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Notes and Comments

Is 'Clarity of Responsibility' Important for Economic Voting? Revisiting Powell and Whitten's Hypothesis

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In the words of Martin Paldam, comparative economic voting studies suffer from a 'great instability' – i.e., economic effects appear in some countries at some times, but not others, and tend to be weak and inconsistent across studies.[1] Powell and Whitten propose a possible solution to the 'instability' of cross-national voting studies: 'to explain differences in retrospective economic voting across nations and over time we must take account of the political context within which elections take place'.[2] More specifically, they take into account the 'context of political responsibility', arguing that voters are more likely to punish/reward incumbent governments if it is very clear which parties are responsible for economic conditions.[3] They find that voters hold government parties responsible for the economy when there is high 'clarity of responsibility', but not when 'clarity of responsibility' is low.

In this Note we respecify and update Powell and Whitten's model, and then retest the 'clarity of responsibility' hypothesis in two different ways. First, in addition to looking at *change in vote for governing parties as a whole* as the dependent variable, as do Powell and Whitten (hereafter, 'PW'), we look at what happens to vote for the *dominant party* in governing coalitions. Secondly, we argue that there are some problems with PW's 'clarity of responsibility' measure, and we propose and test an alternative. We find no support for PW's conclusion that 'clarity of responsibility' is crucial for economic voting; instead, we find that 'political context' defined in a new way *may* be important.

THE ORIGINAL POWELL AND WHITTEN MODEL

PW conduct a pooled cross-sectional time-series analysis of economic voting in nineteen industrialized nations over the period 1969–88. They suggest that when institutional factors 'encourage more influence for the political opposition, voters will be less likely to punish the government for poor performance of the economy. Responsibility for that performance will simply be less clear'.[4] In other words, 'the greater the perceived unified

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[1] Martin Paldam, 'How Robust Is the Vote Function? A Study of Seventeen Nations Over Four Decades', in Helmut Norpoth, Michael Lewis-Beck and Jean-Dominique Lafay, eds, *Economics and Politics: The Calculus of Support* (Ann Arbor: University of Michigan Press, 1991), pp. 9–31, at p. 26.

[2] G. Bingham Powell Jr and Guy D. Whitten, 'A Cross-National Analysis of Economic Voting: Taking Account of the Political Context', *American Journal of Political Science*, 37 (1993), 391–414, p. 409.

[3] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 410.

[4] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 393.

control of policymaking by the incumbent government, the more likely is the citizen to assign responsibility for economic and political outcomes to the incumbents'.[5]

The dependent variable used by PW is the change in governing party's (or parties', in the case of coalition government) vote share from time $t - 1$ to time t. For both theoretical reasons and in order to 'normalize' voting shifts, PW use two lagged vote variables as independent variables; first, *previous vote percentage*, i.e., the percentage of the vote received in the last election, is used. A negative coefficient is expected for this variable, since it is much easier to lose votes from a bigger base than to gain votes. Secondly, *previous vote swing* is also used – i.e., the vote 'swing' between two elections ago ($t - 2$) and the last election ($t - 1$). Again, a negative coefficient is expected. The reasoning here is that a party or government may experience a particularly large 'swing' in one election, but that voters will then tend to 'regress to more "normal" patterns of behavior'.[6]

To measure economic performance, PW include unemployment, inflation and growth. They rely on *comparative* economic measures – that is, they assume that 'voters will evaluate governments relative to some expectations about how the economy should have performed';[7] this was captured by using international average levels of growth, inflation and unemployment as 'baselines' against which voters would evaluate their own countries' performance. PW report that their final results were 'fairly similar' using either these comparative measures or non-comparative measures.[8]

Like others who have studied economic voting, PW find that it is essential to take into account the ideology of the governing party(ies). In order to do this, they used the ratings for parties reported by Castles and Mair; these range from 0 (for ultra left) to 10 (for ultra right).[9] For multiparty governments, PW compute a government's overall score by weighting each government party according to its share of legislative seats. Defining a government as 'right-wing' if its ideology score is over 6.25, they create a right-wing dummy variable, and then create two ideology*economy interaction terms by multiplying this dummy times inflation and unemployment.

Finally, PW suggest that minority governments should be held the 'least responsible for policy outcomes', and all else being equal, would be expected to do better than majority governments.[10] Accordingly, they include a dummy variable for minority governments.

Thus the basic PW model is:

$$\begin{aligned}\text{Change in vote} = {} & \alpha + \beta_1\text{growth} + \beta_2\text{unemployment} + \beta_3\text{inflation} \\ & + \beta_4\text{right-wing*unemployment} + \beta_5\text{right-wing*inflation} \\ & + \beta_6\text{minority dummy} + \beta_7\text{vote percentage last election} \\ & + \beta_8 \text{ 'swing' at last election.}\end{aligned} \tag{1}$$

Economic effects for different types of governments are thus shown as follows: the impact of unemployment on 'moderate-to-left' governments is shown by β_2; its impact on right-wing governments is ($\beta_2 + \beta_4$). Similarly, inflation effects for the two types of

[5] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 398.

[6] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 397; also see Martin Paldam, 'The Distribution of Electoral Results and the Two Explanations of the Cost of Ruling', *European Journal of Political Economy*, 2 (1986), 5–24.

[7] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 396.

[8] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 409.

[9] Francis G. Castles and Peter Mair, 'Left-Right Political Scales: Some Expert Judgments', *European Journal of Political Research*, 12 (1984), 83–8.

[10] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 401.

governments are shown by β_3 and $(\beta_3 + \beta_5)$, respectively. Growth is hypothesized by PW to have a positive impact on all types of government. Again, all economic measures are 'comparative'.

PW first run a regression with all countries together, and find that these economic variables 'routinely fail to reach statistical significance at the 0.05 level'.[11] It is thus suggested that a 'further refinement' of the model is needed, and they turn to investigation of the impact of 'clarity of responsibility'. They create an 'index of clarity' based on five different indicators: 'cohesiveness' of the parties, whether a country has strong legislative committees that can be chaired by opposition party members, opposition control of one legislative chamber, 'pure' minority government, and number of parties in government. They assign one point for each of the first four features in existence at each election, and one point for each party in a government beyond the first; average scores for each country are then computed. The sample was divided into a 'clearer responsibility' group and a 'less clear responsibility' group by splitting the countries roughly where the largest gap between average 'clarity' scores occurred. By this measure, the 'high clarity' of responsibility countries are considered to be Australia, Austria, Canada, France, Greece, Ireland, Japan, New Zealand, Sweden, the United Kingdom and the United States. The low-clarity countries are Belgium, Denmark, Finland, Germany, Italy, the Netherlands and Norway. Note that while the index takes into account more than number of governing parties, ultimately the low-clarity sample is heavily dominated by countries that often have *coalition government*.

Thus, the PW 'clarity of responsibility' hypothesis is that in countries that are defined as having 'low clarity of responsibility', voters will not credit or blame incumbents for the economy because of their inability to pinpoint a responsible party. PW confirm their hypothesis: in the low-clarity group, none of the economic variables are significant, while in the high-clarity group, all of the economic variables except 'comparative inflation' are significant at least at 0.05.[12]

REPLICATING THE POWELL AND WHITTEN MODEL

We now turn to the results of our re-testing of this model. In using pooled cross-sectional time series analysis we needed to be concerned with both cross-sectional or country specific effects and time-serial effects.[13] We found, as did PW, that once we added lagged vote variables, we had no evidence of autocorrelation in the model, allowing us to use ordinary least squares (OLS).[14] We used the same tests for robustness reported in PW's Appendix; these are discussed in the last section of this Note.

When we first began this work, we requested PW's dataset, and they provided the political data but no longer had the original economic data. We then collected the economic data on our own, and expanded the dataset to include more elections. At a later date, Whitten re-collected the economic data for another project and graciously agreed to share it with us. These data were in 'comparative' format similar to the original, but

[11] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 405.

[12] Powell and Whitten, 'A Cross-National Analysis of Economic Voting', p. 407.

[13] See Nathaniel Beck and Jonathan N. Katz, 'What To Do (and Not to Do) with Time-Series Cross-Section Data in Comparative Politics', *American Political Science Review*, 89 (1995), 634–47; James Stimson, 'Regression in Space and Time: A Statistical Essay', *American Journal of Political Science*, 29 (1985), 914–47; Lois Sayrs, *Pooled Time-Series Analysis* (Newbury Park, Calif.: Sage, 1989).

[14] Bartlett's test (i.e., the ratio between the pooled autocorrelation function (ACF) and its standard error) was insignificant (at all lags) at the 0.05 two-tailed level. The Q-statistic (distributed as Chi-square) was also statistically insignificant.

were not identical to it. All of the models that we now discuss were thus run with three different sets of economic indicators: the 'comparative' measures provided by Whitten, and two sets of more straightforward 'non-comparative' measures which differ in how they measure unemployment. One set of non-comparative measures includes real gross domestic product (GDP) growth, inflation, and unemployment rate (i.e., percentage of labour force unemployed); the other uses the same growth and inflation data, but *change* in unemployment rate. Inflation is measured as the percentage change in the consumer price index from the quarter a year prior to the election, to the quarter of the election; similarly, growth and unemployment *change* are the percentage changes in real GDP and unemployment rates, respectively, over that period.

To start with, we re-run the original PW model, *using the 'comparative' economic dataset provided to us by Whitten*, and including only 1969–88, the years included in PW.[15] We start, as did PW, with all countries put together ($n = 104$). Unlike PW, we do find significant economic effects for the sample as a whole (results not shown): moderate-to-left governments are hurt by unemployment and right-wing governments experience a net gain from it; both coefficients are significant at 0.05. The non-comparative economic measures we use below also yield significant economic effects for the sample as a whole. These results hold true when later cases are added, and when changes in model specification are made. In other words, our findings show that it is not *necessary* to take into account 'political context' (i.e., by dividing the sample according to 'clarity of responsibility') in order to find evidence of economic voting in our cross-national sample.

Nevertheless, continuing our replication of PW, we turn next to the high- and low-clarity sub-samples. Using the newly-collected 'comparative' measures acquired from Whitten, the results (Appendix Table A1, first two sets of columns) are rather different from PW, although the basic conclusion holds. There are still no significant economic effects in the low-clarity group, but only *one* economic variable – moderate-to-left unemployment – is significant at $p < 0.05$ for the high-clarity group (although growth is marginally significant), compared to *four* significant effects in PW.[16]

We then run the PW model with the *non-comparative* measures we collected – growth, inflation and one of the two unemployment rate measures. Using *change* in unemployment rate yields no significant economic effects at all in either set of countries (not shown). However, using unemployment rate yields strong and significant economic effects in high-clarity countries but not in low-clarity countries (Appendix Table A1, second two sets of columns). Unlike PW, we find no significant effect for growth in either group of countries. However, in the high clarity countries, *three* other economic variables – moderate-to-left unemployment, right-wing unemployment and right-wing inflation – are significant at at least 0.05, with the fourth (moderate-to-left inflation) marginally significant at $p < 0.10$. None of the economic variables or interactions are significant in the low clarity countries. Thus, we essentially replicate PW.

[15] We have been unable to determine why the '*n*' for 'low-clarity' countries is 43, while it was 41 in the original article. We removed all cases that were missing from the original article (as discussed in Powell and Whitten, 'A Cross-National Analysis of Economic Voting', fn. 3, p. 393).

[16] The new 'comparative' data provided to us by Whitten excluded Greece, while the original study did include Greece, although some Greek cases were missing (PW, p. 393). This left us with a 'high clarity' *n* of 59 rather than the 61 included in the original study. After running these 59 cases, we checked the results with Guy Whitten, who ran them separately, and he confirmed our finding. At that time, in addition to non-rightist unemployment, growth was weakly significant ($p < 0.10$). In order to make our replication as comparable as possible to the original article, we filled in our own 'comparative' data for the two Greek cases. After so doing, the growth effect became insignificant.

Before moving on to add later elections and re-test the hypothesis in new ways, we suggest two changes in model specification. First, the literature on interactions recommends that the components of interactive terms be included both singly and interacted; we thus suggest that a correct model must include the 'right-wing government' dummy by itself.[17] With this modification (last two columns, Appendix Table A1), moderate-to-left inflation for high-clarity countries loses its marginal significance; there is no change with low-clarity countries.

The second modification we propose is dropping economic growth entirely. In *all* of the models we ran for this study growth is *never* significant at 0.05, and in most models it does not even come close (in fact, its *sign* switches between positive and negative). Furthermore, its inclusion generally lowers the adjusted R^2, and it increases the possibility of multicollinearity. We therefore suggest focusing only on inflation and unemployment – the 'big two' indicators on which most economic voting studies have focused.[18] This second modification (results not shown) does not change the results beyond the changes brought about by the first modification (adding the right-wing dummy). Thus, we have confirmed PW's finding of economic effects only in high-clarity countries.

The *signs* of the coefficients for the economic variables are largely consistent across *all* the models reported in this Note and consistent with PW. Specifically, we (like PW) consistently find that the unemployment coefficient alone is negative, and the inflation coefficient positive, while the reverse is true for the two ideology*economy interactions (i.e., right-wing unemployment is positive, right-wing inflation is negative). These results suggest that parties are hurt when the economic problem that they are supposed to be competent at worsens (i.e., right-wing governments are hurt by inflation, while centre-to-left governments are hurt by unemployment), but tend to benefit when the opposite problem occurs. The persistence of these findings suggests a high degree of confidence in them.

We have found that high-clarity countries appear to experience more economic voting than low-clarity ones. The next question is whether the apparent difference between groups of countries is a statistically significant one. A Chow test is designed to answer this question.[19] This involves using the sum of squared residuals from the whole sample regression and the two subsamples to compute an F-statistic to test whether or not the two subsamples are significantly different. The results are the following: For the PW original specification, 'comparative' measures, 1969–88 (first two columns, Appendix Table A1), there is no significant difference between high-clarity and low-clarity regressions ($F = 1.01$, $p = 0.44$). For the models using our 'non-comparative' measures and PW's specification, there is still no difference between the models ($F = 0.958$, $p = 0.48$). The same is true of the final re-specified model, with right-wing dummy and excluding growth ($F = 1.06$, $p = 0.40$).

Thus, economic voting appears to be different between the two samples, but we cannot have a great deal of confidence in the strength of this difference. Our next steps will be (a) to test the impact of adding later elections: more cases may strengthen our conclusion in one direction or the other; (b) given that there does appear to be somewhat less economic voting in low-clarity countries, to test whether or not economic effects may

[17] See Leona Aiken and Stephen G. West, *Multiple Regression: Testing and Interpreting Interactions* (Newbury Park, Calif.: Sage Publications, 1991).

[18] Peter Nannestad and Martin Paldam, 'The V-P Function: A Survey of the Literature on Vote and Popularity Functions after 25 Years', *Public Choice*, 79 (1994), 213–45.

[19] Damodar Gujarati, *Basic Econometrics* (New York: McGraw-Hill, 1978).

be hidden in such countries for reasons other than 'clarity of responsibility', and (c) to test the 'clarity' hypothesis in new ways, including a new (and we argue improved) measure of 'clarity of responsibility'.

UPDATING PW

Our next step is to add data from elections through 1993 – a total of thirty-eight cases.[20] The first two sets of columns of Table 1 present the results of adding more recent elections, incorporating the model specification changes discussed above, and continuing to use the non-comparative economic measures that performed the best thus far and change in whole-government vote as the dependent variable.[21]

First, we can see that the results are essentially the same as for the earlier period, in that economic effects are significant at 0.05 only in high-clarity countries. Specifically, in high-clarity countries, moderate-to-left governments tend to be hurt by unemployment ($b = -0.48$, $p = 0.004$). The right-wing unemployment interaction is positive and significant, but somewhat negated by the negative effect of unemployment by itself, leaving only a negligible positive effect for right-wing governments ($-0.48 + 0.515 = 0.035$). The right-wing inflation effect is also significant ($b = -0.42$, $p = 0.04$).

In the low-clarity countries, we see only a marginally significant effect (right-wing unemployment, $b = 0.757$, $p = 0.08$). All of the results with these new cases are essentially the same as the 1969–88 results in Appendix Table A1. For this updated model, the Chow test is again not significant ($F = 1.12$, $p = 0.36$). Thus, again, it seems that the economy has more of an effect on voting in high-clarity as compared to low-clarity countries, although this result is not borne out with a Chow test. Is it possible that this apparent difference is due to causes other than ability of voters to place blame? We turn to that question next.

ARE ECONOMIC EFFECTS HIDDEN IN LOW-CLARITY COUNTRIES? TESTING A NEW DEPENDENT VARIABLE

Lewis-Beck notes that there are two possible reasons for finding less economic voting in coalition governments as compared to others.[22] One is the difficulty of identifying responsibility in coalitions. The other reason is that with coalition governments voters may be switching their support between parties *within* the coalition; thus, when we look at vote for all governing parties together (as do PW), evidence of economic voting that is actually taking place may be hidden. Given that minimal-connected-winning coalitions are the most likely form of government, and assuming that voters will shift

[20] We found that for Canada 1993 the change in vote for the government (which is also the prime minister's party, as this is a one-party government) was unusually large (– 27 per cent). It appears that some dynamic other than simple economic voting was at work – for example, the impact of the Quebec issue on the party system. None of the models performed very well unless we adjusted for this 'outlier', and so we eliminated that case.

[21] The ideology measure used continues to be that for government as a whole. We tried running models using *prime minister's* ideology instead of *whole-government* ideology, but we found that for all of the models, this lowered the R^2 and weakened the strength of the explanatory variables. Apparently, ideology of government *as a whole* is the best predictor of the impact of inflation and unemployment on vote for a particular party within a coalition government.

[22] Michael S. Lewis-Beck, *Economics and Elections* (Ann Arbor: The University of Michigan Press, 1988).

TABLE 1 *Respecified PW Model with Expanded Timeframe (1967–93), Both Whole Government Vote and PM's Party Vote*

	Dependent variable									
	Whole-government vote				PM's party vote				Whole-govt. vote	
	High-clarity countries		Low-clarity countries		High-clarity countries		Low-clarity countries		Full sample (all countries)	
	B	S.E.	*B*	S.E.	*B*	S.E.	*B*	S.E.	*B*	S.E.
Unemployment	− 0.480**	0.162	− 0.222	0.213	− 0.400*	0.150	− 0.074	0.201	− 0.230	0.174
Inflation	0.186	0.123	− 0.001	0.197	0.223*	0.113	− 0.021	0.169	0.053	0.160
Right-wing government	1.44	2.20	− 5.29	3.88	1.17	2.03	− 3.57	3.90	− 0.676	1.92
Right govt.*Unemployment	0.515*	0.231	0.757 $^{+}$	0.426	0.495*	0.213	0.510	0.411	0.592**	0.200
Right govt.*Inflation.	− 0.420*	0.196	0.460	0.408	− 0.451*	0.181	0.437	0.398	− 0.221	0.173
Minority government	2.47*	1.12	0.937	1.74	3.15**	1.01	1.69	1.43	1.99*	0.942
Previous vote percentage	− 0.082	0.068	− 0.075	0.063	− 0.010	0.065	− 0.097 $^{+}$	0.057	− 0.076 $^{+}$	0.043
Previous vote swing	− 0.204*	0.086	− 0.217 $^{+}$	0.122	− 0.176*	0.085	− 0.134	0.151	− 0.210**	0.070
Constant	0.716	3.72	2.06	4.49	− 3.16	3.49	1.53	1.82	1.46	3.19
'High clarity' dummy	–	–	–	–	–	–	–	–	0.193	1.99
High clarity*unemployment	–	–	–	–	–	–	–	–	− 0.300	0.204
High clarity*inflation	–	–	–	–	–	–	–	–	0.044	0.184
N	83		59		83		59		142	
R^2	0.322		0.277		0.324		0.273		0.283	
Adjusted R^2	0.249		0.161		0.251		0.157		0.222	

$^{+}p < 0.10$, two-tailed $^{*}p < 0.05$, two-tailed $^{**}p < 0.01$, two-tailed $^{***}p < 0.001$, two-tailed

votes between ideologically close parties, then vote-shifting *within* governing coalitions is quite likely.[23]

We thus suggest that an alternative way to test for economic voting cross-nationally is to look at the *vote for the prime minister's party*, which is usually the dominant coalition partner, and thus a likely target for blame or credit.[24] There have been no cross-national studies of economic voting using *vote for prime minister's party* as the dependent variable.[25] The second two sets of columns in Table 1 present the results using this new dependent variable. Our hypothesis about prime minister's (PM's) party revealing 'hidden' economic voting is not borne out; in fact, for high-clarity countries all four economic variables are now significant at 0.05, and the marginally significant effect for low-clarity countries has disappeared. Despite the continued (and even stronger) apparent difference in economic effects between the two samples, however, a Chow test still shows no significant difference between the groups of countries ($F = 1.24$, $p = 0.28$).

Our testing of the 'clarity of responsibility' hypothesis, then, has thus far produced mixed results. On the one hand, economic voting appears to exist only in countries where 'clarity of responsibility' is high. Further, this finding does not appear to be the result of 'hidden' economic effects in 'low-clarity of responsibility' countries. On the other hand, Chow tests have shown no significant difference between the two groups of countries. Perhaps testing the hypothesis differently will help to resolve the ambiguity of these findings. We turn to that next.

ANOTHER WAY TO TEST THE HYPOTHESIS: USING A DUMMY VARIABLE FOR 'CLARITY OF RESPONSIBILITY'

Instead of dividing the sample into high- and low-clarity countries, we can create a dummy variable equal to 1 when a country is defined as 'high-clarity', and 0 otherwise. We can then interact this with the economic variables, and run a whole-sample model with these three new variables. The 'clarity'-economy interactions will show the *additional* economic effect, if any, experienced by governments in high-clarity countries. Theoretically, such a model is identical to the split sample approach, but there are trade-offs to each. The split samples give us a smaller '*n*' to deal with, while the

[23] On coalition formation, see Robert Axelrod, *Conflict of Interest: A Theory of Divergent Goals With Applications to Politics* (Chicago: Markham, 1970); and Abram De Swaan, *Coalition Theories and Cabinet Formation* (Amsterdam: Elsevier, 1973). Some might argue that the appropriate dependent variable is 'all government parties', because what matters is whether voters do hold governments responsible, not whether they try to. We would argue that political scientists (and certainly politicians) have an interest in answering the question of whether or not voters are attempting to hold an incumbent 'responsible' for economic conditions. As another way to look at the issue, note that when one uses whole-government vote as the dependent variable, it really means that a different dependent variable is used for coalition versus single-party governments. For the former set of countries, the variable looks at change in vote for a number of parties summed together; for the latter, the variable looks at change in vote for one party. Yet the comparative economic voting literature tends to assume that the dependent variable is the same across all countries.

[24] There were only seven cases (out of 142) for which the largest party in the coalition was not the prime minister's party. When we ran the model shown in Table 2 with 'change in vote for dominant party' as the dependent variable, the adjusted R^2 was actually slightly lower than with change in prime minister's party vote; coefficients and significance levels were basically the same.

[25] Other studies that do look at individual parties in coalition include Paolo Bellucci, 'Italian Economic Voting: A Deviant Case or Making a Case for a Better Theory?' in Helmut Norpoth *et al.*, eds, *Economics and Politics: The Calculus of Support* (Ann Arbor: University of Michigan Press, 1991), pp. 63–81; Helmut Norpoth, 'The Economy', in Lawrence LeDuc, Richard Niemi and Pippa Norris, eds, *Elections and Voting in Global Perspective* (Thousand Oaks, Calif.: Sage Publications, 1996), pp. 299–318; Anderson, *Blaming the Government*.

whole-sample approach gives us a rather 'noisy' model in which the economic measures are repeated three times. If the results are essentially the same with both approaches, we may have more confidence in them.

The new model thus is the following (where 'hiclar' = high-clarity dummy):

$$\begin{aligned}\text{Change in vote} = \alpha &+ \beta_1 \text{ unemployment} + \beta_2\text{inflation} + \beta_3\text{right-wing dummy}_t \\ &+ \beta_4\text{right-wing*unemployment} + \beta_5\text{right-wing*inflation} \\ &+ \beta_6\text{hiclar dummy} + \beta_7\text{hiclar*unemployment} \\ &+ \beta_8\text{hiclar*inflation} + \beta_9\text{minority dummy} \\ &+ \beta_{10} \text{ vote percentage last election} + \beta_{11} \text{ 'swing' at last election.}\end{aligned} \tag{2}$$

Sample size is 142. Economic effects for different types of governments are thus shown by the following:

Moderate-to-left, low-clarity governments: unemployment = β_1; inflation = β_2
Right-wing low-clarity governments: unemployment = $(\beta_1 + \beta_4)$; inflation = $(\beta_2 + \beta_5)$
Moderate-to-left, high-clarity governments: unemployment = $(\beta_1 + \beta_7)$;
inflation = $(\beta_2 + \beta_8)$
Right-wing high-clarity governments: unemployment = $(\beta_1 + \beta_4 + \beta_7)$;
inflation = $(\beta_2 + \beta_5 + \beta_8)$

The results using whole-government vote (third set of columns, Table 1) are that none of the high-clarity variables – the dummy alone, or the two economic interactions – is significant. This suggests that the economic effects found are not significantly different in 'low-clarity' vs. 'high-clarity' countries. Repeating the procedure with PM's party vote as the dependent variable yields nearly identical results (not shown), except that the clarity–unemployment interaction is marginally significant ($b = -0.339$, $p = 0.08$).

Where do we now stand in terms of PW's hypothesis? The split-sample results shown in Table 1 do show significant economic effects only in high-clarity as opposed to low-clarity countries. However, a Chow test suggests that these apparent differences are not significant. Creating new variables using a high-clarity dummy reinforces this ambiguity, in that none of the clarity interactions are significant at 0.05, although the clarity–unemployment interaction is marginally significant with PM's party vote. Searching for a more definite test of the clarity-of-responsibility hypothesis, we turn how to a new measure of clarity.

TESTING THE MODEL USING ANOTHER INDICATOR OF 'CLARITY'

We have several reasons to question the validity and reliability of PW's measure of clarity of responsibility. First, and most important, this measure is constant for each country over time, and does not capture changes in responsibility which may be perceptible to voters. For example, in the PW model Denmark and Norway are always considered to be 'low-clarity' countries, even though for a combined eight election years (out of seventeen) for the two countries, there were *one-party* governments going into elections. Similarly, the United States is placed in the high-clarity category (in part because the majority party holds all committee chairs), even under divided government, which is often criticized for its lack of party accountability. Some studies have found that economic voting varies *within countries* depending on the degree to which an incumbent can be identified as responsible.[26]

[26] Anderson, *Blaming the Government*; Kevin M. Leyden and Stephen A. Borrelli, 'The Effect of State Economic Conditions on Gubernatorial Elections: Does Unified Government Make a Difference?' *Political Research Quarterly*, 48 (1994), 253–90.

Secondly, we are not convinced that voters are clearly aware of (or their vote influenced by) institutional features such as whether opposition parties share the right to chair committees or strength of party unity.[27] Finally, several components of the index involve placing countries into one of two categories, necessarily ignoring variation within categories, and requiring a certain amount of arbitrariness. All in all, given the dichotomous categorizations that must be made several times over in arriving at the final split sample, we are not confident that PW's index is truly capturing 'clarity of responsibility'.

We suggest that a more parsimonious way to measure 'clarity of responsibility' is simply to look at whether a government is a single-party or coalition government. There is higher 'clarity of responsibility' when one party is in control, and less clarity when there is coalition government. This indicator has a couple of advantages: first, it is clearly visible even to less-sophisticated voters. Secondly, it does not require that countries always be in one category or another: a given country may have one-party government at some elections, and coalition government at others. It seems reasonable to assume that voters would assess 'responsibility' differently in these different circumstances.[28]

Note that while PW's coding scheme did take into account number of parties, this was just one factor among five, and each country's *average* rating was used to put that country in only one category for the whole time period; we therefore do end up with a significantly different mix of cases depending on which 'clarity' indicator is used. While PW's 'high-clarity' sample is dominated by cases of single-party government, 24 per cent of cases are coalition government; similarly, 19 per cent of 'low-clarity' cases have single-party government.

Table 2 presents the results of dividing the sample into one-party governments and coalition governments (based on the government's composition at election time). Comparing one-party and coalition governments (the first and second sets of columns), what we see is completely contrary to the 'clarity of responsibility' hypothesis. *There are no significant economic effects whatsoever for one-party governments, while there are for coalition governments*. Moderate-to-left coalition governments tend to be hurt by unemployment ($b = -0.68$; $p < 0.000$). Such coalition governments, however, benefit from inflation ($b = 0.32$; $p = 0.04$). For coalition governments, the right-wing* unemployment interaction is once again positive and significant, and again nearly cancels out the negative coefficient for unemployment by itself, leaving little net effect from unemployment for right-wing government ($b = -0.68 + 0.67 = -0.01$). For coalition governments, then, economic effects are found mainly in moderate-to-left governments.

The fact that we have found strong economic effects in coalition government using vote for *all* governing parties tends to eliminate the rationale for also looking at vote for PM's party (the argument had been that economic voting could be 'hidden' in coalition governments, and this is apparently not the case). Nevertheless, for the sake

[27] Cross-national research of voter awareness/understanding of political institutions is surprisingly scarce, but results for the US case are not encouraging: for example, Stephen Earl Bennett and Linda L.M. Bennett, 'Out of Sight, Out of Mind: Americans' Knowledge of Party Control of the House of Representatives, 1960–1984', *Political Research Quarterly*, 47 (1993), 67–81, p. 76, find that 'people knew the GOP had won something in Congress in 1980 and 1984, but were unsure of which chamber it was'.

[28] This leaves us with the 'problem' of the United States, in that as long as there is 'united government', the United States goes in the same category as one-party parliamentary systems, which does not seem appropriate. However, this indicator at least puts the United States in different categories when there is 'united' vs. 'divided' government, as opposed to PW's measure, which permanently assigns the United States the 'high-clarity' category.

TABLE 2 *Testing a New 'Clarity of Responsibility' Measure: One-Party vs. Coalition Governments*

	One-party governments		Coalition governments				Full sample (all countries)	
	Dependent variable = Whole-government vote = PM's party vote		Dependent variable = Whole government vote		Dependent variable = PM's party vote		Dependent variable = Whole-government vote	
	B	S.E.	*B*	S.E	*B*	S.E.	*B*	S.E.
Unemployment	− 0.040	0.224	− 0.682***	0.161	− 0.620***	0.139	− 0.109	0.186
Inflation	0.051	0.141	0.320*	0.154	0.279*	0.121	0.130	0.129
Right-wing government	1.29	2.58	1.26	3.11	− 0.783	2.73	1.08	1.97
Right govt. *Unemployment	0.247	0.290	0.670*	0.289	0.780**	0.251	0.510**	0.203
Right govt. *Inflation.	− 0.252	0.219	− 0.445	0.360	− 0.498	0.320	− 0.371*	0.178
Minority government	3.02*	1.26	1.54	1.40	1.76^{+}	1.01	2.52*	0.921
Previous vote percentage	− 0.231*	0.096	− 0.019	0.050	− 0.027	0.043	− 0.055	0.042
Previous vote swing	− 0.181^{+}	0.102	− 0.174^{+}	0.102	− 0.089	0.115	− 0.225**	0.070
Constant	6.23	5.20	− 1.10	3.57	0.313	1.45	− 1.89	2.86
'Coalition' dummy	–	–	–	–	–	–	3.03^{+}	1.85
'Coalition'*unemployment	–	–	–	–	–	–	− 0.513**	0.201
'Coalition'*inflation	–	–	–	–	–	–	0.077	0.175
N	74		68		68		142	
R^2	0.340		0.346		0.355		0.298	
Adjusted R^2	0.259		0.257		0.267		0.239	

$^{+}p < 0.10$, two-tailed $*p < 0.05$, two-tailed $**p < 0.01$, two-tailed $***p < 0.001$, two-tailed

of comparison with Table 1, we also ran the Table 2 models with PM's party vote (in coalition governments). The results (third set of columns, Table 2) show that economic effects are nearly identical in coalition governments whether one looks at all governing parties or just the PM's party.

As with the previous models, we also applied a Chow test of differences for the split-sample regressions in Table 2. Comparing the one-party model with coalition governments using whole-government-vote produces the strongest F-statistic thus far ($F = 1.8$, $p = 0.07$). A test of differences between single-party governments and PM's party vote in coalition government produces a weaker F ($F = 1.56$, $p = 0.14$).

As one last test of coalition/one-party government differences, we can run a whole-sample regression with a dummy variable for 'coalition government', just as we did with PW's 'clarity' measure. The dummy variable is equal to 1 when a country has coalition government, and 0 otherwise, and we interact this with both economic variables. The model is exactly the same as Equation 2, except with 'coalition dummy' substituted for 'high-clarity' dummy; the coalition*economy interactions thus show the extra economic effect experienced by coalition governments. The fourth set of columns of Table 2 show that the coalition*unemployment interaction is significant ($b = -0.51$, $p = 0.01$). The results suggest that *moderate-to-left governments are only hurt by unemployment when they are coalition governments*. Running the same regression with PM's party vote (results not shown) also produces a significant result ($b = -0.4$, $p = 0.04$).

To summarize the results thus far, we have now investigated whether high and low clarity countries are different (as proposed by PW), and whether single-party and coalition governments are different (as we have suggested). We have found that while both of the subsets of cases *appear* different (looking at the split samples in Tables 1 and 2), there is some evidence that the single-party/coalition distinction is more important than is the high/low-clarity distinction. Chow test results show at least a marginally significant difference between single-party/coalition regressions ($p = 0.07$) with whole-government vote, while Chow tests are not significant for the high/low-clarity regressions. Similarly, turning to full-sample models with dummy interaction terms, the coalition–unemployment interaction is significant at 0.05 (with both whole-government and PM's party vote) while the clarity–unemployment interaction reaches only marginal significance, and only with PM's party vote.

Arguably, then, we have found that number of parties in government is more important for economic voting than is clarity of responsibility as defined by PW. However, our key finding is not just that number of parties in government is important, or that it is more important than PW's 'clarity' measure, but that its impact is *precisely the opposite of that predicted by the clarity of responsibility hypothesis*! That is, we find *more* economic effects for coalition than for single-party governments. We turn now to an analysis of these results.

ANALYSIS: WHAT DO THE RESULTS MEAN?

How can we explain the seemingly counter-intuitive finding that we sometimes find *more* economic voting when governments would appear to have *less* 'clarity of responsibility'? First, while it is frequently assumed that 'ability to place blame' is important for economic voting, the question of whether and how it figures into voter calculations is not one that has been addressed empirically, and so we know very little

about it.[29] The idea seems to stem primarily from the 'responsible parties' literature, which tends to focus on the alleged virtues of the Westminster, 'responsible parties' model as compared specifically to the US separation-of-powers system.[30] It is easy to see that under divided government in the United States, voters might indeed have difficulty deciding whom to hold responsible for policy outcomes.[31] Yet, one might argue that the United States is almost unique in its fragmentation of power and particularly low party cohesion. In coalition governments, parties frequently share power in a more co-operative manner than do parties in US divided government. Compared to the United States, it might be more difficult for coalition partners to blame each other – together, they form 'a government', while the same cannot be said of divided control in the United States. It could be argued that 'clarity of responsibility' may actually be reasonably high in coalition governments.

We thus suggest that it is not unreasonable or surprising to find economic effects in coalition governments. One could even argue that there may at times be reasons for stronger economic effects in coalition governments as compared to single-party governments, as our results suggest. First, single-party governments most frequently occur in countries that are essentially two-party systems (i.e., only two major parties have a real chance of gaining power), while coalition governments occur in multiparty systems. One might argue that the economy is more likely to influence a voter's choice when there are more viable alternatives to which he or she can turn. When there are fewer viable alternatives, some voters may avoid demonstrating their dissatisfaction with economic conditions and simply stick with the only choice they see as viable – the incumbent. For example, Table 2 shows that moderate-to-left *coalition* governments are punished for unemployment, while moderate-to-left *single-party* governments are not. If we assume that a voter under a single-party government is in a two-party system, then he or she may be unwilling to punish the incumbent moderate-to-left party for mishandling unemployment because the only alternative party (probably a right-wing party) would be even worse for unemployment! But in a multi-party system there is more likely to be a non-incumbent, anti-unemployment alternative to which voters can turn.

The coalition and single-party government samples differ in more ways than their party systems, however. The coalition government sample is dominated by continental Western European countries, while the single-party government sample is dominated by Anglo-American countries and Japan. The Western European group generally has stronger labour influence (as shown by higher unionization rates), and higher voter turnout, as compared to the 'Anglo-American-Japan' group. It seems reasonable to hypothesize that all of these variables – number of parties, unionization and turnout – might have an impact on economic voting.

Thus, we have suggested that there are a number of plausible reasons for finding more economic voting in coalition as compared to single-party governments. The fact remains, however, that Table 1 shows strong economic voting in high-clarity countries and none in low-clarity ones, even though coalition governments are more concentrated in the latter. How can the results of Tables 1 and 2 be reconciled?

Apparently, there is strong economic voting in some countries and not in others, and

[29] In addition to PW, others who make the argument that ability to place blame may affect economic voting are Anderson, *Blaming the Government*, and Lewis-Beck, *Economics and Elections*.

[30] See, for example, Committee on Political Parties, American Political Science Association, *Toward a More Responsible Two-Party System* (New York: Rinehart, 1950).

[31] Some evidence from the US state level does suggest that divided government tends to impede economic voting (Leyden and Borrelli, 'The Effect of State Economic Conditions on Gubernatorial Elections').

we have seen that dividing the sample in two different ways (Table 1 versus Table 2) produces results where one sample has many economic effects and the other has none. We have seen that both a high-clarity and a coalition government sample produce strong economic effects. The question is, why is this the case? There are a number of possible answers.

(1) If one argues that PW's definition of 'clarity of responsibility' is superior to simple 'number of parties in government', one could make the case that the results for coalition governments are a fluke, driven by the inclusion in that sample of some 'high clarity of responsibility' cases with particularly strong results.
(2) If our earlier speculation is correct, and something about coalition governments produces more economic voting, then we could argue that it is PW's results that are a fluke: there is strong economic voting in the high-clarity sample because of the inclusion of some cases of coalition government in that sample.
(3) Finally, it could be the case that the results support neither PW's 'clarity of responsibility' hypothesis nor the alternative hypothesis we have outlined above. It could be that we find economic effects in both the high-clarity and coalition government samples because there are particularly strong effects in cases *common* to both samples, for reasons having nothing to do with either 'clarity of responsibility' or having coalition government. In other words, these countries may simply be outliers, or there may be some other theoretical reason for strong economic voting there. If this is the case, then elimination of these cases from both the high-clarity and coalition government samples should result in no economic voting in either sample.

There are twenty cases – 24.1 per cent of the high clarity sample and 29.4 per cent of the coalition government sample – that are classified both as high-clarity countries, and as instances of coalition government. Elimination of these cases and re-running high-clarity and coalition regressions hold the key to testing the alternatives presented here.

When we eliminate cases of coalition government from the high-clarity sample, we are left only with single-party high-clarity cases ($n = 63$). According to PW's argument, these should be cases of *particularly high* clarity of responsibility – i.e., these are countries that have institutional features that tend to concentrate responsibility, *plus* we are including only those cases with single-party governments going into an election, further concentrating responsibility. What happens when we re-run the model? It turns out (results not shown) that none of the economic variables are significant at 0.05; only one is marginally significant (right-wing inflation, $b = -0.35$, $p = 0.09$). (Note that for these cases, whole-government vote is equal to PM's party vote and so there is only one regression to look at.) This is quite different from the results found in Table 1 for high-clarity countries.

When we eliminate high-clarity cases from our coalition government sample, we are left only with low-clarity coalition government cases ($n = 48$). Again by PW's logic, these should be cases of *particularly low* clarity of responsibility – i.e., these are countries with institutional features that blur clarity of responsibility, plus we are including only specific cases where these countries have coalition government going into elections, further blurring responsibility. The results of re-running the model are that both moderate-to-left unemployment ($b = -0.51$, $p = 0.02$) and right-wing-unemployment ($b = 1.047$, $p = 0.008$) are significant. (These results are with whole government-vote; the results with PM's party are nearly identical – i.e., unemployment

has $b = -0.38$, $p = 0.03$; right-wing unemployment has $b = 0.95$, $p = 0.007$). In other words, the results are similar to those shown in Table 2; although we no longer have an inflation effect for moderate-to-left governments, we have both unemployment effects and there is actually a stronger net effect of unemployment on right-wing governments.

Where does all this leave us? We have found that elimination of the cases common to both the high clarity and coalition government samples results in a high-clarity sample with few or no economic effects, and a coalition-government sample with continued effects. This strengthens our case against the 'clarity of responsibility' hypothesis, and strengthens our argument that *something* about countries with coalition government produces strong economic voting.

CONCLUSIONS

We have two conclusions. First, we find that using whole-government vote versus vote for PM's party as dependent variables can occasionally produce somewhat different results (as in Table 1), suggesting that scholars of economic voting would be well advised to look at both variables when testing hypotheses. Our key conclusion, though, is that we find no evidence that 'clarity of responsibility' is necessary for economic voting. When we use a measure of 'clarity of responsibility' that is different from PW's – simply number of parties in government – we find that there are significantly different economic effects in single-party versus coalition governments, and the differences are the opposite of what the 'clarity' hypothesis predicts. This suggests that something *other than* 'ability to place blame' causes voters to behave differently when there are coalition versus single-party governments. We have speculated that this result has something to do with the characteristics of the countries that usually have each type of government, for example, the number of viable alternative parties from which voters have to choose.

Paldam suggests that a combination of significant findings and instability in economic voting studies leads to the temptation to believe that 'we are just missing one little trick that would make the function stable'.[32] PW propose, essentially, that controlling for 'clarity of responsibility' may be one of those 'little tricks'. Our tests do not support this; they do suggest, however, that 'political context' defined differently may indeed matter for economic voting.

APPENDIX: SOURCES FOR DATA

We are very grateful to G. Bingham Powell Jr and Guy D. Whitten for sending us the political dataset created for their 1993 *American Journal of Political Science* article. Many, but not all, of the political variables we use come from that dataset; in particular, we relied on their judgements for number of parties in government and government ideology. For cases that were missing from the dataset, we relied on the election results presented annually in the *European Journal of Political Research (EJPR)*, and followed PW's practice of using ideology scores from Castles and Mair.[33] The PW data did not include information on vote for prime minister's party; to obtain that data for early elections we used the same sources used by PW for government as a whole. Voting figures came from Mackie and Rose.[34] More recent voting data came from *Keesing's Record of World Events*. Data on ideology of PM's party came from Castles and

[32] Paldam, 'How Robust is the Vote Function?' p. 29.

[33] Francis G. Castles and Peter Mair, 'Left-Right Political Scales: Some Expert Judgments'. *European Journal of Political Research*, 12 (1984), 83–8.

[34] Thomas T. Mackie and Richard Rose, *The International Almanac of Electoral History* (London: Macmillan, 1991).

TABLE A1 *Replicating Powell and Whitten (1969–88)*

	Dependent variable = Change in whole-government vote											
	'Comparative' Economic Measures, PW Specification				Non-Comparative Economic Measures (see text), PW Specification				Non-Comparative Economic Measures, New Specification			
	High Clarity		Low Clarity		High Clarity		Low Clarity		High Clarity		Low Clarity	
	B	S.E.	*B*	S.E.	*B*	S.E.	*B*	S.E.	*B*	S.E.	*B*	S.E.
Growth	0.41^{+}	0.23	0.06	0.25	0.19	0.21	−0.11	0.29	0.20	0.21	−0.15	0.29
Unemployment	0.45**	0.17	−0.52	0.34	−0.51***	0.14	−0.15	0.30	−0.53***	0.15	−0.21	0.30
Inflation	0.14	0.17	−0.37	0.29	0.19^{+}	0.11	−0.05	0.27	0.17	0.12	−0.11	0.27
Right Government	–	–	–	–	–	–	–	–	−0.73	2.03	−6.94	5.27
Right Gov.*Unemployment	0.51^{+}	0.29	0.95	0.71	0.74***	0.20	0.28	0.38	0.78**	0.24	0.79	0.54
Right Gov.*Inflation	−0.37	0.27	−0.25	0.71	−0.41**	0.14	0.18	0.46	−0.37*	0.18	0.45	0.50
Minority Government	2.66*	1.07	1.16	2.39	2.54*	1.04	1.28	2.44	2.56*	1.05	0.89	2.43
Previous Vote Percentage	0.08	0.08	−0.18*	0.08	0.05	0.08	−0.10	0.09	0.05	0.08	−0.11	0.09
Previous Vote Swing	−0.19*	0.09	−0.35^{+}	0.18	−0.18*	0.08	−0.34^{+}	0.19	−0.17*	0.09	−0.26	0.19
Constant	−7.19^{+}	3.82	6.65	4.76	−5.99	4.04	3.90	6.72	−5.78	4.12	5.71	6.79
N	61		43		61		43		61		43	
R^2	0.31		0.32		0.39		0.27		0.39		0.31	
Adjusted R^2	0.20		0.16		0.30		0.10		0.29		0.12	

$^{+}p < 0.10$, two-tailed; $*p < 0.05$, two-tailed; $**p < 0.01$, two-tailed; $***p < 0.001$, two-tailed

Mair. For the minority government dummy, we relied mostly on PW; in a few cases where there was a disagreement between their determination and Woldendorp *et al.*, we relied on Strom or made our own determination.[35]

Our unemployment data were obtained from the *Bulletin of Labour Statistics*, by the International Labour Organization, Geneva. Inflation figures came from the IMF's *International Financial Statistics*.

[35] Kaare Strom, *Minority Government and Majority Rule* (Cambridge: Cambridge University Press, 1990); Jaap Woldendorp, Hans Keman and Ian Budge, 'Political Data 1945–1990', *European Journal of Political Research*, 24 (1993), 1–120.

The Effect of Referendums on Democratic Citizens: Information, Politicization, Efficacy and Tolerance

MATTHEW MENDELSOHN AND FRED CUTLER*

Government-sponsored referendums on issues of national importance are occurring with greater frequency in countries with only sporadic experience with direct democracy. Comprehensive studies exist which examine the origins, conduct and regulation of referendums, as well as their consequences for the political system.[1] There have also been a large number of studies addressing voting behaviour during particular campaigns,[2] and a great deal of research on the far more elaborate and systematized processes in those countries, notably the United States and Switzerland, with recognized initiative mechanisms for citizens to pose referendum questions.[3] Yet no empirical study has attempted to answer the question of how government-sponsored referendum campaigns in countries with little history of direct democracy affect citizens' democratic comportment more generally.

This is not to say that the literature is not teeming with arguments concerning the

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[1] D. Butler and A. Ranney, *Referendums: A Comparative Study of Practice and Theory* (Cambridge: Cambridge University Press, 1980); D. Butler and A. Ranney, *Referendums Revisited* (Washington, DC: American Enterprise Institute, 1995).

[2] P. Collas, 'Consultations populaires et dernier référendum', *Revue Politique et Parlementaire*, 94/961 (1992), 29–43; B. Criddle, 'The French Referendum on the Maastricht Treaty, September 1992', *Parliamentary Affairs*, 46 (1993), 228–38; N. Peterson and J. Elklit, 'Denmark: Denmark Enters the European Communities', *Scandinavian Political Studies*, 8 (1973), 198–213; B. Galligan, 'The 1988 Referendums and Australia's Record on Constitutional Change', *Parliamentary Affairs*, 43 (1990), 497–506; R. Pierce, H. Valen and O. Listhaug, 'Referendum Voting Behavior: The Norwegian and British Referenda on Membership in the European Community', *American Journal of Political Science*, 27 (1983), 43–63; D. Granberg and S. Holmberg, 'Preference, Expectations, and Voting in Sweden's Referendum on Nuclear Power', *Social Science Quarterly*, 67 (1986), 379–92; P. Hansen, M. Small and K. Siune, 'The Structure of the Debate in the Danish EC Campaign: A Study of an Opinion-Policy Relationship', *Journal of Common Market Studies*, 15 (1975), 93–129; K. Siune and P. Svensson, 'The Danes and the Maastricht Treaty: The Danish EC Referendum of June 1992', *Electoral Studies*, 12 (1993), 99–111; A. Pelinka, 'The Nuclear Power Referendum in Austria', *Electoral Studies*, 2 (1983), 253–61.

[3] K. W. Kobach, *The Referendum: Direct Democracy in Switzerland* (Aldershot, Hants.: Dartmouth University Press, 1993); W. Linder, *Swiss Democracy: Possible Solutions to Conflict in Multicultural Societies* (New York: St Martin's Press, 1994); D. Magleby, *Direct Legislation: Voting on Ballot Propositions in the United States* (Baltimore, Md: Johns Hopkins University Press, 1984); T. E. Cronin, *Direct Democracy: The Politics of Initiative, Referendum and Recall* (Cambridge: Cambridge University Press, 1989). Increasingly, Italy can be grouped with Switzerland and the US states as exceptional in its frequent use of initiatives.

supposedly meritorious or nefarious effects of referendums on citizens. It is customary for books on direct democracy to include a review of these arguments.[4] Proponents contend that referendum campaigns can increase politicization, political knowledge and efficacy,[5] addressing, at least in a small way, the 'democratic deficit'. On the other side, some worry that referendums might bring out intolerance in mass publics and undermine minority rights.[6] These claims, all more or less plausible, are based largely on speculation and have not been subject to empirical investigation. As Budge has remarked, while arguments for and against direct democracy are advanced rather casually, 'little attention has been given to how citizens actually behave when they are consulted'.[7]

Using the Canadian Election Study (CES), we present evidence that speaks to these claims. The CES employed a rolling cross-section methodology and was in the field during the 1992 referendum on the Charlottetown Constitutional Accord. This is the first and only rolling cross-section to be in the field during a referendum campaign. It therefore provides an unparalleled opportunity to examine the dynamics of citizen response to national referendum campaigns. We are not interested in the 'determinants of the vote', a question which has been addressed elsewhere.[8] Rather, we are interested in four broader questions relevant to democratic theory:

1. '*Do referendums increase political knowledge?*' Some have argued that popular consultations have a pedagogic function. Barber, for instance, argues that the 'referendum can … provide a permanent instrument of civic education'.[9] A small number of empirical studies have concluded that voters do learn as campaigns progress, particularly if the government adopts a pedagogic rather than a propagandistic approach.[10] The quantitative evidence, however, is weak,[11] with some scholars simply asserting impressionistically that knowledge increases during campaigns.[12] Moreover, even if there is a general increase in knowledge, it is uncertain whether the campaign induces the habitually inattentive to sit up and pay attention, thereby reducing the 'information gap'.[13]
2. '*Do referendums increase politicization?*' Many proponents of direct democracy suggest that representative institutions, by their nature, depoliticize the average citizen; direct democracy, by contrast, would increase citizens' interest in politics and thus stimulate popular participation.[14] Yet the only evidence for this is that turnout and interest during campaigns tends to be high, which may tell us only that

[4] Cronin, *Direct Democracy*; Butler and Ranney, *Referendums*, chap. 2; I. Budge, *The New Challenge of Direct Democracy* (Cambridge: Polity Press, 1996), chap. 3.

[5] I. Bohnet and B. S. Frey, 'Direct Democratic Rules: The Role of Discussion', *Kyklos*, 47 (1994), 341–54.

[6] G. Sartori, *The Theory of Democracy Revisited* (Chatham, NJ: Chatham House, 1987).

[7] Budge, *The New Challenge*, p. 33.

[8] R. Johnston, A. Blais, N. Nevitte and E. Gidengil, *The Challenge of Direct Democracy* (Montreal and Kingston: McGill–Queen's University Press, 1996); L. LeDuc and J. H. Pammett, 'Referendum Voting: Attitudes and Behaviour in the 1992 Constitutional Referendum', *Canadian Journal of Political Science*, 28 (1995), 3–33.

[9] B. Barber, *Strong Democracy: Participatory Politics for a New Age* (Berkeley: University of California Press, 1984), p. 284.

[10] Peterson and Elklit, 'Denmark'; Hansen, Small and Siune, 'The Structure of the Debate in the Danish EC Campaign'.

[11] Collas, 'Consultations popularies et dernier référendum'; N. F. Christiansen, 'The Danish "No" to Maastricht', *New Left Review*, 195 (1992), 97–101; Siune and Svensson, 'The Danes and the Masstricht Treaty'.

[12] Bohnet and Frey, 'Direct Democratic Rules'.

[13] On the 'information gap', see R. Neuman, *The Paradox of Mass Politics: Knowledge and Opinion in the American Electorate* (Cambridge, Mass.: Harvard University Press, 1986).

[14] Budge, *The New Challenge*, p. 69.

referendums are usually held on issues of high salience, not that referendum campaigns actually politicize citizens.

3. '*Do referendums promote political efficacy?*' Some suggest that elections are a weak form of democratic control and citizens are alienated due to a lack of visible and direct influence on political outcomes.[15] Referendums, by some accounts, would therefore increase political efficacy by offering citizens a direct say in policy making. However, a variety of alternative explanations for alienation are possible and no study has been conducted on the question.
4. '*Do referendums encourage political intolerance?*' The most common criticism of referendums is that they provoke the polarization of political discourse, usually on symbolic or emotional issues, and hence activate authoritarian tendencies, tribal loyalties and a vulgar majoritarianism.[16] However, there is little evidence to support this concern in relation to national referendums in established democracies. Evidence of intolerance has turned up when studying American state initiatives[17] – which is not surprising since many are explicitly directed towards questions dealing with minority rights – or during times of great upheaval, such as the spate of consultations in Eastern Europe following the fall of the Soviet Union.[18] Yet it is doubtful whether these consultations tell us much about how citizens behave during national referendums in established democracies. In fact, Svensson argues that referendums in Denmark help protect the disenfranchised and generally powerless.[19]

Scholars have lamented the patchy state of our knowledge[20] and a body of understanding is growing that allows for theory-building on the determinants of referendum voting.[21] However, on the four questions addressed in this Note[22] political science has offered

[15] C. B. Macpherson, *The Life and Times of Liberal Democracy* (New York: Oxford University Press, 1997), pp. 88–94.

[16] Sartori, *The Theory of Democracy Revisited*; though some scholars have argued the opposite, suggesting that direct democracy could help citizens understand the concerns of others. See Barber, *Strong Democracy*; Budge, *The New Challenge*.

[17] B. Gamble, 'Putting Civil Rights to a Popular Vote', *American Journal of Political Science*, 41 (1997), 245–69: *contra* Cronin, *Direct Democracy*, pp. 197–8.

[18] H. Brady and C. Kaplan, 'Eastern Europe and the Former Soviet Union', in Butler and Ranney, *Referendums Revisited*, pp. 174–217.

[19] P. Svensson, 'Denmark: The Referendum as Minority Protection', in M. Gallagher and P. V. Uleri, *The Referendum Experience in Europe* (London: Macmillan, 1996), pp. 33–51.

[20] P. Svensson, 'Class, Party and Ideology: A Danish Case Study of Electoral Behaviour in Referendums', *Scandinavian Political Studies*, 7 (1984), 175–96, at p. 176, S. S. Nilson and T. Bjorklund, 'Ideal Types of Referendum Behaviour', *Scandinavian Political Studies*, 9 (1986), 265–78; Morel writes: 'Même si l'actualité fournit parfois l'occasion d'effectuer des bilans ou synthèses, elle tend plutôt à susciter des traveaux parcellaires, déterminés par des orientations contingentes et manquant à la fois de recul et d'unité méthodologique' (L. Morel, 'Le référendum: l'état des recherches', *Revue Française de Science Politique*, 42 (1992), 835–64).

[21] For example, see R. Pierce *et al.*, 'Referendum Voting Behavior'; Svensson, 'Class, Party, and Ideology'; M. Franklin, M. Marsh and L. McLaren, 'Uncorking the Bottle: Popular Opposition to European Unification in the Wake of Maastricht', *Journal of Common Market Studies*, 32 (1994), 455–72, p. 470; H. D. Clarke and A. Kornberg, 'The Politics and Economics of Constitutional Choice: Voting in Canada's 1992 National Referendum', *Journal of Politics*, 56 (1994), 940–62; O. Tonsgaard, 'A Theoretical Model of Referendum Behaviour', in P. Gundelach and K. Siune, eds, *From Voters to Participants: Essays in Honour of Ole Borre* (Århus: Politica, 1992), pp. 132–47; K. Siune, P. Svensson and O. Tonsgaard, 'The European Union: The Danes Said "No" in 1992 but "Yes" in 1993: How and Why?' *Electoral Studies*, 13 (1994), 107–16.

[22] It should be noted that the first two of these variables are campaign specific questions – knowledge and interest related to the campaign – while the latter two questions speak to issues not directly related to the referendum – efficacy and tolerance more generally. However, all four can be thought of as relating to the effect of referendums on citizens' democratic comportment, regardless of whether this is campaign specific – learning – or general – tolerance of others.

offered mostly speculation.[23] Answers to these questions of course could be affected by the idiosyncrasies of a given campaign. However, while it is true that 'each campaign is different', political scientists have none the less built a substantial body of knowledge concerning electoral behaviour across time and space and there is no compelling reason why the same cannot be accomplished with respect to referendum behaviour.[24] By identifying the Canadian experience in 1992, we generate re-testable hypotheses that may be applied to future referendum campaigns.

DATA AND METHODS

We draw on the 1992 CES data. The CES employed a rolling cross-section and interviewed about eighty different respondents each of the thirty-one days of the official campaign period. Overall, 2,530 respondents were interviewed. A rolling cross-section is primarily concerned with the dynamics of opinion evolution, as each day of the campaign provides its own mini sample. Accordingly, the key independent variable in our analysis, *Date of Interview*, measures the effect of the campaign because it takes on a higher value (1) for those interviewed at the end of the campaign than for those interviewed at the beginning (0). Because the date on which a respondent was interviewed is independent of any other characteristics of the respondent, there is usually no need to control for other determinants of the dependent variables.[25] Our primary expository technique is therefore to present graphs with the mean values of the dependent variables on the *y*-axis and the date of the campaign on the *x*-axis, though these graphs are accompanied by various regression-type methods to make statistical inferences and to examine more complicated questions.[26]

[23] In perhaps the most comprehensive recent treatment of the referendum across Europe, significant attention is devoted to voting behaviour in a variety of countries, but little to the effect of referendums on citizens' capacity to act as democratic actors (Gallagher and Uleri, *The Referendum Experience*).

[24] Political scientists have also built up substantial knowledge about voter behaviour during citizen sponsored initiatives (see Magleby, *Direct Legislation*; Cronin, *Direct Democracy*; Kobach, *The Referendum*; and Linder, *Swiss Democracy*). However, it would be a mistake to presume that this literature is relevant for the questions addressed in this article. Some scholars have used a casual mix of American and Swiss findings on the initiative to comment on European government sponsored referendums (Morel, 'Le référendum'), or have generalized from the US, Swiss and Italian experiences to comment more broadly on direct democracy (Budge, *The New Challenge*, chap. 3). However, if we take seriously the claim that 'institutions matter', then it should be clear that different kinds of consultations will lead to different effects. The fact that Swiss and American initiatives regularly ask questions of low salience and see participation rates around 30 per cent are but two facts that should remind us to distinguish clearly government-sponsored referendums on issues of national importance from initiative campaigns on ordinary policy issues. Both Butler and Ranney, *Referendums Revisited*, pp. 221–6, and Gallagher and Uleri, *The Referendum Experience*, p. 9, concur with this admonition.

[25] It is important to keep in mind that our interest is in identifying the effect of the campaign (as measured by *Date*) on the dependent variables, not in formally modelling the dependent variables. We are therefore interested only in controlling on the right-hand side of the equation variables that could be correlated with both *Date* and the dependent variables, not all conceivable variables that would allow us to explain more of the variance in the dependent variable. For this kind of analysis, what is crucial is that the coefficent for *Date* remains stable regardless of the specification, and this is the case. Earlier 'kitchen sink' models, with a variety of different control variables added, were presented in an earlier version of this article (Mendelsohn, 'Direct Democracy and Political Behaviour', paper presented to annual meeting of the Canadian Political Science Association, 1994). However, it is important to ensure that *Date* is not correlated with other variables, as it is possible that the grounds on which people self-selected themselves into or out of the sample changed as the campaign progressed. No socio-demographic variable was correlated with *Date*, other than Catholicism, and then only slightly. The inclusion of a Catholic dummy variable – like the inclusion of a variety of socio-demographic variables – did not affect the *Date* and *Media* coefficients in any equation.

[26] All variable definitions may be accessed on the internet version of this article in Appendix A.

The 1992 referendum was held on the constitutional reforms known as the Charlottetown Accord. Canada had been in a profound constitutional crisis since 1990, when the Meech Lake Accord, which had been designed to respond to the concerns of the French-speaking province of Quebec, failed to secure the necessary support. The Meech Lake Accord was rejected because many citizens believed the process of negotiation was too elite-driven and closed to the public, and because public opinion in English-speaking Canada found the Accord to be too generous towards Quebec. The Charlottetown Accord emerged through a more open process of public consultation and attempted to find a compromise between Quebec's minimum demands and pressures from elsewhere in the country. This national referendum, the first in a half-century, had the potential to resolve Canada's most divisive political issue, the place of Quebec in Canada. Instead, it went down to defeat, receiving only 45 per cent of the national vote, including just 43 per cent in Quebec.

RESULTS

'Do referendums increase political knowledge?'

The dependent variable is an index of political knowledge constructed from questions asking where prominent intervenors stood on the Accord, and runs from 0 to 5 correct attributions. Although this measure does not include factual knowledge about the Accord and is therefore a somewhat limited form of political information, it proxies quite well for more substantive knowledge.[27] Furthermore, the post-referendum survey shows that 63 per cent of the electorate could name one or none of the proposals in the Accord, implying that many needed guidance from intervenors to make up for their shortfall of substantive information.[28]

Figure 1 plots trend lines (five-day moving averages) of two variables: objective knowledge and self-reported news exposure. Both clearly increased over the course of the campaign. The *prima facie* evidence is that there was political learning, but only a very modest amount: from an average of two intervenors correctly attributed at the beginning of the campaign to 2.5 at the end. Whether this constitutes impressive learning or not is a matter of interpretation, but it is clear that the campaign did not transform an otherwise poorly-informed citizenry into a community of political junkies.

Other questions arise. First, what explains the increase in knowledge: do citizens seek out information through paying closer attention to the media, or is there simply more information available? Figure 1 indicates that small increases in reported media use parallel the increase in knowledge, yet this graphical depiction hardly constitutes evidence of a causal relationship. To assess the degree to which the effect of date on knowledge comes through increased attention to the media, a two-equation structural model is necessary. In the first equation, media use is regressed on date of the campaign and a number of plausible socio-demographic and attitudinal determinants of media use during the 1992 referendum. In the information equation, the media-use variable is thus a predicted value determined by the first equation, so that the effect of date on media

[27] For more justification of this approach, see Johnston *et al.*, *The Challenge of Direct Democracy*, chap. 5. Our own predictive equation (ordered logit) of knowledge of substantive proposals (asked during the post-referendum wave, running 0–3 correct answers), using only knowledge of intervenors during the campaign wave, generates the following predictions: 0 intervenors = 0.41; 1 = 0.82; 2 = 1.22; 3 = 1.63; 4 = 2.04; 5 = 2.45.

[28] This is consistent with the logic of A. Lupia, 'Shortcuts Versus Encyclopedias: Information and Voting Behavior on California Insurance Reform Elections', *American Political Science Review*, 88 (1994), 63–76.

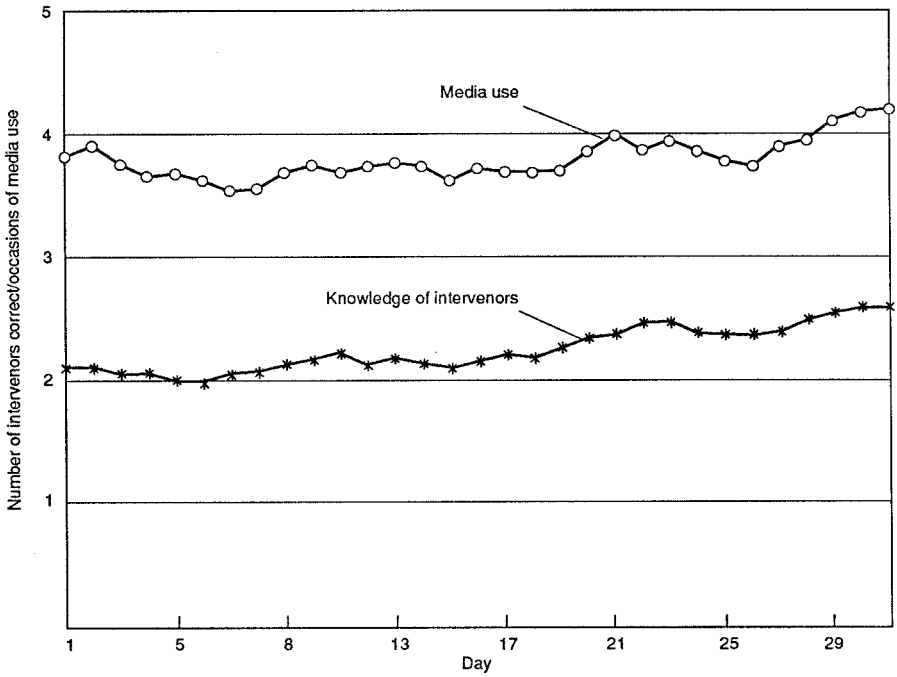

Fig. 1. Information and media use – five-day moving averages

use is captured in the media coefficient. Any remaining effects of the progress of the campaign (i.e. those not related to individuals' increased media consumption) will be estimated by the coefficient on date. Crucially, we control for the respondent's self-reported interest in the campaign and the prestige of their occupation, which together are likely to capture most other – mainly interpersonal – influences on a person's level of campaign-related knowledge. The structure of the model, estimated using two-stage least squares[29] is therefore:

$$\text{MEDIA USE} = \beta_0 + \beta_1\, date + \beta_2\, interest + \beta_3\, young + \beta_4\, old + \beta_5\, income + \beta_6\, education + \beta_7\, employed + \beta_8\, linguistic\ minority + \beta_9\, female + \beta_{10}\, born\ in\ Canada + \beta_{11}\, PID + \beta_{12}\, no\ say\ in\ gov't + \beta_{13}\, dissatisfied\ with\ process + \beta_{14}\, worse\ off\ if\ Quebec\ separates + \beta_{15}\, francophone + \varepsilon_1 \quad (1)$$

$$\text{INFORMATION} = \beta_0 + \beta_1\, date + \beta_2\, \text{MEDIA USE} + \beta_3\, interest + \beta_4\, occupational\ prestige + \varepsilon_2 \quad (2)$$

The results are presented in Table 1. First, the simple linear regression in Model 1 provides individual-level confirmation that the campaign increased knowledge by about

[29] We tested for non-linearities in these two equations using a quadratic term (date^2). It does appear that most of the media increase came in the second half of the campaign, though the data are not rich enough to confirm this story with a great deal of confidence.

one-half intervenor per person ($b = 0.46$).[30] Model 2 then introduces media consumption as a control, and the effect of the campaign on learning is reduced, though the strong independent effect of the campaign on knowledge remains.[31] However, this 'control' for media use does not account for the possible effect of an increase in media use as the campaign progresses.

Testing the mechanism for increases in knowledge comes in Model 3. In the media use equation, the coefficient estimate of 0.74 on *date* indicates that from the beginning to the end of the campaign the increase in media consumption was about three-quarters of a day per week.[32] In the information equation, the coefficient on media use is of moderate strength and very precise, while the effect of *date* is reduced substantially and is of marginal statistical significance. Even though the media measure is quite crude, the coefficient indicates that among those who read a newspaper or watched the news one more day per week, about one in three would place one extra intervenor correctly ($b = 0.34$). This suggests that much of the campaign-induced learning derives from increases in media consumption. We therefore find confirmation that as the campaign unfolds, people pay slightly more attention to the news and learn as a result, though if media use were more accurately measured, even more of the effect of the campaign might be channelled through this variable.

Although we now know that the 1992 campaign produced a modest amount of learning, an equally important question arises: does the (albeit modest) learning take place among those who need it most? If referendums are merely adding information to the stocks of the well informed, one cannot speak of referendums as democratizing the flow of political information. To identify who is learning, a measure of general political knowledge must be found. Although we do not have access to a measure of political knowledge prior to the campaign, we do have the next best thing: a measure of factual information for the 65 per cent of respondents reinterviewed during the 1993 CES election wave thirteen months after the referendum. This measure is a very good proxy for a respondent's level of general political information prior to the referendum campaign,[33] since the questions are unrelated to the referendum or to constitutional issues. This variable defines four groups by the number of correct answers to three questions (0–3 facts correct). We estimate four separate Tobit models of the effect of date on knowledge, including a quadratic term to allow for non-linearities. Tobit allows us to control for the truncation of the dependant variable, so as to avoid missing any learning taking place among the already highly informed. The estimates are best

[30] We also estimated a Tobit model to account for the truncation of the dependent variable at five correct intervenors. Tobit is necessary because we might otherwise find no learning for the already well-informed because they start at so high a level and have nowhere to go on the five-question measure (see W. Greene, *Econometric Analysis*, 2nd edn (New York: Macmillan, 1993), pp. 694–6). The estimation produced a statistically indistinguishable coefficient ($b = 0.51$).

[31] Controlling for both interest and occupational prestige in Model 2 does not affect the conclusions, though the coefficients are slightly suppressed, as we would expect: *date*: 0.30 (0.12), *media*: 0.12 (0.01)

[32] This is a significant increase given how poor the measurement of attentiveness to the political media is. The measure is an inevitably error-laden *recall* of *frequency* of news-watching and newspaper-reading. It does not measure *attention* or, better still, actual *reception* of information. Moreover, since people consume news products for more than political news, the measure is even poorer. Nevertheless, the very strong and precise effects of the media measure on information (those at the 90th percentile are two points higher on the six-point information scale than those at the 10th percentile) suggests that it is a valid indicator of attention to the richest source of information about the campaign.

[33] A justification of this approach can be found in R. Johnston, A. Blais, H. Brady and J. Crête, *Letting the People Decide* (Montreal: McGill–Queen's University Press, 1992), pp. 264–5.

TABLE 1 *Estimates of the Effect of the Campaign on Political Knowledge*†

	Model 1 (OLS)	*Model 2 (OLS)*	*Model 3 (TSLS)*	
	Information (0–5)	Information (0–5)	Media Use (0–14)	Information (0–5)
Constant (1)	2.03*	0.91*	5.56*	− 1.52*
	(0.08)	(0.09)	(0.51)	(0.17)
Date (0–1)	0.46*	0.34*	0.74*	0.20
	(0.13)	(0.12)	(0.29)	(0.13)
Media Use (1–14)	–	0.15*	–	0.34*
		(0.01)		(0.02)
Interest (0–1)	–	–	2.61*	0.62*
			(0.30)	(0.15)
Occupational prestige (20–104)	–	–	–	0.017*
				(0.003)
Young (age $< 30 = 1$)	–	–	− 2.70*	–
			(0.21)	
Old ($> 60 = 1$)	–	–	1.73*	–
			(0.62)	
Income (0–1)	–	–	2.40*	–
			(0.39)	
Education (0–1)	–	–	0.21*	–
			(0.05)	
Employed (0,1)	–	–	− 0.86*	–
			(0.34)	
Female (0,1)	–	–	− 0.96*	–
			(0.18)	
Has PID (0,1)	–	–	0.50*	–
			(0.18)	
Dissatisfied with process (0,1)	–	–	0.69*	–
			(0.18)	
No say in gov't (0,1)	–	–	− 0.41*	–
			(0.20)	
Worse off if Quebec separates (0,1)	–	–	0.09	–
			(0.19)	
Francophone (0,1)	–	–	0.35	–
			(0.20)	
Linguistic minority (0,1)	–	–	− 0.08	–
			(0.33)	
Std. err. of estimate			4.00	1.65
R^2 (OLS), F (TSLS)	0.007	0.179	$33.52_{2,115}$	$160.64_{2,126}$
N	2,433	2,433	2,130	2,130

† Estimates with standard errors in parentheses below.
* Indicates $p > 0.05$.

summarized with predicted values graphed in Figure 2. The best informed 15 per cent of the population show little evidence of learning over the course of the campaign. The biggest increase comes for the next-best informed group, learning the correct position

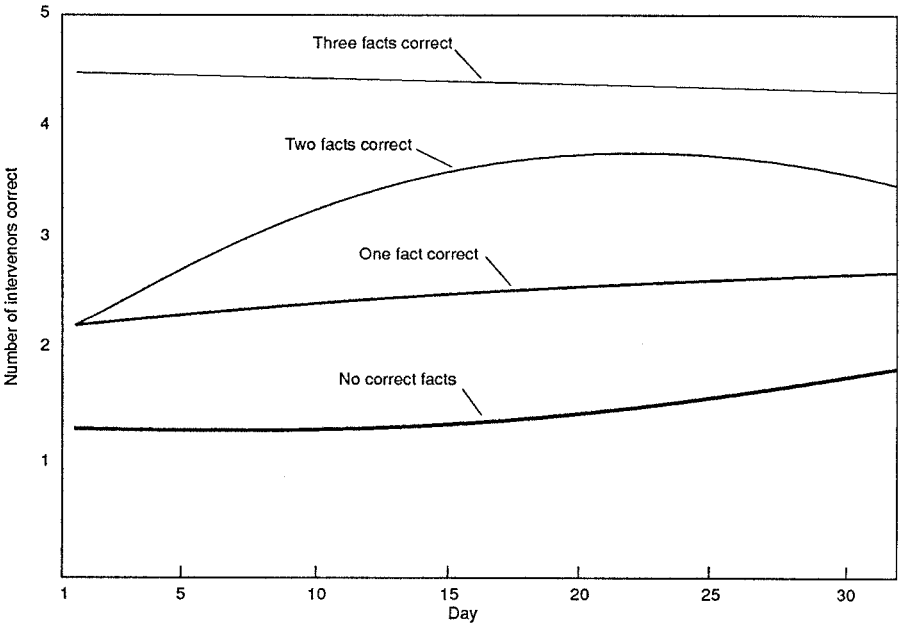

Fig. 2. Learning by general level of knowledge
Note: The predicted values shown here are a summary of the estimates that may be found in Appendix B of the internet version of this article.

of one full intervenor (1.14), or nearly double the average learning for all citizens. For the two bottom groups, there is also evidence of learning, but information gains tend to be smaller or come later in the campaign.[34] These estimates indicate that the campaign reduced the gap between the generally informed and poorly informed, with approximately a quarter of the measured information gap disappearing.[35]

'Do referendums increase politicization?'

If so, is increased interest the beginning of a causal chain that leads to the increased media consumption and knowledge that we saw above? Figure 3 plots the moving average of the interest variable. There is some fluctuation, but no systematic change in self-reported levels of interest as the campaign progressed. We specified a variety of more elaborate models with the inclusion of all plausible control variables but none showed any statistically discernible effect of the campaign on political interest. It should be noted, however, that the level of interest in the referendum was relatively high to begin with, with nearly three in four respondents professing that they were 'very' or 'fairly'

[34] The differences apparent in Figure 2 are not a result of information proxying for interest: an identical analysis using interest to create four sub-groups showed no differences in learning over the course of the campaign.

[35] There remains the possibility that the Tobit procedure cannot capture learning among the originally best informed if the truncation does not conform to a truncated normal distribution, though this seems rather unlikely. The results show, at the very least, that the learning of the least informed is probably not less than that of the best-informed segment of the population.

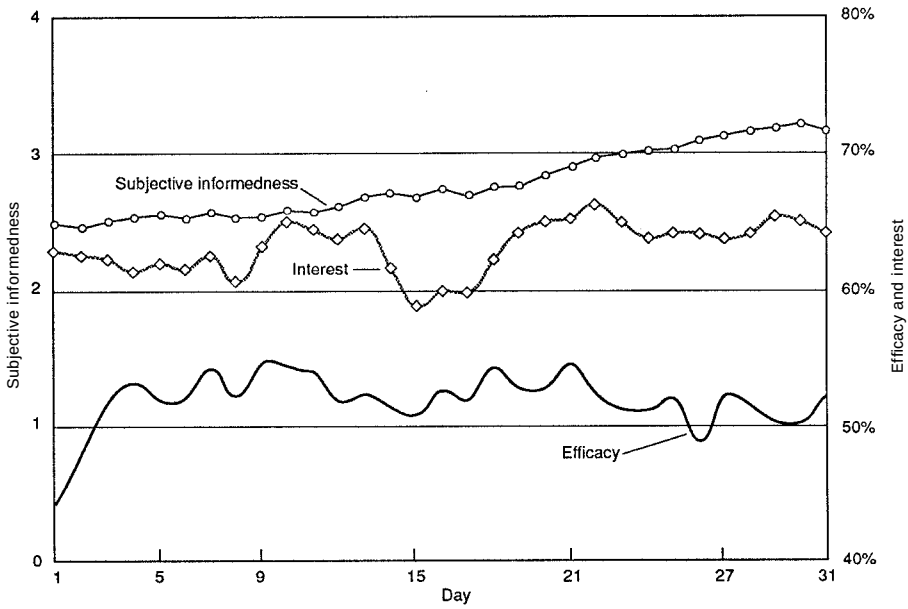

Fig. 3. Interest and efficacy – five-day moving averages

interested in the referendum. So we can conclude that, at least in 1992, interest was high due to the importance of the question and the rarity of referendums in Canada, but that the campaign itself did not appear to politicize citizens.

This result, however, is paradoxical, as it implies that the learning and increased attention to the media discussed above took place without voters admitting to greater interest. The puzzle implies one or both of the following: (1) citizens felt a duty to pay closer attention to the media as voting day approached, but were not in fact more interested; and/or (2) that the standards by which people assessed their level of interest increased as the campaign went on, implying that citizens did become more interested but did not feel any more interested relative to what they perceived as an increasing social standard. Testing these competing hypotheses is impossible with the data at hand. If the second hypothesis is more accurate, it means that self-reported media exposure is probably a better indicator of interest than the subjective self-assessment. If we reject the self-assessment of interest as too interpersonally incomparable and thus contaminated by measurement error,[36] we are left with a behavioural manifestation of interest – namely, attention to the media – which did go up during the campaign. While the argument that objective interest did in fact increase slightly over the course of the campaign is debatable, it is the most plausible story told by these data: as referendum campaigns unfold, citizens pay closer attention to the media (perhaps because they are more interested, perhaps because they think they must), resulting in small but significant increases in political knowledge.

[36] H. E. Brady, 'The Perils of Survey Research: Inter-Personally Incomparable Responses', *Political Methodology*, 11 (1987), 269–91.

'Do referendums promote political efficacy?'

We draw on two items that tap aspects of the complex concept of efficacy. First, we use a traditional question to measure 'external efficacy' – whether respondents felt that they had 'no say' in government decisions.[37] In order to ascertain the effect of the campaign, we first plot the five-day moving average (Figure 3), which shows little overall movement (the ordinary least squares (OLS) coefficient was indistinguishable from zero).[38] However, there is a noticeable increase of about ten points over the first week of the campaign. Perhaps, then, the real increase in efficacy comes from the simple awareness of the referendum's existence, rather than steady growth throughout the campaign, though the small number of cases prevents us from drawing this conclusion with any degree of certainty. If the announcement of the referendum is enough to boost efficacy, early movement makes sense as the less attentive find out about the referendum's existence during the first week of the campaign. Such an argument implies that the early campaign reading would be comparable to usual efficacy ratings in Canada, though the overall campaign reading would be above historic levels.

In fact, such an account is consistent with the time-series of political efficacy in Canada over the previous twenty-five years (Figure 4). While the early campaign reading is close to the previous inter-election reading, the full campaign average is roughly on par with the two federal elections thought to have most engaged the electorate (the election of 1968, which brought new leadership to power in the form of Pierre Trudeau, and the election of 1988 which focused on the controversial Free Trade Agreement with the United States).[39] Figure 4 also shows a significant increase over the reading in 1990, taken shortly after the failure of the previous constitutional package, widely described as elite-driven. Therefore, the data suggest that the referendum's existence slightly boosted Canadians' political efficacy, but only to a level roughly comparable to exciting (1968) or important (1988) elections.[40]

Our second variable is not a standard measure of internal efficacy – the feeling that one is capable of meaningful participation – but seems to capture its essence. The measure is of the respondent's assessment of their own level of information. Figure 3 shows a significant increase in voters' sense of informedness. Despite the likelihood of a social desirability bias on this question, the result provides suggestive evidence that citizens felt more capable of participation. However, this increase in internal efficacy may be regarded in a less than positive way: recall that this significant increase in sense of informedness was coupled with the very moderate increase in real political knowledge reported in Figure 1.

[37] Due to what seems to be a computer programming error in the survey instrument, our external efficacy variable is limited to respondents interviewed in English. This reduces the *N* to 1,584.

[38] We attended to the possibility that the efficacy response was influenced by whether one's preferred side appeared to be winning. No systematic dynamic differences could be found between those who thought they were on the winning and those who thought they were on the losing sides (though YES voters tended to be more efficacious in general).

[39] On the 1968 election, see J. Meisel, *Working Papers on Canadian Politics* (Montreal and Kingston: McGill–Queen's University Press, 1975); on the 1988 election, see Johnston *et al.*, *Letting the People Decide*.

[40] The fact that we observe the effect over the longer time-series means that we might plausibly attribute the change to any of the events of 1992. A powerful counter-argument to this alternative hypothesis is that this relatively high level of efficacy existed *despite* a tremendously unpopular Federal government (according to the 1992 CES, the governing Conservatives had the support of only 16.7 per cent of decided voters and their leader had a thermometer rating of just 32.9). The customary link between government popularity and efficacy was absent in 1992, suggesting that the referendum detached responses to the 'no say' question from evaluations of the current government.

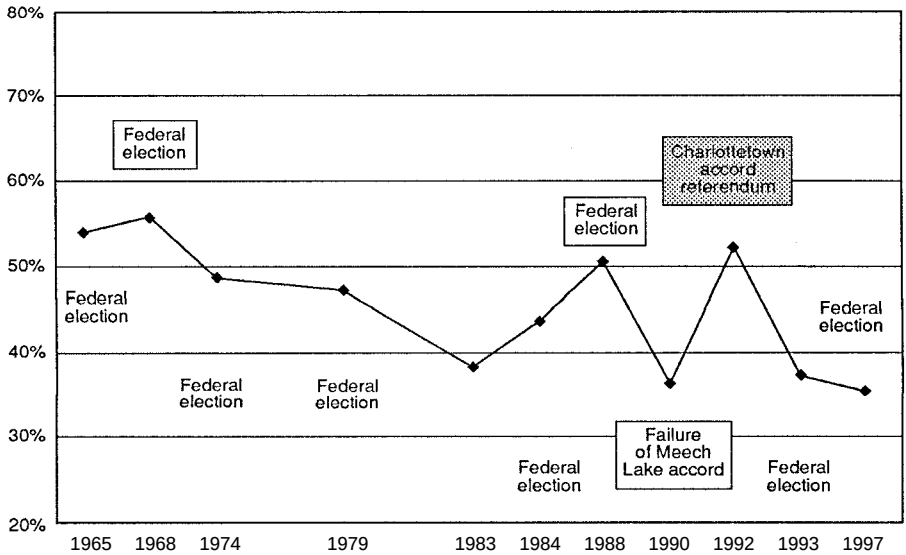

Fig. 4. Efficacy, 1965–97: percentage of English respondents disagreeing with statement, 'People like me have no say in what government does.'

'Do referendums encourage political intolerance?'

The CES contains several measures that allow us to test the proposition that referendum campaigns activate 'tribal' loyalties and threaten minority rights. We employ the feeling thermometers measuring warmth towards 'immigrants', 'English Canadians', and 'Quebec'. We also constructed a 'provincentrism' measure: for English-speakers outside Quebec, we subtracted the respondent's rating of Quebec from their rating of their own province, and did the reverse for French-speaking Quebeckers. We employ two other indicators: whether 'letting the majority decide' or 'protecting the rights of minorities' was 'more important', and whether immigrants ought to 'try harder to be more like other Canadians'.

Figure 5 shows the now-familiar five-day moving average for each of these variables. In all cases except 'provincentrism' a downward trend would indicate increasing intolerance. The most obvious feature of the graph is that no measure moves in the less tolerant direction. Four of the seven series show a statistically significant linear trend towards greater tolerance, while the others have insignificant coefficients signed in the more tolerant direction (regression results not shown).[41] There is no evidence that the referendum campaign provoked hostility towards out-groups, despite an often nasty campaign that invited voters to do just that. Though such results may not hold for all referendums, this finding does underline that it cannot be assumed blithely that

[41] We tested for the possibility that the 'majority rule–minority rights' item moved in the more 'tolerant' direction due to a dramatic movement towards 'minority rights' by Francophone Quebecers, possibly dissimulating a shift towards 'majority rule' by Canadians outside Quebec. This was not the case, with similar coefficients similarly signed for both groups.

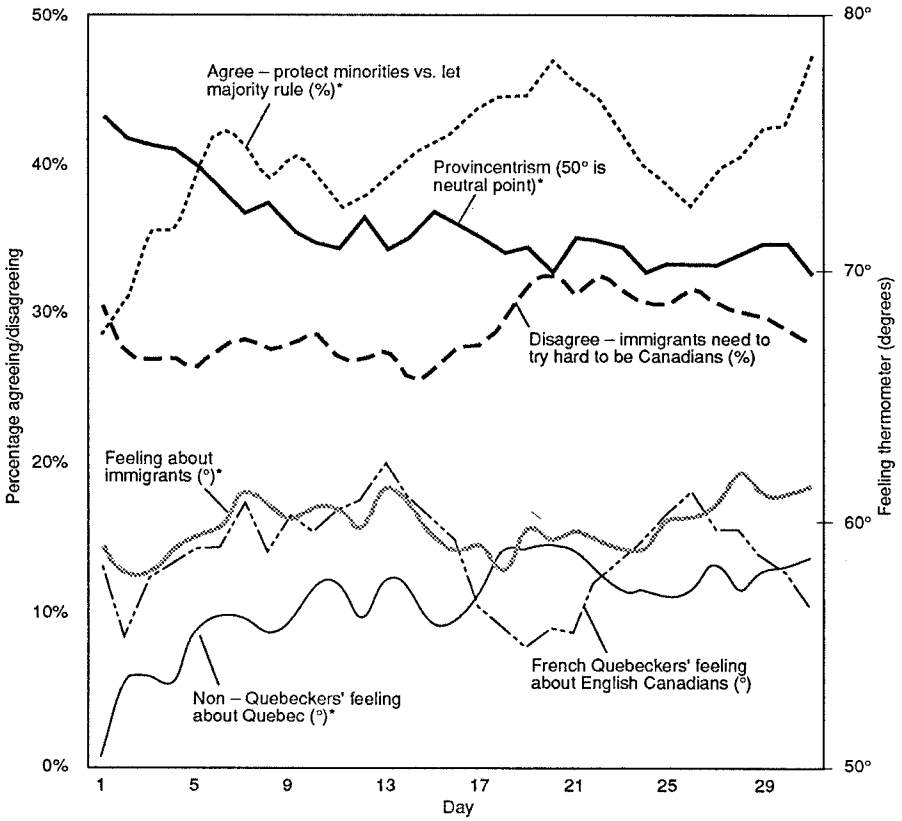

Fig. 5. Tolerance – five-day moving averages

campaigns provoke intolerance or inter-group hostility. In fact, in 1992 the opposite was true.

CONCLUSIONS

Many observers in liberal democracies have become increasingly preoccupied with the democratic deficit, declining confidence in existing institutions, and the rise of anti-democratic movements. Some have suggested that one way to combat these trends would be to increase citizen participation in decision making, which could include the periodic use of the referendum on issues of national importance.[42] Although we have examined one campaign only, and have just begun to scratch the surface of the broad question of the effect of referendums on citizens' democratic capacities, the data allow us to posit a number of propositions:

[42] P. Resnick, *Twenty-First Century Democracy* (Montreal: McGill–Queen's, 1997) suggests that the occasional use of the national referendum provides a 'safety valve' for feelings of disaffection.

1. *Citizens learn as referendum campaigns progress; this learning is limited, but takes place even among the most poorly informed segments of the electorate*.
2. *Voters do not claim to be more politicized as the campaign progresses; however, they do pay closer attention to the media*. This behaviour, which suggests increased interest, contributed to the observed learning.
3. *Referendums do little (but not nothing) to combat low political efficacy*. Efficacy did not increase over the course of the campaign, though citizens did feel more efficacious during the 1992 campaign than in the previous few years, at a level comparable to that which occurs during important elections.
4. *Referendum campaigns do not provoke an increase in intolerant attitudes*. The conventional wisdom following the 1992 referendum was that the public was simply too intolerant to be consulted, voting on the basis of their worst prejudices about their fellow citizens. However, our evidence demonstrates that, despite the constant invitation of political leaders, Canadians did not become more hostile towards other groups. On those measures which did show movement, there was a tendency towards the more tolerant position.

National consultations may contribute in a small way to healthier democratic politics. Despite the fact that Canadian political leaders had been coerced into holding a referendum and many observers felt that the political class was looking to orchestrate a response and seek legitimation, rather than engage in genuine consultation, we detected an increase in objective information and attention to the media. Furthermore, Canadians were more willing to challenge the claim that 'people like me have no say in what the government does' than at any time in the 1990s. The most damaging argument against referendums – that they prompt intolerance – was found to be groundless in the case examined. Yet, if referendums are to offer substantial increases in politicization, efficacy and political knowledge, these consultations probably need to occur on a more regular basis, be more fully institutionalized, be part of a broader process of citizen participation, and occur in situations where there is a genuineness of purpose amongst political elites. Although the democratic deficit will not be addressed by occasional national referendums, we found no evidence that these consultations contribute to the problem. Some of these conclusions may appear self-evident, yet it is important to be clear about what the limited use of national referendums can and cannot do.

Because the CES concentrated more heavily on the issues of the 1992 referendum than on its democratic effect on citizens, our conclusions need to be re-tested using better measures during another campaign. Unfortunately, most studies of voting behaviour during referendums appear to be highly influenced by the concepts used to study electoral behaviour, judging by the large number of papers addressing the question of the determinants of the vote during referendums. However, theoretical works on the democratic effect of referendums tend to be devoid of empirical content. Research using well-constructed survey items explicitly designed to test democratic theory and the arguments for and against the use of referendums is required to bridge this gap in our understanding of an increasingly important component of democratic political practice.

APPENDIX A – VARIABLE DEFINITIONS

Variable	*Source and Definition*
Date	refdate: recoded to 0–1.
Media use	refa2 + refa5: 'How many days in the past week did you [watch the news on T.V] + [read a daily newspaper]', Result is 0–14 (days).
Interest	refint1: 'Would you say you are very interested (1), fairly interested (0.66), not very interested (0.33), or not at all interested (0) in the referendum campaign'
Political knowledge*	An index of five factual questions. Respondents who answered correctly received 1, others received 0. The questions were: 1. (refg2a) 'Has the business community taken a public position on the agreement?' (Yes, for the agreement = 1; Yes, some in support, some against = 1) 2. (refg3a) 'Has the women's movement taken a public position?' (Yes, against the agreement = 1; Yes, some in support, some against = 1) 3. (refg1a) 'Has former Prime Minister Trudeau taken a public position?' (those interviewed before 2 October, No = 1; those interviewed on or after 2 October, Yes, against the agreement = 1) 4. (refg5a & refg8a) 'Has Preston Manning (non-Quebec respondents)/Jean Allaire (Quebec respondents) taken a public position?' (Yes, against = 1). 5. (refg6a & refg7a) 'Has Peter Lougheed (non-Quebec respondents)/ Claude Castonguay (Quebec respondents) taken a public position?' (Yes, in support = 1). The index therefore runs from 0 to 5.
Dissatisfied with process	refc5: 'Are you satisfied with how the Charlottetown agreement was reached?' No = 1, otherwise 0.
Quebec resident	refprov: 1 if refprov = 4, otherwise 0.
Has PID	refl1c: 1 if respondent gave a partisan identification (Lib, PC, NDP, Reform, or BQ), otherwise 0.
No say in government (efficacy measure)	refh15: 'People like me have no say in what government does'. Agree = 1, otherwise 0.
Employed	refjob1: 'Are you presently working for pay, are you unemployed, retired, a student, or a homemaker?' Recoded to dummy: employed = 1, otherwise 0.
Income	refo18 & 18a: Income recoded to thousands, standardized to 0–1.
Education	refo3: 1 = No schooling through 11 = Professional/Doctorate, standardized to 0–1.
Old	refage: year of birth. Result subtracted from 1993 to give age. Dummy: 1 if > 60.
Young	refage: year of birth. Result subtracted from 1993 to give age. Dummy: 1 if < 30.
Female	refrgen: Dummy: male = 0, female = 1.
Linguistic minority	refn16: 'What language did you first learn that you still understand?' All non-English/non-French = 1, otherwise = 0.
Born in Canada	refn12: 'In what country were you born?' Canada = 1, otherwise = 0.
Francophone	refn16: First language learned. Dummy: French = 1, otherwise 0.
Worse off if Quebec separates	ref14: 'If Quebec separates from the rest of Canada will your standard of living get better, stay the same, or get worse'. 1 = worse off, 0 otherwise.

Provincentrism	For Quebec francophones: (refh26: 'How do you feel about Quebec' 0–100)– (refh27: 'How do you feel about Canada' 0–100). For English Canadians outside Quebec: (refh28: 'How do you feel about [R's province]' 0–100) – (refh27: 'How do you feel about Quebec' 0–100).
Feelings towards immigrants	refh30: 'How do you feel about immigrants?' 0–100.
Feelings towards English Candians	refh29 (francophones only): 'How do you feel about English Canadians?' 0–100.
Feelings towards Quebec	refh29 (non-Quebec anglophones only): 'How do you feel about Quebec?' 0–100.
Immigrants should try harder	refh19: 'People who come to live in Canada should try harder to be more like other Canadians. Do you agree or disagree?' Agree = 0, otherwise 1.
Democracy should protect minorities:	refh20: 'Which is more important in a democratic society: Letting the majority decide or protecting the needs and rights of minorities?' Protecting minorities = 1, otherwise 0.
Prior information	3 – question index from campaign-wave of CES, October–November 1993. cpsh4: 'What would you say the unemployment rate in Canada is these days, approximately?' Correct is between 8% and 13%. cpsh6: 'What would you say the federal government's deficit is approximately?' Correct is between 21 and 50 billion dollars. cpsi1j: 'Do you happen to know what Kim Campbell's cabinet job was before she became Prime Minister?' Correct = Justice or Defence.

*It is an open question whether those respondents, when asked where the women's movement and the business community stood on the Accord, replied that 'some were in favour and some opposed' should be classified as having given a correct answer. Some believe the middle category is a refuge for guessers. This logic prompted Johnston *et al.* (*The Challenge of Direct Democracy*) to classify them as having given an incorrect response. However, Johnston *et al.* concede that the response 'some in favour, some opposed' is technically correct but add a logically contradictory comment: 'The largest percentage of incorrect [answers regarding an intervenor's position] was for the women's movement, but this is not surprising, as the movement was deeply divided' (p. 124). In our view, theoretically, the fact that the movement was deeply divided suggests that a 'some in favour, some opposed' response cannot be categorized as 'incorrect'. As suggested by one of the anonymous reviewers, we settled the question empirically: using the measure of general political knowledge found in the campaign wave of the CES (discussed above) we compared the mean level of political knowledge for the three groups (those who got it right, those who got it wrong, and those who replied 'some in favour, some opposed'). For the women's movement, those who offered the 'some–some' answer were in fact the most knowledgeable group, and for the business community, those who offered the 'some–some' answer were marginally less knowledgeable than those who said the business community was in favour of the deal, but far more informed than those who said it was opposed. Therefore, those in the 'some–some' category are about as informed as those who gave an unambiguously correct answer (and perhaps more so), and far different from those who gave the wrong answer. Categorizing them as 'incorrect' would introduce more measurement error than classifying them as 'correct'. Using the alternative definition, the coefficients (and standard errors) are: Model 1: *date* 0.39 (0.13); Model 2: *date*: 0.28 (0.12), *media:* 0.14 (0.01); Model 3: *date:* 0.16 (0.11), *media*: 0.31 (0.02).

APPENDIX B

*Estimates of the Effect of the Campaign on Political Knowledge by Prior Information**

Sub Group (facts correct in 1993)	0 ($N = 423$)	1 ($N = 395$)	2 ($N = 371$)	3 ($N = 188$)
Constant	1.30	2.26	2.36	4.48
	(0.18)	(0.25)	(0.25)	(0.34)
Date (0–1)	− 0.22	0.69	4.19	− 0.18
	(0.82)	(1.09)	(1.11)	(1.48)
Date Squared (0–1)	0.76	− 0.22	− 3.05	0.06
	(0.79)	(1.04)	(1.06)	(1.40)
Std. Err. of Est.	1.33	1.69	1.70	1.52
Joint Test F_2-value:	6.47	2.62	14.86	0.09
Date + Date$^2 = 0$	($p > 0.01$)	($p > 0.11$)	($p > 0.0001$)	($p > 0.76$)

*Estimated by Tobit. Dependent variable censored at five correct responses. These estimates are the basis of Figure 2.