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The future of Election Studies: Mid-Life Crisis or New youth?

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ABSTRACT

In a number of countries Michigan style national election studies have now been conducted for around 40 years or more. Given their relative inattention to context and difficulties in disentangling cause and effect it might be considered time for radical change to or even abandonment of such studies. However the longevity of the studies coupled with a growing programme of comparative research means that it is only now beginning to be possible to address some key questions about the impact of context on voting behaviour. What is required in order to take advantage of this newly emerging opportunity is a change to the way we analyse national election studies rather than in their design.

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National election studies have reached middle age. The first of the pioneer Michigan US studies was undertaken in 1952, the Swedish series began in 1956 while Norway was not far behind with its first study in 1957. Studies in Britain and Germany will soon be celebrating their 40th birthday (Mochmann et al, 1998; Thomassen, 1994a). So this is clearly an appropriate moment to evaluate their legacy. Are election studies approaching a mid-life crisis? Or are they perhaps not quite so old as they seem?

Michigan style national election studies have one important distinguishing characteristic. They endeavour to understand elections and electoral behaviour by interviewing as close to polling day as possible a nationally representative sample of the population eligible to vote. Elections are examined primarily through the prism of voters. What does and does not influence voters is ascertained by comparing the behaviour and attitudes of one group of voters at a particular election with that of another group at the same election. It is assumed that if we can understand what accounts for differences between voters at an election we can understand what accounts for the outcome and key characteristics of elections as a whole.

In practice, of course, voters are far from being the only actors in an election. Parties and candidates issue appeals and make attempts to get their supporters to the polls. The media report what parties and candidates say and may also offer their own interpretations. And events, be they the state of the economy, foreign wars, or a simple gaffe, may take on an importance that no one can control. The behaviour and attitudes of voters are of course the end product of these influences, but whether we can adequately understand the electoral process by looking at the end product alone is at least open to question. This was certainly the view of the authors of the Columbia local studies that were in vogue before the Michigan model became the dominant approach in survey based electoral research (Benney et al 1956; Berelson et al 1954; Lazarsfeld et al, 1948; Milne and MacKenzie, 1954; Milne and MacKenzie, 1958; Valen and Katz, 1964).

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The content of the media, what parties and candidates did, and indeed how voters interacted with each other socially and politically were all commonly of at least as much interest to these authors as were the eventual reactions and behaviour of voters.

But even if we leave these considerations aside, there is good reason to question exactly how powerful the Michigan approach is. At the heart of any research enterprise is a wish to be able to say something about causation. We may, for example, wish to take a social psychological approach and ask whether a low level or absence of party identification means that voters are more likely to switch their votes (Campbell et al., 1960; Campbell et al., 1966). Or we may be concerned about the rationality of the electorate and enquire whether voters vote for the party that is closest to them on the issues that they consider important (Downs, 1957; Key, 1966). Such questions have been at the heart of scholarly debate about the electoral process for the last fifty years. Yet all too often in Michigan-style survey research we are left wondering whether a correlation between party identification and volatility does show that party identification influences vote or whether in fact it indicates that vote influences party identification (Butler and Stokes, 1974; Crewe et al, 1977; Thomassen, 1976). Equally we often find it difficult to tell whether voters do in fact vote for the party that is closest to them on the issues that matter to them or whether in truth they are simply inclined to think that the party they have decided to vote for anyway must be closest to them on the issues (Miller, 1976; Morris, 1995).

There are then apparently two important limitations to Michigan style national election studies. They pay too little attention to the context in which vote is cast. And even when they do find groups of voters that behave differently from one another, they can still find it difficult to disentangle cause and effect. Little wonder then that some might suppose national election studies to be approaching a mid-life crisis. After fifty years of apparently ascertaining association rather than causation, the law of diminishing returns might well be thought to have set in for what, within political science at least, is often seen as a relatively expensive research resource.

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Of course we have exaggerated the case. Michigan style national election studies have attempted to rise to the challenges of studying context and of ascertaining cause and effect. Perhaps the clearest example of the former is the use of the so-called 'rolling thunder' design whereby a small representative sample of voters is interviewed on each day of the election campaign. Coupled with analysis of the content of the media and of the speeches and actions of politicians this design enables us to examine whether the aggregate distribution of party support changes in response to media output and campaign events. At the same time, by bringing together all of the interviews undertaken during the campaign, we also still have access to a large national representative sample. This rolling thunder design has perhaps been used to particular effect in Canada (Bartels, 1988; Johnston et al, 1992).

Meanwhile, Michigan style election studies have never been entirely without the means to unravel causation. Here the most valuable tool is time. If an attitude is measured at time t and a behaviour at time $t+1$ then while the attitude is evidently a potential influence on the behaviour, the opposite cannot be the case. And many national election studies have introduced time into their designs either by deploying a so-called pre-post design in which the same group of voters is interviewed both before and after polling day, and/or an inter-election panel design in which voters interviewed at the last election are contacted again at the next one (Curtice and Semetko, 1994; Miller and Shanks, 1996). While potentially subject to problems of conditioning and attrition, these panel designs also have the advantage that they reduce our reliance on respondents' memory of past attitudes or behaviour, thereby giving us, for example, more reliable estimates of the volatility of voting behaviour (Himmelweit et al, 1978).

Even so, these variations on the Michigan approach still have important limitations (see also Johnston and Brady, this issue). True, there is considerable value in knowing that voters' attitudes or behaviour can be changed by campaign events or the messages of the media. We may learn,

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for example, that the election campaign is not simply a ritual (Butler, 1952). However it may well be that what matters most in an election campaign is not the variation in the content of the messages received by the electorate from one day to the next but rather what is said consistently during an election campaign as a whole. After all, parties will usually decide what their policy stance is for a particular election and then stick with it for the whole of that election campaign. If so, the content of the campaign may be a constant rather than a variable, making its impact difficult to analyse. True, attempts are sometimes made to get around this problem by, for example, looking for differences between those who say they have followed the campaign or the media closely and those who say they have not (Norris et al, 1999). But these attempts always suffer from the problem that if the media or campaigns have truly been influential then their message will have reached not only those who followed the election closely but also percolated through to those who did not.

However, while parties may consistently deliver the same message at any one election, it is not uncommon for them to change their policy stance from one election to the next (Budge et al, 1987; Klingemann et al 1994) . So while there may be little variance in the messages transmitted to voters within a campaign, we can certainly anticipate variance in their content between campaigns. This implies that if we wish to understand the impact of campaigns we should often be comparing across elections rather than dissecting what happens within individual campaigns.

It is not that users of national election studies have failed to look at changes between elections. Yet what is striking is the linear and predominantly apolitical nature of the main intellectual debates that have been addressed by such work. The dominant concept has been that of dealignment. This is the suggestion that, as a result of changes in the nature of society (such as rising education levels, rising affluence, and the advent of television), the bonds between parties and voters are in long-term secular decline: the bonds referred to are either social psychological (partisan dealignment) and/or sociological (class dealignment) (Clark and Lipset, 1991; Crewe,

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1984; Dalton, 1996; Dalton et al, 1984; Evans, 1999; Franklin et al, 1992; Miller and Shanks, 1996; Nieuwbeerta, 1995; Wattenberg, 1996). In other words, long-term change in electoral behaviour is regarded as the product of long-term gradual secular social change without reference to potentially fluctuating political circumstances.

But if we believe that campaigns matter then a very different approach would seem in order. Perhaps the relationship between social background and vote depends not so much on social change as on the kind of election campaign that is fought. We might, for example, expect there to be a stronger relationship between social background and vote if the parties put forward very different policy programmes from each other than if they all propose very similar programmes (Evans, 1999; Kitschelt, 1994). There is certainly some evidence to suggest that from 1970 onwards at least the strength of the relationship between class and vote in Britain has fluctuated in line with the varying policy distance between the Conservative and Labour parties (Evans et al, 1999). So rather than simply looking for linear trends over time we should regard elections as independent events whose political context needs to be measured and impact evaluated.

However it may well be the case that some important influences on voting behaviour in a country do not in fact change much from one election to the next. One obvious candidate in this category is a country's constitutional structure and electoral system. Yet while these rarely change from one election to the next, there is every reason to believe that they have an important influence on the way that people vote. We might, for example, expect the traits of individual candidates to matter more to voters in a presidential system than in a parliamentary one (McAllister, 1996). We might expect the incidence and character of strategic voting to be different under single member plurality from what it is under a party list system with a significant de factor or de jure threshold (Blais and Massicotte, 1996). And we might anticipate that prospective voting is more common under a proportional system than a majoritarian one (Schumpeter, 1976). Yet unless the same country uses a different electoral system for different elections it is difficult to assess the

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validity of such propositions through single country analysis. Rather we need to compare what happens across countries.

But to do this of course requires that the same survey questions be asked in a similar manner in different countries. In other words, it is not sufficient that election studies are national in scope; they need also to incorporate an international dimension (Granberg and Holmberg, 1988; Thomassen, 1994b). Yet until recently there was no forum in which national election studies systematically collaborated with each other in the collection of comparable data, although the European Election Study, which covers all European Union countries on the occasion of European Parliament elections, has shown the potential of such endeavours (van der Eijk and Franklin, 1996; Katz and Wessels, 1999; Schmitt and Thomassen, 1999). This significant omission is now being corrected through the Comparative Study of Electoral Systems Project (CSES) which since 1996 has seen at least two dozen countries administer a common module of questions designed to facilitate the examination of the impact of constitutional and electoral systems on voting behaviour (Comparative Study of Electoral Systems, 1996).

But as might already be apparent, our argument implies more than just the deployment of a temporal and comparative framework. It also requires three other important if related changes to our traditional approach towards election studies. The first of these is that we cannot expect to understand elections and electoral behaviour simply by looking at them through the prism of the voter. Rather we need also to measure the macro-context within which electoral behaviour takes place, be this the policy platforms of the parties, the output of the media, or a country's political or economic structure.

The reason of course why we want to measure the environment is in order to assess its impact. This brings us to our second change. As we have noted, much election study based research has hitherto consisted of the identification of associations measured at the level of the individual

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voter, associations that with varying degrees of success may be taken to denote causation. But once we decide to bring context into our picture we are often no longer simply interested in whether A causes B. Rather we are interested in whether the relationship between A and B varies according to the context, C. For example, is the relationship between social background and vote stronger when parties make ideologically more differentiated appeals? Or is the association between candidate evaluations and vote choice stronger in presidential systems than in parliamentary ones? In short, we become interested in the conditions under which relationships exist or become stronger rather than just establishing the general validity or otherwise of particular individual level associations.

Such considerations apply even if we have panel data at our disposal. We have noted that panels are better placed to measure volatility, that is the amount of switching of electoral preferences between any two points in time. Volatility is often regarded as an attribute of voters, the product of a low level of psychological attachment to parties. Yet in truth volatility may also be an attribute of elections. Voters might, for example, be expected to be more likely to change their preferences during an election campaign if the parties' policy positions are close to each other than if they are far apart. Or voters may be thought more likely to switch their votes from one election to the next if one or more parties adopt significantly different policy positions at succeeding elections. Thus just as we may be interested in the contexts under which the association between two variables in a single wave survey may be weaker or stronger, we can also be concerned to establish the conditions under which the relationship between variables collected in different waves of a panel are weaker or stronger.

From this flows our third change of approach. If each election potentially provides a unique context, then what will determine the power of our research is not simply the number of respondents that we have interviewed at any particular election (or indeed over how many years election studies have been conducted), but rather how many elections (of differing character)

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have been covered. It is only when a number of elections have been studied, some of them taking on one character, some another, that we can begin to assess whether there are any general statements that can be made about the relationship between context and voter behaviour. And of course we have to bear in mind that more than one potentially significant aspect of context may vary between elections so it is relatively easy for the number of theoretically relevant variables to be greater than the number of elections for which we have data.

If we look at national election studies in terms of the number of elections studied, we see them in a new light. We discover that rather than being middle-aged they are at most only now coming of age. The US study of the 2000 presidential election was but the 18th such study. The Swedish study is no more than a teenager, having conducted its 14th study in 1998, while with just ten elections the British study is only now bordering on adolescence. Far from approaching a mid-life crisis, our national election time series are in truth only just beginning to make it possible to address some of the questions we have raised about the possible influence of political context.

Consider once again for example, the claim that the strength of the class or indeed any other cleavage depends on the ideological distance between the parties, a distance that may fluctuate from election to election. While that claim appears to fit the evidence well in Britain from 1970 onwards, it seems incapable of accounting for the strength of the association between class and vote at the two elections covered by the British Election Study series prior to that date. The claim can thus only be considered plausible rather than proven. Meanwhile the relationship between class and vote proved to be at its weakest yet at the last election in 1997, a finding that is consistent with the argument that a secular process of class dealignment is in operation as well as with the claim that what matters is the ideological distance between the parties. Quite evidently the relative merits of the respective theories cannot be determined without evidence from further elections.

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National election studies are then in fact about to present us with an important new opportunity to advance our understanding of electoral behaviour. By exploiting the developing time series within countries, and by engaging in systematic programmes of international collaboration between countries, they are beginning to make it possible to turn elections and systems from constants into variables. Not least of the attractions of this is that a field of scientific endeavour that has hitherto been dominated by theories derived from social psychology (party identification), economics (rational choice theory) and sociology (class dealignment) may at last begin to explore the role that politics plays in voting behaviour.

To exploit this potential we do not need to change the design of election studies so much as the way we approach their conceptualisation and analysis. We need to regard any individual election study as an instance of context and to introduce into our analyses measures of what we think are the theoretically important features of that context (see, for example, Weakliem and Heath, 1999). Yet our ability to do this rests on our willingness to be patient. We can only understand how elections differ from one another if indeed we have collected the same information at each election. Asking the same old questions in the same old election studies using the same old research designs might appear to be a symptom of a research endeavour that is approaching sclerotic old age. In truth such an approach is vital to our ability to understand the electoral process. Having nurtured election studies through their youth, we should certainly ensure that we do not now miss out on the fruits of their young adulthood through premature change or retirement.

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