Political information and motivation: A case of reciprocal causality?

James R. Tilley, Patrick Sturgis+ and Nick Allum+*

*Nuffield College, Oxford

+Department of Sociology, University of Surrey

Political knowledge has been shown to influence a host of substantively important outcomes, such as participation, issue preference and vote choice. The causes of individual heterogeneity in knowledge have gone relatively unexplored however. What leads some individuals to become political sophisticates and others not? Generally, motivation has been considered as key in this regard; those who are interested in politics seek out and retain political information which leads, in turn, to higher levels of interest in politics. Using data from the 1992-1997 and 1997-2001 British Election Panel Studies, however, we find no evidence of a reciprocal effects mechanism. Although this 'virtuous circle' model of sophistication has accrued some empirical support in the US, with British data we find evidence only for a uni-directional effect from interest to knowledge. We argue that previous analyses are potentially flawed due to their reliance on cross-sectional data and likely violation of model assumptions.

It is increasingly apparent that political knowledge has a significant role to play in explaining variation in political attitudes and behaviour (Delli Carpini and Keeter 1996; Bartels 1996; Althaus 1998; 2003; Converse 2000). Whether this emphasis is genuinely novel or simply a return to the ideas of 'sophistication' that had their hey-day in the in the 1960s (Campbell *et al* 1960, Converse 1964) is debateable. It is nonetheless clear, however, that scholars investigating electoral behaviour can no longer ignore the fact that voters vary in how much they know about politics. Virtually all major theoretical accounts of voting decisions now include political knowledge in some form or another. Advocates of economic voting have shown that the influence of information about the economy on preferences is moderated by political knowledge, and not always in the expected direction. For example, although, as we might expect, economic news coverage has greater influence on the vote intentions of more sophisticated citizens (Krause 1997), it appears that 'pocketbook' voting is also, counter-intuitively, more prevalent among the more knowledgeable (Mutz 1993, Gomez and Wilson 2001). Equally, issue voting, 'core-values' and social group explanations of vote choice are now assumed to interact with political knowledge. This moderation effect applies not only to vote choice (Bartels 1996; Delli Carpini and Keeter 1996; Zaller 1992; Tilley and Heath 2003; Bartle 1997), but also to the impact of values (Brewer 2003; Goren 2001; Zaller 1991, 1992; Delli Carpini and Keeter 1996) and the media (Krosnick and Kinder 1990; Krosnick and Brannon 1993) on policy preferences.

Yet despite this wealth of research on the exogenous influence of knowledge on attitudes and behaviour, little attention has been paid to the antecedents of variation in political knowledge within and between individual voters. Extant empirical investigations have mostly relied on regression-based analysis of cross-sectional data. On the whole this has shown that, while social characteristics such as age and social class are only weakly related to political knowledge, the strongest predictors tend to be cognitive variables such as political interest, educational attainment and media consumption (see for example Neuman 1981; Lambert *et al* 1988; Nadeau and Niemi 1995; Bennett 1988, 1996; Bartle 1997, Delli Carpini and Keeter 1996).

Two main explanations are offered as to why this is the case. The first assumes that knowledge, interest, education and media consumption are essentially indicators of the same latent 'political sophistication' construct. At its crudest level this perspective posits that knowledge, interest and so forth can be regarded as synonymous. Indeed, several authors investigating the conceptual basis of political sophistication use these indicators interchangeably, or combined into an additive scale (for example, Macdonald *et al* (1995) use interest and education as a proxy for sophistication). Under close scrutiny, however, it is clear that an individual's interest in, attention to, knowledge of and interest in politics are, theoretically and empirically, quite distinct.

A conceptually more credible approach treats sophistication as multi-dimensional, with each sub-dimension having somewhat different causes and effects. Neuman (1986) specifies three main dimensions of sophistication in politics; political 'knowledge', political 'salience' and political 'conceptualization'. While political 'salience' does to some extent conflate attentiveness to the media with interest in information received from the media, it is clearly an improvement to specify these dimensions as sub-components, rather than merely observed indicators of the broader sophistication construct (Neuman 1986, Guo and Moy 1998).

The problem with this multi-dimensional approach is that the possibly causal relationships between the main factors remain unexplained and unexplored. Thus, many authors have tended to concentrate on knowledge as the primary facet of sophistication, with political interest and media exposure posited as its primary exogenous causes (e.g. Bartle 2000). While this may result in models that are able to reliably predict variation in citizen knowledge, there is a clear danger that these estimates suffer a significant endogeneity bias. For, while political interest may indeed by fully exogenous to information holding, it is easy to construct a coherent narrative which suggests that increased knowledge of a subject such as politics might also lead to a concomitant increase in interest in that subject. As Smith puts it:

> *"Interest in politics presumably causes people to pay more attention to politics and thus to learn more about politics… but knowing a good deal about politics is likely to make people more interested in it"*
>
> (Smith 1989, p192).

While such a 'virtuous circle' model of political knowledge and interest appears to be an implicit assumption in much sophistication research (see, for example, Delli Carpini and Keeter 1996: p347), few studies have attempted to empirically confirm the existence of this non-recursive relationship. Two exceptions to this rule are Nadeau *et al* (1995) and Luskin (1990), both of which provide support for a reciprocal effects model using 2 Stage Least Squares (2SLS) regression on U.S. cross-sectional data. While these instrumental variable approaches are a considerable improvement on conventional strategies of handling simultaneity in cross-sectional analyses, their estimates will only be unbiased if a rather strict set of assumptions about the structure of the variance/covariance matrix can be met (Bowden and Turkington 1984; Finkel 1995).

FIGURE 1 ABOUT HERE

Figure 1 shows a (simplified) version of Luskin's sophistication model. In order to identify the reciprocal paths, the instruments for interest and knowledge need to be distinct; i.e. each block of instruments must be strong predictors of one endogenous variable but only weakly related to the other. Luskin uses parental interest and age as his instruments for respondent political interest, and education, media usage and social class as his instruments for political knowledge. While this specification is sufficient to allow model identification, it is not at all clear that the full set of assumptions for unbiased 2SLS estimation have been met. In particular, the specification of zero covariance between education, media use and political interest seems, at best, expedient. Indeed Smith, in his exhaustive treatment of the question, cautions against this type of specification:

> *"What causes sophistication that does not also cause interest? … I could arbitrarily identify the model with a series of false assumptions. For instance I could claim that education causes interest in politics, but nothing else … But making such false assumptions would just be biasing and distorting the results"*
>
> (Smith 1989, p194).

Thus, a more theoretically plausible model would be as shown in Figure 2, with all regression paths estimated between exogenous and endogenous variables and a reciprocal path between interest and knowledge.

FIGURE 2 ABOUT HERE

This parameterization would, however, re-introduce the identification problem that Luskin's specification is designed to avoid. The estimation problem for these parameters would appear, then, to be near intractable with cross-sectional data.

Analysis of repeated measures data sidesteps many of the endogeneity problems inherent in data collected at a single time point as observations at time t_1 can clearly not influence observations at time t_{-1}. However, repeated measures of political knowledge on probability samples of the general population are extremely rare. This is primarily because survey researchers are rightly wary of endangering the cooperation of already reluctant respondents by repeatedly administering 'tests', but also because there is a basic, if implicit, assumption that political knowledge is highly stable in the short to medium term and little point, therefore, in repeatedly measuring it. In the remainder of this paper, we take advantage of British panel data containing repeated measures of both political knowledge and interest to separate out the causal effects of each on the other over time

Data and Measures

We use data from two panel surveys, the British Election Panel Study of 1997-2001 and a truncated version of the 1992-1997 British Election Panel Study running from 1995-1997. We were unable to use the full 1992-1997 panel because the knowledge and interest questions were not administered in the 1992-1994 waves. We use a complete case analysis, taking only those respondents who participated in all five waves for the 1997-2001 panel and those who participated in the 1995, 1996 and 1997 waves of the 1992-1997 panel.

Political Knowledge

Following Andersen (Andersen 2003; see also Andersen *et al* 2002, 2004), we use the correct ordering of the three main British political parties[1] on three different issue dimensions as our measure of political knowledge. The three issue dimensions are: European integration, income redistribution; and taxation and spending. For each issue, respondents are presented with an 11-point scale with contrasting statements anchoring each pole (for example, 1 reading "cut taxes and spend less" and 11 reading "increase taxes and spend more") and are asked to locate first themselves, then the three main parties on each dimension. The questions read as follows:

European Integration

"Some people feel that Britain should do all it can to unite fully with the European Union. These people would put themselves in Box 1. Other people feel that Britain should do all it can to protect its independence from the European Union. These people would put themselves in Box 11."

Income redistribution

"Some people fell that government should make much greater efforts to make people's incomes more equal. These people would put themselves in Box 1. Other people feel that government should be much less concerned about how equal people's incomes are. These people would put themselves in Box 11."

Taxation and spending

"Some people feel that the government should put up taxes a lot and spend much more on health and social services. These people would put themselves in Box 1. Other people feel that the government should cut taxes a lot and spend much less on health and social services. These people would put themselves in Box 11."

For each dimension, therefore, there are three rankings that respondents can get either right or wrong: Conservative v Labour; Conservative v Liberal Democrat; and Labour v Liberal Democrat. On all three dimensions the correct ranking places the Conservatives to the right of both Labour and the Liberal Democrats. On Europe and tax/spend, the correct ranking placed Labour to the right of the Liberal Democrats, while on equalising incomes, the rank ordering places Labour to the left of the Liberal Democrats. Each scale therefore yields three binary variables, with '1' indicating a correct placement and '0' indicating an incorrect placement. 'Don't know's and responses that placed two parties at the same point on the scale were treated as incorrect and coded zero.[2] 'Correct' rakings on these dimensions were taken from expert surveys and published manifesto analyses (Bara and Budge 2001; Laver 1998). Before the 1992 election, placing parties on the tax-spend and income redistribution issues was straightforward for all parties, as both expert surveys and manifesto analysis put the parties as running on a left to right spectrum from Labour to Conservative, with the Liberal Democrats in between (Laver and Hunt 1992; Budge 1999). Whilst there were some major changes in the policy positions of the Labour and Liberal Democrat parties after 1992 (Heath et al 2001), the Conservatives remained consistently more right-wing than both their opponents on economic issues. The ordering of the parties on the European issue is also quite clear. Although the Conservative Party strengthened its Eurosceptic view following the 1992 election, there was no change in the ordering of the parties on the European dimension issue between 1992 and 2001, with the Conservatives consistently the most Eurosceptic and the Liberal Democrats the most Europhile, with Labour somewhere in between (Bara and Budge 2001; Budge 1999).

Confirmatory factor models of the nine derived binary variables showed that the indicators taken from the Labour/Liberal Democrat contrasts did not load on the same common factor as the variables derived from the Labour/Conservative and Liberal Democrat/Conservative contrasts; factor loadings for these items were low and the models showed a poor fit to the data. Dropping these items significantly improved the fit of the data, with all items loading on the same common factor, which we interpret as political knowledge. For our final knowledge measure we therefore decided to drop the Labour/Liberal Democrat items, yielding a six item scale. This measure, of course, taps only one aspect of political knowledge – what Delli Carpini and Keeter term the "people and parties" dimension (Delli Carpini and Keeter 1996). We have no indicators of other important components of political knowledge, such as knowledge of the institutional structures of government, how government passes legislation and the names of candidates and representatives. Nonetheless, knowledge of the policy positions of the main parties is perhaps the most important dimension of political knowledge (Gilens 2001; and the one we might expect to be most responsive to variation in political interest.

Political interest

Our other key analytical variable is political interest, which is measured at all five waves of the pane using the standard question "How much interest do you generally have in what is going on in politics? A great deal, quite a lot, some, not very much, none at all".

Analysis

We begin by fitting a reciprocal effects model to the 1997 cross-sectional data. This is equivalent in form to that used by Luskin (1990), although we employ fewer exogenous variables and estimate the model with Maximum Likelihood rather than 2 Stage Least Squares[1]. Political knowledge, measured as a summed scale, is regressed on attention paid to politics in the media and political interest is regressed on an indicator of educational attainment. Reciprocal paths run between political knowledge and political interest. The path diagram for this model (Model 1), showing standardised parameter estimates, is presented in Figure 3.

FIGURE 3 ABOUT HERE

[1] Note that we are not here trying to replicate Luskin's model, as the same variables are not available in our data set. We are merely showing that using an instrumental variable approach, identification of these reciprocal parameters is possible.

All four regression paths are significant at the 99% level of confidence or below. As with Luskin's cross-sectional analysis, the parameter estimates support the hypothesis of a non-recursive relationship between political interest and political knowledge. Measures of overall model fit, however, indicate that the model is misspecified (Chi Square = 83 on 2 degrees of freedom, $p < 0.001$; CFI = .895; RMSEA = .143).[3] Examination of modification indices reveals that the misspecification relates to the constraint of regression paths between education and political interest and media attention and political knowledge being zero. These constraints are applied for purposes of model identification, rather than on a priori theoretical grounds and are clearly not justifiable given the empirical data. So, although the model shows a significant relationship in both directions, there are several alternative theoretical models that could explain this, beyond the existence of an actual reciprocal causal relationship.

First, the misspecification of structural parameters discussed above, could be the cause. That is to say the significant paths between knowledge and interest may arise due to the empirically unjustified constraints on other regression paths in the model. Because the (non-zero) relationships between media attention and knowledge and between education and interest are constrained to zero, these effects will manifest themselves as spurious correlations between knowledge and interest (Finkel, 1995). It is not, of course, possible to test this hypothesis however, because such a model would not be identified. Second, it is possible that knowledge and interest are causally unrelated to one another but are caused by the same variable(s), which are not included in the model. Again, the effects of such unobserved causal variables would manifest themselves through the paths linking knowledge and interest. A third reason why the apparent reciprocal relationship may in fact be spurious is because neither variable makes any correction for error in the measurement of these latent constructs. It is unlikely, however, that these survey questions are perfect measures of the underlying concepts and, to the extent that their observed variance is at least partially related to unobserved variables, the effect will again be to inflate the magnitude of the association between the uncorrected variables (Bohrnstedt and Carter 1971).

It is clear, then, that for both statistical and theoretical reasons, cross-sectional data does not give much leverage on questions of reciprocal causality and we must be extremely cautious when making causal inferences on the basis of this type of association. However, in the current instance, because we are able to make use of longitudinal measures of both knowledge and interest, we are able to militate some of the problems discussed above. We use a cross-lagged panel data model (Campbell and Kenny 1999; Finkel 1995; Marsh and Yeung 1997) specified over five waves of measurement. Where there are two variables of interest, Y_1 and Y_2, each variable at t_2 is regressed on both its lagged score and the lagged score of the other variable at t_1. Cross-lagged panel models are particularly well-suited to examining reciprocal causality because they provide an estimate of the (lagged) effect of each variable of interest on the other(s), net of autocorrelation of each variable with its lagged measurement. Cross-lagged models, therefore, tell us how much variation in X at time t_1 is able to predict change in variable Y between times t_1 and t_2, net of controls specified in the model. A schematic path diagram of our *a priori* model is presented in Figure 4.

FIGURE 4 ABOUT HERE

Political knowledge is specified as a latent trait, measured by six observed binary indicators, two from each issue placement question. The residual variances of the knowledge indicators from the same issue question are allowed to covary due to their common subject matter and question wording. Factor loadings for the same item are constrained to equality across waves to impose meaning invariance on the items as measures of political knowledge (Meredith 1993).[4] Interest is measured using the single indicator described earlier. Given our previous discussion of the potential biasing effects of measurement error, this is clearly not ideal but there were no suitable additional indicators of political interest administered at all five waves of the panel.

In the absence of multiple indicators of a concept, it is advisable to set the error variance of the single indicator to a plausible value, based on theory, or previous research. In this case we used a three item model of political interest from variables included in the 1997 wave to obtain an estimate of the error variance of the political interest variable. The two indicators, in addition to the item under consideration, were 'interest in political news on the television' and 'interest in political news in newspapers'. Error variance for a single indicator can be calculated as the product of the variance of the item and 1-Cronbach's Alpha for the scale from which it is taken (Hayduk 1987). Application of this formula gives an error variance of .23 for political interest in the 1997 wave (scale Alpha = .77; variance of political interest = 1). We adjusted this figure to .20 for the subsequent waves because this level of increase in reliability between the first two waves of a panel is common in attitudinal data (Jagodzinski et al 1987; Sturgis and Allum 2004).

The model applies controls for age, sex, educational level and attention paid to politics in newspapers, all measured in 1997. Where, after initial model estimation, control paths were found to be non-significant, these were constrained to zero and the model re-estimated.

The unstandardised path coefficients for the stability and lagged effects are constrained to equality across waves. This is based on the rationale that (a) these causal influences are likely to be stable over this relatively short period and (b) because estimating each effect freely would impose a highly unlikely 'linear annual' time function on the causal mechanism. There is no reason to assume that knowledge and interest influence one another over neat annual cycles and imposing such a structure would likely lead to biased estimates of any underlying causal processes (Gollob & Reichardt, 1987; Lorenz, Conger, Simons, & Whitbeck, 1995; Sher, Wood & Raskin, 1996). Although applying this equality constraint does not provide a solution to the problem of mapping discrete measurement intervals on to continuous processes, it does give a longer and, therefore, more realistic time frame over which to examine the hypothesised relationships. We can think of the cross lagged and stability coefficients from this specification then, as representing 'average' effects over the five year duration of the panel.

The final thing of importance to note about the model in Figure 4 is the specification of covariances between the disturbance terms, both across and within waves, of the endogenous variables. Regression analysis assumes zero order coefficients for these parameters and violation of these assumptions results in biased estimates of structural parameters (James, Mulaik and Brett 1982). There are good reasons to assume, however, that in many instances this assumption is unwarranted. If, for example, a third variable, Z, causes both endogenous variables Y_1 and Y_2 but Z is not included in the model, the disturbance terms of Y_1 and Y_2 will necessarily be correlated (Anderson and Williams 1992). The problem is perhaps worse with longitudinal data because of the likelihood of auto-correlation between the disturbances of the lagged endogenous variables, resulting from a stable unobserved cause of the variable in question over time (Williams and Podsakoff 1989). This is not an esoteric statistical point; failing to account for these error covariance structures can lead to seriously flawed causal inference. In particular, it is probable that unobserved variable bias will result in incorrect attribution of causal relationships between variables (Singer and Willett 2003; Anderson and Williams 1992). As with the control variables, if these disturbance covariance paths were found to be non-significant after initial model estimation, they were constrained to zero and the model re-estimated. All models are estimated using WLSMV estimation in MPlus 2.14 (Muthen and Muthen 2003).

Table 1 shows the key unstandardised estimates from the model specified in Figure 3.[5] Parameters significant at the 95% level of confidence are indicated with an asterisk, standard errors are in parentheses and standardised coefficients in bold. The first column of Table 1 shows a model which constrains the disturbance covariances to zero, the second column shows the same model, with the disturbance covariances freely estimated.

TABLE 1 ABOUT HERE

Model 2, then, provides continued support for a reciprocal effects model; prior knowledge level is significantly and positively related to subsequent change in political interest and political interest is equivalently related to political knowledge. The stability coefficients are positive, significant and of a high magnitude. Both cross-lagged effects are, however, weak in magnitude and only just reach statistical significance at the 95% confidence level, which is about what we should expect given the high stability of these variables over the time period examined; if neither construct shows much change at the individual level, we are likely to find that any predictors of change exert only a weak effect.

Model 3, however, supports only a uni-directional effects model; once covariance paths between disturbance terms are estimated the parameter linking political knowledge to subsequent change in political interest becomes non-significant. Model fit indices show both models fit the data well according to conventional criteria (Hu and Bentler 1998). Model 3 appears to show a slight improvement in fit, relative to Model 2, though note that WLSMV estimation does not yield a Chi Square distribution for nested models to actually test for difference in fit between them. We interpret this difference in fit, nonetheless, as supporting the inclusion of these parameters in the model.

It would seem then, that the initial support for a reciprocal effects model in both Models 1 and 2 was artefactual. Using longitudinal data and a more appropriate statistical model led to a sharp drop in both statistical significance and magnitude of effects between Models 1 and 2. Further controlling for disturbance covariances resulted in the regression of interest on knowledge becoming non-significant over the duration of the panel. In the light of our earlier discussion of causal inference with observational data, it would appear that extant empirical support for a reciprocal effects model using cross-sectional data, results largely from unobserved variables which are causally antecedent to both knowledge and interest.

TABLE 2 ABOUT HERE

Lastly, we investigate the possibility that these findings are peculiar to the political context examined. Table 2 shows the same models fitted to the 1992-1997 British Election Panel Study. Equivalent variables were only included in the 1995-1997 waves of the panel, so we have fewer observations than were available in Models 2 and 3. The results, however, are almost identical; Model 4 supports a reciprocal effects model with similar coefficient magnitudes and variance estimates to those found in Model 4. Again, however, allowing covariance paths between disturbance terms to be freely estimated in Model 5 results in the path which regresses interest on knowledge becoming non-significant. Both models again fit the data well, with Model 5 showing indications of a slight improvement in fit, relative to Model 4.

Discussion

The results we have presented here suggest that rather than knowledge and interest being reciprocally related, interest is, in fact, fully exogenous to knowledge, in the short to medium term. An increased interest in politics over the course of an electoral cycle appears to stimulate a growth in an individual's ability to correctly place parties on important issue dimensions. We find no evidence, however, to support the idea that becoming more knowledgeable about politics leads to higher reported interest. These relationships were found to hold in two different election cycles. These findings are both counter-intuitive and contrary to the prevailing wisdom in political science. What might have given rise to these apparently anomalous findings? First, we must consider some limitations to the inferences which can be drawn from our own analysis.

Although our use of repeated measures data gives some leverage on the directional flow of causality between our two concepts, the historical period available to us is limited to a five year electoral cycle in a single country. In our replication analysis, we are only able to make use of an even shorter, three year panel. This means we are unable to pick up on more 'glacial' individual trajectories which, perhaps, begin to develop in adolescence and continue in gradual increments throughout the life course. Indeed, such a gradual, life course trajectory fits more closely with theoretical accounts of the development of political sophistication than our electoral cycle 'snapshot' (Smith 1989, Luskin 1990). We do not, therefore, interpret our findings as demonstrating unequivocally the absence of a causal link between political information holding and motivation to acquire political information. Our inference is limited to the time period we have been able to examine.

A second limitation of our analysis derives from our operationalisation of political knowledge. Although this type of policy information has been argued to be the most important dimension of the broader political knowledge construct (cites), we are clearly measuring this, and only this, part of the concept. We cannot discount the possibility, therefore, that other types of political knowledge may indeed exert a causal influence on an individual's interest in politics over time, including the sorts of time periods we have examined here.

Having recognised these limitations to the inferences which can be made from our analyses, however, we are nonetheless confident that, for this dimension of political knowledge, over this short to medium time period, no evidence can be found to support a reciprocal causal relationship between political knowledge and interest. Furthermore, we believe that our use of repeated measures data and model specification yield more valid and reliable estimates of these effects than previous investigations, relying as they do on Instrumental variables models on cross-sectional data, are able to afford.

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Figure 1: *A simplified version of Luskin's model of political interest and political knowledge*

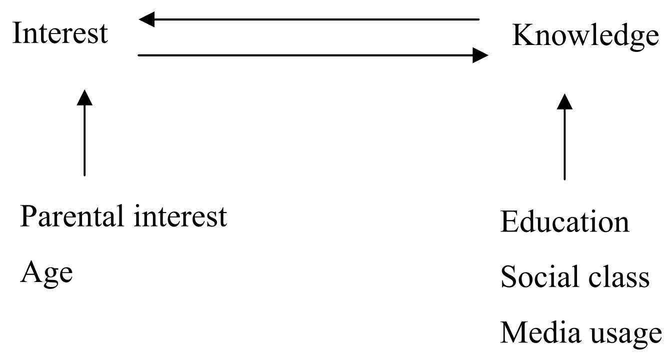

Figure 2: *Alternative model of political interest and political knowledge*

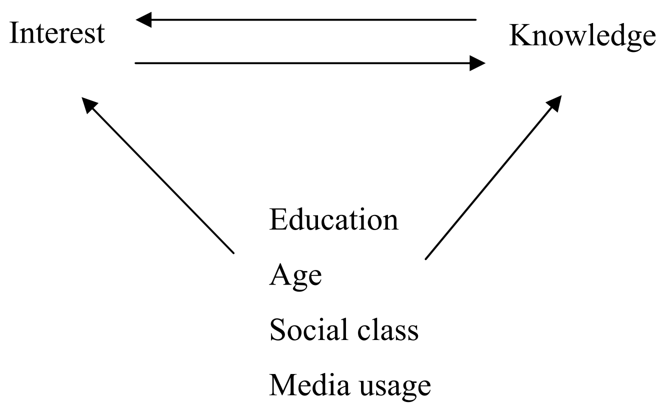

Figure 3: *Path Diagram and Parameter Estimates for Model 1*

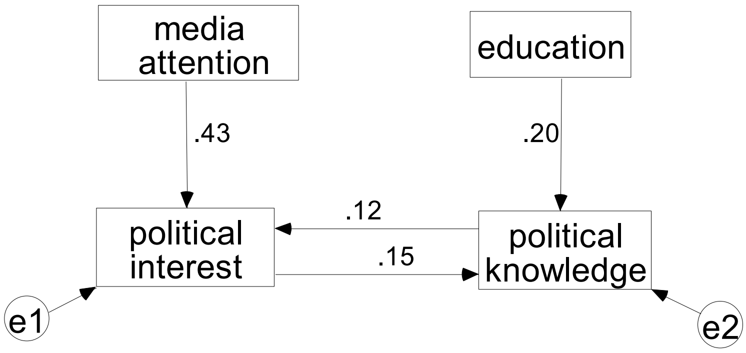

Figure 4: *Path Diagram for Longitudinal Model*

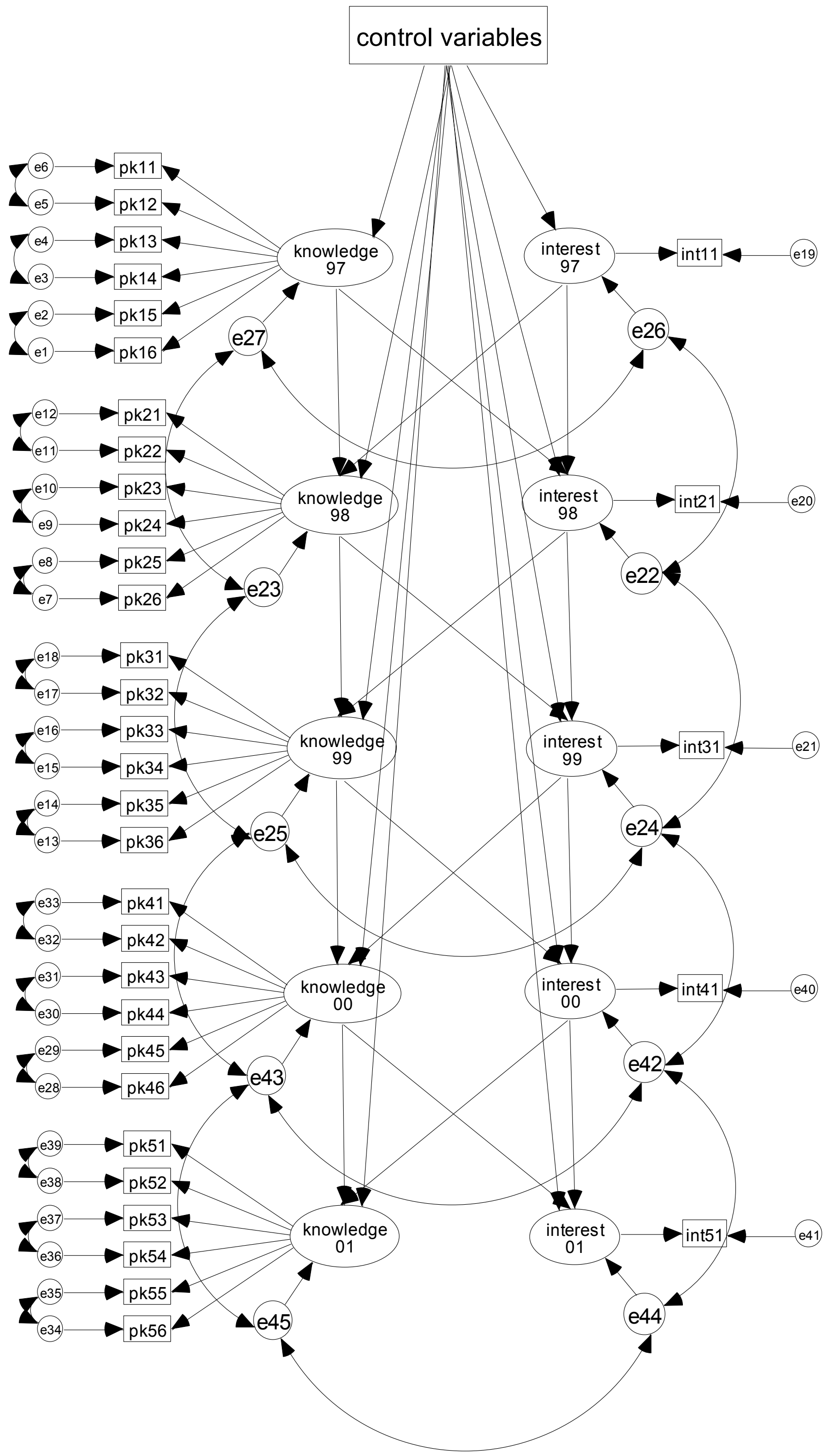

Table 1: *Estimates of Stability and Lagged Effects from Full Structural Model 1997-2001*

Parameter	Model 2 Disturbance covariances = 0	Model 3 Disturbance covariances estimated
Knowledge stability	0.927* (0.012) **0.920**	0.934* (0.012) **0.913**
Interest stability	0.946* (0.011) **0.919**	0.994* (0.013) **0.966**
Interest → Knowledge	0.030* (0.008) **0.037**	0.030* (0.008) **0.036**
Knowledge → Interest	0.047* (0.010) **0.038**	0.007 (0.011) **0.006**

Source = British Election Panel Study 1992-1997; N=1980; *=significant at p<0.05; standard errors in parentheses; standardised coefficients in bold.

Model 1: Chi Square = 1419, df = 188, p <0.001; CFI = .944; TLI = .973; RMSEA=.057

Model 2: Chi Square = 1346[6], df = 187, p <0.001; CFI = .947; TLI = .974; RMSEA=.056

Table 2: *Estimates of Stability and Lagged Effects from Full Structural Model 1995-1997*

Parameter	Model 4	Model 5
	Disturbance covariances = 0	Disturbance covariances estimated
Knowledge stability	0.857* (0.029)	0.868* (0.030)
	0.922	**0.933**
Interest stability	0.851* (0.020)	0.895* (0.021)
	0.882	**0.928**
Interest → Knowledge	0.078* (0.021)	0.059* (0.025)
	0.093	**0.068**
Knowledge → Interest	0.061* (0.020)	0.012 (0.024)
	0.057	**0.011**

Source = British Election Panel Study 1992-1997; N=1980; *=significant at $p<0.05$; standard errors in parentheses; standardised coefficients in bold.

Model 1: Chi Square = 637, df = 77, p <0.001; CFI = .979; TLI = .986; RMSEA=.074

Model 2: Chi Square = 549[7], df = 76, p <0.001; CFI = .982; TLI = .988; RMSEA=.06

Notes

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1 In some years in Scotland and Wales the scales also asked a question regarding the placement of the two nationalist parties; these assessments have not been taken into account here however.

2 Mondak (2001) identifies some technical problems concerning 'don't knows' and item format that tend to afflict civics type scales. For example, he points out that open-ended questions that ask for, say, the name of the Vice-President, discourage 'shy' respondents that may have partial knowledge. He argues that knowledge or information scales should be designed to limit any don't know responses and encourage guessing thereby somewhat reducing reliability, but increasing validity. We think this is less of a problem since these placements are direct tests of knowledge. If one is voting on the basis of these issues then one needs to know where the parties stand relative to one another, it seems reasonable to include 'don't knows' as incorrect responses.

3 There is an ongoing debate amongst practitioners of SEM concerning the relative merits of measures of 'exact' and 'close' model fit. We do not attempt to resolve these issues here but present both varieties and leave readers to judge the adequacy of the models themselves. Because Chi Square is sensitive to sample size and distributional misspecification, alternative measures of model fit, which correct for factors such as sample size and complexity of the model, are frequently used when sample sizes are large and/or when many parameters are being estimated. Hu & Bentler (1999) advocate the use of the Comparative Fit Index (CFI) in conjunction with the Root Mean Square Error of Approximation (RMSEA). They suggest cut-off criteria of > 0.95 for CFI in conjunction with values of RMSEA < 0.06 for acceptable model fit.

4 We impose this constraint because, as specific issues rise and fall as the focus of public and media attention during the electoral cycle, we cannot be certain that correctly placing the parties on specific issue dimensions remains constant as an indicator of political knowledge over time. If this constraint of factorial invariance is not empirically justifiable, it will be

manifested in the measures of overall model fit.

[5] Contact lead author for full details of the model, including factor loadings, item thresholds, error variances and covariances.

[6] The chi-square value for WLSMV estimation cannot be used for chi-square difference tests.

[7] The chi-square value for WLSMV estimation cannot be used for chi-square difference tests.

Appendix

Question wordings for additional items used to estimate error variance of political interest question:

People pay attention to different parts of the **television** news. When you watch the news on **television**, how much attention do you pay to stories about politics...

1 ...a great deal,

2 quite a bit,

3 some,

4 a little,

5 or, none?

People pay attention to different parts of **newspapers**. When you read *(name of paper at)*, how much attention do you pay to stories about politics ...

1 ...a great deal,

2 quite a bit,

3 some,

4 a little,

5 or, none?