. There can be no doubt that his own meticulous scholarship will stand the test of time for precisely the same reasons.

Others in this volume have written about Phillips’ skills as a teacher and his unflagging generosity, something I personally experienced when he agreed to fly over from the U.S.A. at his own expense to examine my Oxford doctorate. Even more impressive, however, was his subsequent generosity with ideas and genuine enthusiasm for work so potentially invasive of his own. His example in this respect is another important part of his legacy. There is, of course, now a new wind blowing across the Atlantic, notably from the east coast, which seeks to rehabilitate the role of traditional empirical approaches in the humanities. John would doubtless have approved.


30 See, for example, his reviews in *Albion*, XXV (1993), 712-14 and *American Historical Review*, CI (1996), 1206-7.