

## Partisan Bias, Economic Perception, and the Campaign

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## Partisan Bias, Economic Perception and the Campaign

Voter perception of a diversity of politically important objects is strongly conditioned by partisanship. The claim is as old as they get in the study of political behaviour and has been replicated empirically time and again.<sup>1</sup> Whether the focus is the personal qualities of candidates, the policy positions of parties, or even the state of the national economy, the impact of partisan commitment on perception has been found to be substantively significant, statistically robust and enduring over time. As Bartels (2002) concludes, “partisan loyalties have pervasive effects on perceptions of the political world” (138).

This is not good news for representative democracy.<sup>2</sup> If voting decisions are to contain meaningful information about the preferences of citizens, it seems obvious that those decisions must incorporate real—or, at least, unbiased—information about politically relevant conditions. A distorted view of the political world will produce distorted political decision-making. Thus, the impact of partisanship on perception—what is here termed *partisan bias*—is clearly a problem for democratic politics.

Yet, there are reasons to suspect that the magnitude of partisan bias varies—in particular, the informational onslaught of the modern election campaign may lead to significant *reductions* in partisan bias. The claim rests on the assumption that election campaigns are broadly educative experiences for voters—that is, that election campaigns ‘enlighten’ the voter (Gelman and King 1993). The assumption has lately become popular in work on campaign effects, but has yet to be applied to the question of partisan bias (see, for example, Gelman and King 1993; Bartels 1992, 2006; Finkel 1993; Markus 1988, 1992; Stevenson and Vavreck 2000; Sekhon 2004; Anderson, Tilley and Heath 2005; Arceneaux 2005). The basic intuition is that the contestation and noise of the campaign supplies voters with both the motivation and resources to form improved political perceptions. Accordingly, the progress of the campaign may lead to a reduction in partisan bias and, in so doing, help to facilitate the representative function of democratic elections.

The aim of this paper is to evaluate this ‘campaign learning’ argument. The principal research question is, thus, *Does the progress of the campaign erode partisan bias in political perception?* A secondary research question raises the bar for campaign learning a little higher: *Does the progress of the campaign ‘narrow the gap’ in the degree of partisan bias between those at high and low levels of pre-existing political information?*

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<sup>1</sup> See, for example: Lazarsfeld, Berelson and Gaudet 1944; Berelson, Lazarsfeld and McPhee 1954; Campbell, Converse, Miller and Stokes 1960, 1966; Markus and Converse 1979; Franklin and Jackson 1983; Lodge and Hamill 1986; Conover, Feldman and Knight 1987; Jacoby 1988; Conover and Feldman 1989; Rahn 1993; Finkel 1993; Granberg 1993; Kinder and Mebane 1993; Mutz 1994; Duch, Palmer and Anderson 2000; Bartels 2002; Johnston, Hagen and Jamieson 2004; but see Gerber and Green 1999; Krosnick 2002.

<sup>2</sup> There is one clear exception to this statement, given certain theoretical and empirical assumptions: when partisan bias in perception of candidates’ policy positions reflects “partisan stereotyping” (Rahn 1993). However, such stereotyping is only compatible with democratic precepts under two, rather strict, conditions: partisan labels are reliable indicators of candidates’ policy positions, and voters actually know what policy positions go with what partisan label.

The question recognizes the fact that partisan bias may matter most to those with the smallest store of politically relevant ‘facts’ and whose perceptions, consequently, may be less constrained by political reality.<sup>3</sup> At the same time, the question acknowledges a fundamental fact of mass political attitudes and behaviour: the distribution of political competence is highly unequal.<sup>4</sup> A robust form of campaign learning, thus, must feature both ‘enlightenment’ and ‘equalization.’

The empirical focus of the analysis is partisan bias in national economic perceptions. A focus on the economy is a good place to start for several reasons. First, the economy is a ubiquitous political concern—economic matters are central to political contestation and to social well being everywhere. Second, no doubt as a result of this ubiquity, the political psychology of economic perceptions has been widely studied, most prominently in the extensive literature on economic voting. Finally, this thriving research interest in the link between the economy and politics has produced a surfeit of useful data—especially survey data—suitable for addressing questions about a variety of topics, including campaign learning, in a wide range of national and institutional contexts.

Data for the analysis come from survey data collected during ten elections: the five Canadian general elections from 1988 to 2004, the New Zealand elections of 1996, 1999 and 2002, the British general election of 2001, and the US Presidential election of 2000.<sup>5</sup> These data share the unique property of having been collected as rolling cross-sections (RCS). RCS survey methodology essentially produces a random sample of respondents for each day of the survey period (typically the length of the campaign) (Johnston and Brady 2002). These data, thus, permit explicit modelling of interactions between partisanship and campaign time.

The paper is organized as follows. The first section considers the theoretical background, along with its surprisingly varied empirical implications. Next comes discussion of methodological issues. This is followed by presentation and discussion of the results. The final, concluding section summarizes and reflects on the paper’s broader significance.

## **Theoretical Context**

Campaign learning arguments aside, theoretical expectations regarding the impact of the campaign on partisan bias in national economic perception vary considerably. Expectations vary according to assumptions about the nature of partisan bias, the nature and function of discourse about the economy, and the nature of the campaign’s impact on

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<sup>3</sup> The evidence for heterogeneity in political cognition is legion and, notably, extends to the literature on campaign effects (see, *inter alia*, Sniderman, Brody and Tetlock 1991; Zaller 1992; Johnston et al. 1996; Alvarez 1997; Gerber and Green 1999; Lau and Redlawsk 2001; Ansolobehere and Iyengar 1995; Kahn and Kenney 1997; Johnston, Hagen and Jamieson 2004).

<sup>4</sup> The *de rigueur* citations supporting such a claim include: Campbell et al. 1960; Converse 1964; Butler and Stokes 1966; Sniderman, Brody and Tetlock 1991; Luskin 1991; Zaller 1992; Delli Carpini and Keeter 1996. Fournier (2002) evaluates the claim in the Canadian context.

<sup>5</sup> Details in the Appendix.

voter attention to economic discourse. The empirical literature offers little reason to form strong expectations on any of these points—a diversity of assumptions are commonly made, with empirical evidence available to support each view. The approach here is, consequently, catholic. This section presents the theoretical possibilities in each area and then considers the empirical implications of each possible combination of assumptions.

### *Partisan Bias*

Two broad theoretical approaches to the general phenomenon of partisan bias are prominent in the literature. The first view holds that partisanship functions as an ‘informational shortcut’ for voters (Downs 1957; Sniderman, Brody and Tetlock 1991; Popkin 1991). The basic idea is that, in the absence of information that is directly relevant to the formation of a given perception, voters make inferences from partisanship that substitute for the missing information. The proposition has been established most impressively in the domain of candidate perception (Lodge and Hamill 1986; Conover and Feldman 1989; Rahn 1993). Here, voters have been found to draw on “partisan stereotypes” (Rahn 1993) and schemas (Lodge and Hamill 1986) to ‘fill in the blanks’ in their perceptions of candidates—for instance, by inferring a candidate’s policy positions and other traits from knowledge of the candidate’s partisanship. A similar process could be at work in the economic domain. Strong partisans may infer that economic conditions are good when their party is in office, providing they regard their party as a competent economic manager (cf. Kinder and Mebane 1983; Conover, Feldman and Knight 1987).

A crucial implication of the shortcuts argument for the present analysis is that reliance on shortcuts decreases as information levels increase. That is, as voters acquire information that is more directly relevant to a given perception—say, a candidate’s explicit statement on a policy issue or information concerning real economic conditions—they should be less likely to fall back on partisan stereotypes and schemas (e.g. Lodge and Hamill 1986; Sniderman, Brody and Tetlock 1991). As regards campaign learning in the economic domain, then, if the campaign generates economic information that is directly relevant to the formation of national economic perceptions, partisan bias in such perceptions should fall with the approach of Election Day.<sup>6</sup>

A complication with this line of argument concerns the amount of information voters require to make use of shortcuts in the first place. For perceptions of candidate characteristics, the link seems obvious and, therefore, likely to be ubiquitous. This may not be the case for the economy. That is, the link may require ‘contextualizing’ information—information that helps voters to make connections between partisanship and national economic perceptions (Mutz 1994, 1998). Results in Duch, Palmer and Anderson (2000) and Bartels (2002) would tend to support this view. The impact of the campaign, then, may not be bias reducing at all. Of crucial importance here is the nature

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<sup>6</sup> This is the implication of findings in Conover, Feldman and Knight (1987), who find that partisanship and other political attitudes have a greater impact on prospective (i.e. future-oriented) than retrospective (i.e. past-oriented) economic perceptions. This finding is consonant with the ‘partisanship-as-shortcut’ hypothesis, at least insofar as information relevant to retrospective judgements is intrinsically more accessible than comparable information for prospective judgements.

of campaign discourse concerning the economy, and the relative proportions of ‘contextualizing’ and ‘raw economic’ information present in that discourse. More on these distinctions in campaign discourse is presented below. Note for now that, under certain conditions, the impact of the campaign may be to increase reliance on partisan shortcuts in the formation of economic perceptions.

The second view of partisan bias is less concerned with inference processes in the absence of information than it is with reception processes in the presence of information. This view focuses on selective attention to and biased perception of incoming information and is, in fact, the classic view of partisan bias in political perception (see, especially, Lazarsfeld, Berelson and Gaudet 1944; Berelson, Lazarsfeld and McPhee 1954; Campbell, Converse, Miller and Stokes 1960, 1966; Finkel 1993; Granberg 1993; Bartels 2002). The argument is that voters filter political information and bend their political perceptions in the direction of partisanship in order to minimize the psychic stress associated with dissonance in one’s belief system (Festinger 1957). Applied to the economic domain, the expectation is that voters either ignore or distort economic information that implies perceptions of the national economy that are uncongenial to their partisan inclinations—i.e. good economic news when their party is out, bad economic news when their party is in. The result is systematic bias in economic perceptions according to partisanship.

The tendency to filter or distort economic information that is inconsistent with one’s partisan commitments should increase as awareness of such inconsistency increases (Festinger 1957), and this awareness, in turn, should be a function of information levels (Sniderman, Brody and Tetlock 1991; Zaller 1992). That is, as one’s store of political information increases, especially information that posits links between partisanship and economic perceptions (i.e. ‘contextualizing’ information), the likelihood of selective attention and biased perception should increase. Thus, the impact of the campaign on partisan bias that is rooted in these processes may be to increase partisan bias, depending on, again, the relative proportions of ‘contextualizing’ and ‘raw economic’ information in campaign discourse. Here, then, the predictions of the ‘shortcuts’ and ‘biased perception’ interpretations of partisan bias converge, with complications for the analysis as discussed below.

### *Campaign Economic Discourse*

As suggested above, predictions about the impact of the campaign on partisan bias in national economic perceptions depend crucially on assumptions about the nature of campaign discourse about the economy. Indeed, different combinations of assumptions about campaign discourse, on the one hand, and the nature of partisan bias, on the other, can imply that the campaign has a bias-reducing effect, a bias-increasing effect, or no effect at all. These possibilities are developed in more detail below, but consider first the two general types of relevant campaign discourse that are implicit in the theoretical literature.

The first type of discourse, already hinted at, can be termed ‘raw economic information,’ or simply ‘raw data.’ This kind of discourse is just as it sounds: information concerning the objective state of the economy, although not necessarily ‘objective’ information. Raw data can include explicit reports concerning key economic indicators, such as change in the Gross Domestic Product or in unemployment levels, and also more summary interpretations of the state of the economy, such as television news reports of general changes in economic conditions. Raw data may even include ‘episodic’ depictions of personal economic hardship, such as reports of individual economic hardships or job losses in a particular industrial sector (Iyengar 1991). Whatever the particular informational content, the distinguishing feature of raw data in the present context is its non-political character. This kind of discourse should have obvious significance for the formation of national economic perceptions, but no direct implications for attributions of political responsibility for either the creation or amelioration of economic problems (Kinder and Mebane 1983), much less implications for partisanship or perceptions of candidates.

In contrast, the second type of discourse, ‘contextualizing information,’ should have exactly these kinds of attributional and perceptual implications. This kind of discourse makes explicit links between economic conditions and the actions of political incumbents. Such contextualizing discourse might take the form of explicit attributions of credit and blame for economic outcomes to parties and candidates, or it may appear as more general discourse about the importance of ‘economic competence’ as a consideration in the vote decision. No matter what the particular form, the key feature of such discourse is that, in contrast to raw data, its character is manifestly political (Mutz 1994, 1998).

In practice, these two ideal types may be interlaced in campaign discourse. In fact, this kind of ‘mixed’ discourse may be the dominant mode of economic information dissemination during election campaigns. But the raw data-contextualizing discourse distinction is important theoretically, for each makes a different potential contribution to campaign learning in the economic domain. Specifically, as suggested above, increases in raw data might, if partisanship functions as an information shortcut, offset partisan bias in economic perceptions. Increases in contextualizing information, on the other hand, should increase bias in economic judgement, whether partisanship functions as a shortcut or perceptual filter (see below).

### *The Campaign and Voter Attention*

Assumptions about the impact of the campaign on voter attention are the final area of important theoretical variation pertaining to this analysis. Expectations here are crucial to evaluating the argument that the campaign offsets pre-existing inequalities in political learning. The key concern is the way in which the campaign’s impact on partisan bias is articulated across the spectrum of chronic political information. The crucial question is, thus, for whom does the campaign matter, and how?

An important prior theoretical concern is the impact of political communications across information levels in general. For the present analysis, two variables are important in this regard. The first is the likelihood of exposure to political communications, especially as indexed by variables such as political interest and attention. Increasing levels of information tend to imply increasing interest and attention (Luskin 1987; Zaller 1992, 1996; Price and Zaller 1993; Delli Carpini and Keeter 1996), and so high information individuals should, all other things being equal, be most responsive to campaign economic discourse, whether of the raw data or contextualizing variety. However, all things are not likely to be equal, as information also should be positively correlated with a variable that should limit response to campaign economic discourse, the weight of previously acquired information. The variable is crucial to Bayesian models of political learning, such as that of Gerber and Green (1999), and also figures in longstanding arguments concerning the stability of partisanship (Converse 1962). The argument, in short, is that the attitudinal impact of new information is constrained by the accumulation of information preceding it. Thus, among the information rich, the positive effect of attentiveness on the impact of campaign economic discourse may be offset by a comparable negative effect from the weight of previously acquired economic information.<sup>7</sup> For campaign effects, the summation of these opposing forces may imply roughly equivalent effects across information levels.

The progress of the campaign, however, may disrupt this pattern. In particular, the campaign may untie the link between political information and attention, insofar as the noise and excitement of the campaign increases the intrinsic interest of politics and, by extension, voter motivation to attend to political developments. This, of course, is the assumption of Gelman and King (1993) and other exponents of the ‘enlightenment’ image of campaigns. But, even if the campaign does typically stimulate political interest and attention, it is unclear if motivational gains are spread evenly across the electorate or concentrated where they might do the most good—that is, among the chronically uninformed, uninterested and inattentive.

Three possible assumptions about the impact of the campaign on voter attention inform the present analysis. The first assumption is that the campaign has no impact on voter attention. In this case, campaign economic discourse should, as suggested above, have roughly equivalent effects across information levels (i.e. among high and low information voters). The second possible assumption is that the campaign increases voter attention in equal amounts across pre-existing levels of political information. In this case, campaign economic discourse should, again, have similar effects across information levels.<sup>8</sup> The final possible assumption is that the campaign increases voter attention primarily among

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<sup>7</sup> Note that message acceptance, the traditional ‘second mediator’ in Zaller’s two-mediator model of political persuasion, does not appear here. The sort of partisan biased information processing that constrains the persuasive effects that interest Zaller should actually magnify the effects that are of interest here—indeed, they are the effects of interest, at least according to the biased perception image of partisan bias.

<sup>8</sup> The exact distribution of effects depends on assumptions about the relative weight of exposure and *ex ante* information (i.e. the weight of previously acquired information) in determining the magnitude of the campaign’s impact across information levels. In the present analysis, however, indicators of the relevant quantities are too imperfectly measured for subtle predictions of this sort.

low information voters. The expectation in this case, consequently, would be for campaign effects to maximize among the chronically ill informed.

### *Empirical Implications*

Now that the range of plausible assumptions about the nature of partisan bias, the nature and function of campaign economic discourse, and the nature of the campaign's impact on voter attention have been reviewed, it is possible to consider the empirical implications of the interaction of these assumptions. This interaction is depicted in Table 1. Each cell of the table represents a combination of assumptions about the nature of partisan bias, campaign economic discourse, and attention increases in the campaign. Cells contain three predictions relevant to the analysis: the information level with the highest initial partisan bias in national economic perceptions (i.e. magnitude of partisan effect on the first day of the campaign); the relative magnitude of campaign effects across information levels ('Equal,' greater among 'High' information voters, greater among 'Low' information voters); and the direction of the campaign's effect (+, -, Ø). Each cell is also numbered for reference.

First, some general remarks about Table 1. The table depicts twelve combinations of theoretical assumptions, but in fact contains only six unique predictions. This is a crucial fact of the analysis, as alluded to in the introduction of this paper. That is, the present analysis is in some degree indeterminate theoretically—different theoretical assumptions are compatible with the same pattern of results. Cells 3, 4 and 6 contain a common prediction, as do cells 9, 10 and 12, cells 2 and 8, and cells 5 and 11. This is an obvious difficulty for the analysis. That said, the campaign learning argument is compatible with only one combination of theoretical assumptions—those defined by cell 7—and so the findings can be decisive in this regard. Furthermore, the analysis can distinguish somewhat between assumptions about the nature of campaign discourse, as no prediction is common between the 'raw data' cells and the 'contextualizing' and 'mixed' cells. As discussed below, this fact permits the analysis to be quite instructive theoretically.

Now turn to specific predictions:

- *Cells 1 and 7* depict the campaign's impact if partisan bias functions as an informational shortcut and campaign economic discourse is strictly constituted by economic raw data. In both cases, initial bias should be maximized among the low information group, as these voters are most lacking in information directly relevant to economic perception. Likewise, in both cases the campaign's effect on partisan bias should be negative—the campaign should reduce reliance on the partisan shortcut in the formation of economic judgements. Where the predictions of these two cells differ is in the relative magnitude of effects across different information levels. If the campaign drives attention increases for all voters or produces no attention increases at all ('General or Nil'), campaign economic discourse should offset bias in roughly equal amounts across information levels (cell 1). If, on the other hand, attention increases are asymmetrical (i.e. increases are concentrated among low information voters), the bias-reducing effect of the

- campaign should be maximized among low information voters (cell 7). Cell 7, thus, is the ‘campaign learning’ cell: the campaign erodes partisan bias for all voters, but does so mostly in the low information group, and so narrows the gap across information levels. Learning also takes place under the conditions of cell 1, of course, but pre-existing information inequalities remain. As will now become clear, learning, in this sense, does not take place under any other combination of theoretical assumptions.
- *Cells 2 and 8* depict the campaign’s impact if partisan bias results from selective attention and perceptual bias and the campaign generates only economic raw data. Whether attention increases are general, nil, or concentrated among low information voters, the prediction is the same: initial partisan bias is greatest among high information voters and the campaign has no impact on the magnitude of that bias. That the initial bias is greater in the high information group follows from their greater awareness of the implications of economic perceptions for partisan commitments. For a related reason, the effect of the campaign is blunted here: if the campaign produces only economic raw data, there is no reason to expect awareness of the links between partisanship and economic judgement to rise, either among low or high information voters and under any assumption about the campaign’s impact on attention.
  - *Cells 3 and 9* imagine the campaign’s impact if partisan bias is rooted in shortcutting processes and the campaign generates only contextualizing economic discourse. Under these conditions, bias is appropriately magnified for high information voters and increases for all over the course of the campaign. Increases are steeper for low information voters if attention increases are concentrated in this group (cell 9). In one sense, this latter prediction is the ‘worst case scenario’: bias increases for all, but especially for low information voters. On the other hand, these asymmetrical bias increases should narrow pre-existing gaps in bias, and so have an equalizing effect, if a perverse one.
  - *Cells 4 and 10* depict the impact of the campaign if partisan bias is rooted in selective attention and biased perception and the campaign generates only contextualizing discourse. Predictions here are the same as in 3 and 9, respectively. Bias is concentrated among high information voters (they make the strongest initial partisanship-perception links), the campaign increases bias for all, and asymmetrical increases in attention mean that bias increases most for low information voters. Thus, the shortcut and biased perception accounts blend into each other empirically to the extent that shortcutting is not automatic (i.e. depends on contextualizing information). This point returns in discussion of results and in the conclusion.
  - *Cells 5 and 11* contain predictions for campaign effects if partisan bias functions as an informational shortcut and if the campaign produces both raw data and contextualizing discourse. In a sense, these cells sum the effects in cells 1 and 3, on the one hand, and cells 8 and 9, on the other. The result is a null prediction on all counts. If the campaign generates raw data that reduces shortcutting at the same time as it produces contextualizing discourse that facilitates it, the effects might be largely offsetting. As such, these two impacts of the campaign should be confounded in the analysis, irrespective of varying dynamics in attention. For

a similar reason, there may be no systematic differences in the initial effects of partisanship, that is, if pre-existing information is also a mixture of raw data and contextualizing cues. A key implication for the empirical analysis is that apparently null effects may in fact conceal a good deal of theoretically important action.

- *Cells 6 and 12* imagine the impact of the campaign if partisan bias is rooted in biased perception and attention and campaign economic discourse is a mixture of raw data and contextualizing information. The cells can be thought of as the sum of cells 2 and 4, on the one hand, and cells 8 and 10, on the other. The result is a campaign that increases bias for all, although initial effects are larger among high information voters, and which may concentrate bias among low information voters if attention increases are maximized among this group.

Overall, the predictions in Table 1 make several important points. First, theory is clearly divided on the impact of the campaign on partisan bias. Varying combinations of equally plausible assumptions produces a wide range of predictions. This point is not well appreciated in the literature. Second, almost none of these predictions are compatible with the campaign learning argument. Evaluating the argument on the basis of theory, thus, produces faint hope for the hypothesis. Third, as noted above, similar predictions about campaign dynamics can be derived from a range of theoretical assumptions. The implication is that the analysis of campaign dynamics cannot resolve all of the theoretical questions surrounding the impact of the campaign. That said, a final important point made by Table 1 is that a major theoretical fault line in accounts of the campaign's impact on partisan bias is defined by assumptions about the nature of campaign economic discourse. As indicated above, no prediction straddles this fault line.

## Methodology

### *Modeling*

The paper's basic analytical approach is to regress, for each election, national economic perceptions on party identification and interactions of party identification with campaign time, while controlling for important socio-demographics.<sup>9</sup> Separate estimates are presented for high and low information voters, along with a preliminary estimation pooling observations from both groups. Negative coefficients on the interactive terms imply linear reductions in bias across the campaigns, while positive coefficients indicate increases. All estimations are OLS.

Two equations figure in the analysis:

$$NEP = f(\beta_0 + \beta_1 UNEMP + \beta_2 INCOME + \beta_3 INCPID + \beta_4 NOPID + \beta_5 DAY + \sum \beta_k SOCDEM_k + u), \quad (1)$$

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<sup>9</sup> These vary by election as described in the Appendix.

$$NEP = f(\beta_0 + \beta_1 UNEMP + \beta_2 INCOME + \beta_3 INCPID + \beta_4 NOPID + \beta_5 DAY + \beta_6 UNEMP*DAY + \beta_7 INCOME*DAY + \beta_8 INCPID*DAY + \sum \beta_k SOCDEM_k + u), \quad (2)$$

where

*NEP* = national economic perceptions;  
*INCOME* = income scale (0-1);  
*UNEMP* = unemployed (dummy);  
*DAY* = day of campaign;  
*INCPID* = incumbent party identification (dummy);  
*NOPID* = no party identification<sup>10</sup> (dummy);  
*SOCDEM<sub>k</sub>* = a set of socio-demographic variables (see appendix); and,  
*u* = random error.

Equation (1) is the static model. The set-up assumes effects do not vary over time; these estimates are presented for their intrinsic interest, although they do not speak directly to the paper's theoretical concerns. Estimates for Equation (2) are key in this regard. This dynamic model of partisan effects on national economic perceptions contains the key interaction of interest—between incumbent party identification and day of campaign—and also controls for campaign dynamics in two other determinants of national economic perceptions by including the interactions *UNEMP\*DAY* and *INCOME\*DAY*.<sup>11</sup>

### *Measurement*

Economic effects on vote choice have been widely studied, so survey instrumentation concerning economic evaluations is commonplace and reasonably comparable across surveys. In the present sample of elections the typical item is some minor variation on the following:

How do you think the general economic situation in New Zealand now compares with a year ago? Is it the same, better or worse? [NZES 1999]

All but two of the surveys contain an item of this sort.<sup>12</sup> In the deviant surveys, the NAES 2000 and NZES 2002, the relevant items query respondent attitudes toward the economy “today” and “these days,” respectively. Both items have been found instructive in previous analyses of these elections (Johnston, Hagen and Jamieson 2004; Vowles et al. 2004; Matthews 2005; Matthews and Johnston 2005) and are suitable proxies for the more standard retrospective measure.<sup>13</sup>

<sup>10</sup> For the BES 2001 estimation, the variable pools those with no partisanship and those with non-major party identifications.

<sup>11</sup> Research reported elsewhere (Matthews 2006) uncovers significant campaign dynamics in unemployment and income effects for several of the elections in the analysis.

<sup>12</sup> All economic perceptions items are, of course, from the campaign wave of each survey. Exact question wordings are available from the author.

<sup>13</sup> In the post-election wave of the NZES 2002, for instance, there is a relatively strong correlation between the ‘current’ economic evaluation measure and a more standard retrospective measure:  $r = 0.49$ .

For its part, party identification is also measured quite generically across surveys.<sup>14</sup> The typical item is some variation on the following:

In federal politics, do you usually think of yourself as a Liberal, Conservative, NDP, Bloc Quebecois, or none of these? [CES 2004]

In the models, as indicated above, incumbent partisans and those expressing no party identification (or no major party identification, in the British case) are separated from all others—i.e. those identified with some party other than the government party. The set-up is obviously a simplification. But, the incumbent partisans vs. other partisans contrast does go to the heart of arguments about partisan bias: all other things being equal, incumbent partisans should take a more positive view of the economy than everyone else. Non-partisans are isolated to clarify the perceptual contrast between the voters that matter, that is, those who possess live identification with a political party.

Levels of pre-existing or chronic political information are measured, where possible, with indicators of general, factual political knowledge, following best practices in the literature (Price and Zaller 1993; Delli Carpini and Keeter 1996). Where this is not possible, political information is measured using indicators of general political interest and media attention, both alone and in combination.<sup>15</sup> Irrespective of the measurement details, for each election, the indicator of political sophistication is split at its midpoint to yield high and low sophistication groups.

The measure of campaign time, *DAY*, is simply the day of the campaign. The variable is a counter that starts at 1 on the first day of the campaign period and reaches its maximum (which varies across elections) on the day before Election Day. The longest campaign in the analysis is the US Presidential election of 2000—for analysis of that election, the counter runs from 1 to 62. The beginning of the general election campaign in the US is typically fixed at the day after Labour Day (Campbell 2000; Johnston, Hagen and Jamieson 2004) and that convention is adopted here. The start of the remaining campaigns is set to the day on which Parliament is dissolved—or ‘writ day’—making these campaigns roughly half as long as in the US case.<sup>16</sup>

Measurement details concerning the sociodemographic controls are reported in the Appendix.

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<sup>14</sup> Exact question wordings available from the author.

<sup>15</sup> Exact details are reported elsewhere (Matthews 2006).

<sup>16</sup> Campaign lengths (days): Canada, 1988: 48; Canada, 1993: 45; Canada, 1997: 36; Canada, 2000: 34; Canada, 2004: 36; New Zealand, 1996: 37; New Zealand, 1999: 30; New Zealand, 2002: 36; United Kingdom, 2001: 30; United States, 2000: 63.

## Results

Analysis will proceed in three stages, focusing strictly on the coefficients of theoretical interest in each model:  $\beta_3$  and  $\beta_8$ .<sup>17</sup> First, general estimates of both models are presented for each election; these estimations pool observations across information levels. Next, estimates are presented that condition on information—separate coefficients are reported for high and low information respondents. Finally, the conditional estimates are graphed. This last stage is crucial to addressing the central concerns of the paper: Does the campaign promote political learning and offset pre-existing inequalities in learning? And what theoretical assumptions best fit the data?

### *General Estimates*

General estimates for the static model are quite sensible (see Table 2). Effects are uniformly positive, although of varying magnitudes. The biggest effect is for the US in 2000, the smallest for the UK in 2001. The median effect is a modest 0.073.<sup>18</sup> This suggests that the typical difference between incumbent and non-incumbent partisans in national economic perceptions across these elections is of only minor substantive consequence, bearing in mind that the national economic perceptions measure ranges from 0 to 1. This seems good news as regards the quality of vote choice. Still, it remains that partisan bias in NEP is ubiquitous across these elections. Furthermore, depending on the dynamics of partisan bias, this ‘general’ estimate may seriously understate the ultimate impact of partisanship on economic judgement by Election Day.

Estimates for the dynamic model are less hopeful as regards the quality of vote choice. First, most of the interactive terms are statistically insignificant and substantively small, suggesting that the campaign has no detectable effect on partisan bias, either positive or negative. Second, those effects that do reach statistical significance are positive—that is, they imply a campaign that increases, rather than offsets, partisan bias in NEP. The elections concerned are New Zealand 2002 and United States 2000. On the other hand, two statistically insignificant effects of substantively significant magnitudes are negative: Canada 2004 and New Zealand 1996. These elections might have witnessed consequential erosion in partisan bias across their campaigns.

Overall, then, pooling observations across information levels produces a pattern of results with mixed theoretical implications. The typical campaign would not seem to ameliorate partisan bias to any significant extent. And we can be certain that the campaign exacerbates bias in two cases. Still, there is a hint that the campaign may reduce bias at times. Does conditioning on information level clarify the pattern?

### *Conditional Estimates*

As with the general estimates, conditional estimates of the static model are quite sensible (see Table 3). All estimates are positive and all but three are statistically significant.

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<sup>17</sup> Full model estimates available from the author.

<sup>18</sup> This is the mean of the effects for New Zealand in 2002 and Canada in 1988.

More strikingly, the effects of partisanship are larger among high than low information voters in nine of the ten elections. In six of these nine cases, furthermore, effects for high information voters are at least twice as large as effects in the low information group. This implies that the theoretical image of partisanship as an automatic shortcut to economic judgment is highly questionable. More plausible, it would seem, is the notion that the link between partisanship and national economic perceptions depends on a non-trivial amount of linking or contextualizing information, whether the link functions as a shortcut in the absence of information or as a perceptual screen in the presence of information.

Estimates for the dynamic model carry more equivocal and complex implications. Only one of the interactive terms is significant, for high information voters in Canada in 2004, and it implies a strongly negative or bias-reducing effect. That said, eleven other coefficients are substantively large ( $\geq 0.0010$ ) and, of these, seven are positive—they imply bias-increasing campaigns. Thus, the estimates suggest that, in most elections, the campaign had some effect on partisan bias in national economic perceptions, often a bias-increasing one.

The high level of uncertainty attaching to most of the estimates counsels caution in interpreting these results. Still, this uncertainty comes as little surprise: the analysis is bedeviled by small sample sizes in some cases (especially for high information voters in New Zealand) and by a high threat of multicollinearity in almost every case, resulting from the presence of three overlapping interactive terms in the dynamic model. Furthermore, graphing the estimated effects, as in Figure 1, reveals that substantively significant dynamics in partisan bias may have occurred in many of the elections, even if these effects are estimated imprecisely. It seems sensible, then, not to overlook important patterns in the data simply on account of technical limitations.

### *Interpreting the Dynamics*

The plots in Figure 1 are designed to speak to the structure of the predictions contained in Table 1. They depict, for each election, the estimated linear impact of campaign time on the magnitude of partisan bias in national economic perceptions by information level. Thus, at a glance, it is possible to see the group in which the initial partisan effect is maximized, the direction of the campaign's effect on partisan bias, and the relative magnitude of the campaign's effect across information levels.

Note first that a look across the plots draws attention to an important fact: in all elections, initial partisan bias is greatest amongst the most informed. Indeed, in six elections partisan bias is greater for high information voters throughout the campaign. This rules out, in a sense, the automatic shortcut argument, for if partisanship is a shortcut in the absence of information, its use should be greatest among the least informed. The finding also squares with the clear pattern of results for the static model when conditioning on information. The implication is that arguments emphasizing the importance of contextualizing links between partisanship and economic judgment are on the right track.

What do the plots imply for the paper's major research questions? For one thing, there is little evidence that the campaign generally decreases partisan bias in national economic perceptions. Eleven of the twenty slopes are positive. Most of these shifts, however, are substantively small and none is statistically significant. Only six of the twenty shifts seem to have made a substantive, if still modest, contribution to the magnitude of partisan bias across their respective campaigns (that is, shifts were roughly 0.10). Of these, three shifts are bias-increasing: among low information voters in Canada 2000 and New Zealand 2002, and among high information voters in New Zealand 1999. The three bias-reducing shifts are: among high information voters in Canada 2000 and 2004, and among low information voters in New Zealand 1996. The overall pattern, then, seems perfectly neutral: the campaign's effect is typically insignificant (by statistical and substantive standards) and, even when the campaign does matter, its effect is as likely to be bias-increasing as bias-reducing. The campaign's effect is also as likely to appear among high as among low information voters.

The direction of the campaign's effect aside, is there any tendency for the campaign to narrow the gap between information levels? For the most part, the answer to this question is no. And this makes sense in view of the above: most of the time, the campaign's effect on partisan bias is undetectable. Where the campaign's effect is detectable, in two cases the gap—as indicated by the difference in partisan effects between high and low information voters—narrows to a substantively significant extent. In Canada 2004, partisan bias among high information voters drops dramatically over the campaign, but is basically unmoved among low information voters, for whom the bias is weaker to begin with. This approximates campaign-learning expectations, save for the fact that it is high information voters who apparently have the most learning to do. In Canada 2000, by contrast, the information gap narrows in a perverse way, by offsetting bias in the high information group and exacerbating it in the low information group. The finding is striking, recalling that no theoretical account implies opposite effects across information levels; this point returns below. Finally, the gap increases in two cases—in New Zealand 1996 and 1999. In the former, partisan bias drops among low information voters while it remains essentially unmoved in the high information group. In the latter, the pattern is reversed—the gap grows as bias increases among high information voters.

These results, thus, leave the campaign learning argument largely in tatters. There is no general tendency for the campaign to reduce partisan bias in national economic perceptions, nor to narrow the gap in the magnitude of bias across information levels. Most of the time, the campaign's effect is modest in the extreme. When the campaign does matter, it is as likely to increase as decrease partisan bias, and to grow as shrink gaps in pre-existing political learning.

How do the results square with the theoretical expectations developed in Table 1? Most of the elections approximate some combination of the assumptions. At the same time, however, no election fits only one combination of assumptions, a consequence of the high degree of overlap among predictions, discussed above. Five elections nearly fit the essentially null expectations of cells 5 and 11—where partisanship functions as a shortcut in the context of campaign economic discourse that is a mixture of raw data and

contextualizing cues. The elections concerned are Canada 1988, 1993 and 1997, United Kingdom 2001, and United States 2000. None of these elections witnessed any significant movement in partisan bias across their campaigns, neither among high nor low information voters. The one divergence between these elections and the predicted effects is in the clear difference in initial effects across information groups—effects in all cases are clearly maximized among high information voters. This may imply that, even if campaign discourse is mixed in content, pre-existing informational differences are defined mainly in terms of contextualizing cues.

One election, New Zealand 2002, approximates the common expectations across cells 9, 10, and 12—partisan bias is initially greatest among high information voters but increases sharply in the low information group across the campaign period. These results fit a campaign that features, on the one hand, contextualizing discourse, attention increases predominantly among low information voters, and partisan bias rooted in either shortcutting or biased attention and perception, or, on the other hand, mixed campaign economic discourse, attention increases mainly in the low information group, and partisan bias rooted in biased attention and perception.

The four remaining elections do not fit any combination of the plausible assumptions. The strangest case is Canada 2000, where, as noted above, bias decreases among high information voters but increases among low information voters. This clearly perverts the campaign learning argument. One theoretical possibility, however, is that the campaign has different effects across information levels. In particular, the campaign may supply low information voters with sufficient motivation and contextualizing cues to make use of shortcuts, at the same time as it motivates high information voters to pay careful attention to raw economic data, which may be more cognitively demanding in any case. Such a view seems compatible with dual process theories of information processing in social psychology (e.g. Petty and Cacioppo 1986). In this way, the campaign actually may facilitate a kind of ‘learning’ for all, but a different kind of learning for each.

Of course, no other campaign fits this pattern. In Canada in 2004, the campaign cut partisan bias among high information voters almost to nothing, but had very little impact at all on low information voters. This may reflect the kind of intensive information processing hinted at above among high information voters. But then, if the campaign had such motivating power for the information rich, why did it have virtually no effect on low information voters? Now consider an election exhibiting the opposite pattern: New Zealand 1996. There, the campaign cut the bias among low information voters, but hardly moved high information voters at all, who took a quite biased view of economic conditions throughout the campaign. If this reflects an increase in intensive information processing among low information voters, the motivating power of the campaign must have been massive. Why, then, was this effect confined to the information poor? The plot thickens with New Zealand 1999, where the campaign increased partisan bias among high information voters but left the low information group virtually untouched.

The overall pattern, then, has murky implications for theory. Half the elections approximate a common and theoretically plausible pattern: the campaign supplies a

mixture of contextualizing cues and raw economic data with offsetting effects on the magnitude of shortcutting across the electorate. One election approximates a pattern that is common to three combinations of theoretical assumptions: the campaign supplies contextualizing or mixed economic discourse that increases shortcutting or biased attention and perception over the campaign, with effects concentrated among low information voters to whom the campaign supplies extra motivation for political attention. The four remaining elections fit no combination of the plausible assumptions, exhibiting variable combinations of bias increases and reductions at all levels of information.

## **Conclusion**

The results in this paper make several important points. Most significantly, the campaign-learning hypothesis clearly fails. There is no general tendency for the campaign to offset partisan bias in national economic perceptions, nor to narrow the gap in perceptual quality across information levels. For the most part, the campaign's effect is nearly undetectable. And, when the campaign does matter, it is as likely to increase as decrease both bias in perception and gaps in the quality of those perceptions across the electorate.

As it happens, this finding comes as little surprise, especially in view of the state of theory relating to partisan bias and campaign effects. A careful review of the variety of assumptions current in the literature, and of the implications of these assumptions when considered in combination, offers little reason to expect the campaign to be a 'learning' experience for voters. Minor adjustments in theoretical perspective lead to dramatic differences in empirical expectations. Most theoretically imaginable campaigns either increase partisan bias in economic judgment or have no effect at all. This crucial point is not well understood in the literature on campaign learning.

Theory also supplies different microfoundations for the same campaign effects. The major overlap is between the image of partisanship as a shortcut that requires a non-trivial store of contextualizing information, and the view of partisan bias as rooted in biased attention and perception. The one domain in which the two accounts lead to clearly different predicted effects is where campaign information is a mixture of raw economic data and contextualizing cues. The shortcuts interpretation implies that the two kinds of information work against each other, one decreasing bias, the other increasing it; the biased attention and perception interpretation, on the other hand, anticipates only bias increasing campaigns.

It is here that the results of the current paper are theoretically instructive. Five of the ten elections witnessed no significant campaign dynamics in partisan bias. This comports with the shortcut interpretation of partisanship and the assumption that the campaign produces economic discourse that mixes both raw data and contextualizing cues. Behind these apparently null effects may have been increases in the capacity of voters to shortcut, along with simultaneous decreases in their need to do so.

The results are also instructive more generally on the nature of partisan bias. Most of the evidence points away from the view of partisanship as strictly an automatic shortcut to economic judgment. Bias is clearly most pronounced among the most informed. This could mean that partisanship is a shortcut that requires contextualizing cues, as suggested above, or that bias is rooted in patterns of attention and cognitive distortion. Either way, it suggests that context matters, either as it makes shortcutting possible or brings dissonant cognitions into awareness.

That said, there are some indications that the campaign can produce the kind of substitution of ‘real’ information for partisan inference implied by the automatic shortcuts account. Bias clearly drops in a few campaigns—indeed, the only campaign effect to register at conventional levels of statistical significance in the conditional estimates is a massive drop in partisan bias among high information voters in Canada in 2004. This seems to suggest that no one cognitive model fits all: different voters engage in different cognitive practices and in different combinations.

Part of the pattern may hinge on the dynamics of campaign attention. No simple theory about the campaign’s impact on attention works. The campaign’s effect—when it has an effect—may occur at high or low levels of pre-existing political information. Furthermore, the nature of those effects does not vary systematically across information levels, although there is a slight tendency to find mainly bias increases among the information poor and bias decreases among the information rich. This may reflect differentiation in the nature of the campaign’s effect on attention across information levels, as suggested above: the campaign may motivate the information rich to become cognitive sophisticates, while at the same time raising the information poor from cognitive pauperism to mere cognitive miser-hood.

But the pattern in this regard is very weak—the campaign’s effect on attention eludes generalization. Different campaigns motivate different voters in different degrees. This contingency makes a larger point about the results: no simplistic, general assumptions about the nature of the campaign’s impact on partisan bias will do. This paper has underlined what are likely the key variables: the nature of partisan bias, the nature of campaign economic discourse, and the dynamics of attention. The next step is a direct test of the total model.

## Appendix

### *Data Sources*

Data from the 1988 Canadian National Election Study, funded by the Social Sciences and Humanities Research Centre (SSHRC), were collected by the Institute for Social Research (ISR), York University for Richard Johnston, André Blais, Henry E. Brady and Jean Crête. Data from the 1993 Canadian Election Study were provided by the ISR. The survey was funded by the SSHRC and was completed for the 1992/93 Canadian Election Team of Johnston, Blais, Brady, Elisabeth Gidengil and Neil Nevitte. Data for the 1997 Canadian Election Study were provided by the ISR. The survey was funded by the SSHRC and was completed for the 1997 Canadian Election Team of Blais, Gidengil, Richard Nadeau and Nevitte. Data from the 2000 Canadian Election Study were collected by the ISR and the Jolicoeur & Associates for Blais, Gidengil, Nadeau and Nevitte. The survey was funded by the SSHRC, Elections Canada and the Institute for Research on Public Policy. The 2004 Canadian Election Study was funded by the SSHRC in partnership with Elections Canada and the Institute for Research on Public Policy. The principal co-investigators of the study were Blais, Gidengil, Nevitte, Joanna Everitt and Patrick Fournier.

For the NZES 1996, the principal investigators were Jack Vowles, Peter Aimer, Helena Catt, Raymond Miller, Susan Banducci, Jeffrey Karp, and David Denmark. Funding for the 1996 NZES was provided for by Foundation for Research, Science, and Technology (FRST), the Waikato School of Social Sciences Research Committee and the University of Waikato, University of Auckland Research Committees and Lottery Science. For the NZES 1999, the principal investigators were Vowles, Aimer, Miller, Banducci, Karp, and Ann Sullivan. Funding for the 1999 NZES was provided for by Foundation for Research, Science, and Technology (FRST) and the University of Waikato. For the NZES 2002, the principal investigators were Vowles, Aimer, Miller, Banducci, and Karp. Funding for the 2002 NZES was provided for by Foundation for Research, Science, and Technology (FRST).

For the BES 2001, the principal investigators were David Sanders, Paul Whiteley, Harold Clarke and Marianne Stewart. The survey was funded by the Economic and Social Research Council.

For the NAES 2000, the principal investigators were Richard Johnston, Kathleen Hall Jamieson and Michael G. Hagen. The NAES was funded by the Annenberg Policy Center.

### *Modeling and Measurement Details*

Note: Sociodemographics; weight. All variables are dummy variables unless otherwise indicated.

*Canada, 1988, 1993, 1997, 2000, 2004:* Woman, Age (>55 yrs.=1), Non-European Ethnicity, French Speaker, Catholic, Non-religious, West, Quebec, Atlantic, Degree, Unemployed, Union member/household, Income (scaled 0,1); wt1, cpsnwt1, cpsnwt1, cesnwt, cesnwt.

*New Zealand, 1996:* Age (scalar), Woman, Homeowner, Working Class Self-Identification, Degree, Union member/household, Public Sector Employee, Unemployed, Income (scaled 0,1); nqwt.

*New Zealand, 1999:* Age (scalar), Woman, Maori, Degree, Union member/household, Public Sector Employee, Manual Worker, Farmer, Unemployed, Income (scaled 0,1); ncwt.

*New Zealand, 2002:* Age (scalar), Woman, Union member/household, Manual Worker, Unemployed, Income (scaled 0,1); fcawt.

*Britain, 2001:* Age (scalar), Woman, Homeowner, Southeast, Southwest, Midlands, North, Wales, Scotland, Working Class (objective measure), Unemployed; weight.

*United States, 2000:* Male, Black, Evangelical, Union member/household, Unemployed, Income (scaled 0,1); (no weight).

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Table 1. Predicted Campaign Effects by Theoretical Assumptions

	Campaign Economic Discourse					
	Raw Data		Contextualizing		Mixed	
Partisan Bias / Attention Increases	Shortcut	Bias	Shortcut	Bias	Shortcut	Bias
General or Nil	(1) Low, Equal, -	(2) High, Equal, Ø	(3) High, Equal, +	(4) High, Equal, +	(5) Equal, Equal, Ø	(6) High, Equal, +
Low Info. Only	(7) Low, Low, -	(8) High, Equal, Ø	(9) High, Low, +	(10) High, Low, +	(11) Equal, Equal, Ø	(12) High, Low, +

Note: Cell entries indicate predicted (i) information level with highest initial effect, (ii) relative magnitude of effects across information levels, and (iii) direction of effect.

Table 2. *INCPID* Effects on *NEP* and the Campaign: General Model

	<u>Static Model</u>	<u>Dynamic Model</u>		
	<i>INCPID</i>	<i>INCPID</i>	<i>INCPID</i> *DAY	<i>N</i>
Canada				
1988	<b>0.0738</b> 0.0115	<b>0.0645</b> 0.0209	0.0004 0.0007	2803
1993	<b>0.0377</b> 0.0119	0.0256 0.0221	0.0005 0.0009	3004
1997	<b>0.0690</b> 0.0167	<b>0.0710</b> 0.0406	-0.0001 0.0015	3157
2000	<b>0.0658</b> 0.0184	<b>0.0799</b> 0.0268	-0.0007 0.0015	3026
2004	<b>0.1130</b> 0.0159	<b>0.1483</b> 0.0353	-0.0017 0.0016	3519
New Zealand				
1996	<b>0.1166</b> 0.0181	<b>0.1498</b> 0.0354	-0.0019 0.0016	1328
1999	<b>0.0771</b> 0.0207	0.0552 0.0411	0.0014 0.0018	1180
2002	<b>0.0725</b> 0.0177	0.0297 0.0295	<b>0.0025</b> 0.0014	1485
United Kingdom				
2001	<b>0.2102</b> 0.0066	<b>0.2089</b> 0.0145	0.0001 0.0008	4602
United States				
2000	<b>0.0369</b> 0.0052	<b>0.0214</b> 0.0084	<b>0.0005</b> 0.0002	17382

Note: Main cell entries are OLS estimates. Robust standard errors below. Coefficients in bold significant at .10 or better.

Table 3. *INCPID* Effects on *NEP* and the Campaign: Estimates by Information Level

	High Information				Low Information			
	Static Model		Dynamic Model		Static Model		Dynamic Model	
	<i>PID</i>	<i>PID</i>	<i>PID*DAY</i>	<i>N</i>	<i>PID</i>	<i>PID</i>	<i>PID*DAY</i>	<i>N</i>
Canada								
1988	<b>0.0963</b>	<b>0.0831</b>	0.0005	1524	<b>0.0428</b>	0.0433	-0.0001	1273
	0.0154	0.0299	0.0010		0.0163	0.0267	0.0008	
1993	<b>0.0697</b>	<b>0.0477</b>	0.0010	1088	0.0223	-0.0074	0.0012	1767
	0.0197	0.0394	0.0015		0.0141	0.0277	0.0011	
1997	<b>0.0688</b>	<b>0.0956</b>	-0.0013	1577	<b>0.0689</b>	0.0492	0.0010	1580
	0.0219	0.0541	0.0023		0.0256	0.0501	0.0022	
2000	<b>0.0756</b>	<b>0.1189</b>	-0.0022	2069	0.0361	-0.0022	0.0020	957
	0.0232	0.0302	0.0015		0.0371	0.0626	0.0036	
2004	<b>0.1297</b>	<b>0.2060</b>	<b>-0.0040</b>	1132	<b>0.1002</b>	<b>0.1165</b>	-0.0008	2386
	0.0285	0.0537	0.0022		0.0187	0.0397	0.0017	
New Zealand								
1996	<b>0.1922</b>	<b>0.2049</b>	-0.0006	333	<b>0.0882</b>	<b>0.1318</b>	-0.0026	942
	0.0393	0.0647	0.0026		0.0197	0.0371	0.0017	
1999	<b>0.1530</b>	0.1088	0.0027	219	<b>0.0623</b>	0.0441	0.0012	952
	0.0448	0.0926	0.0046		0.0236	0.0427	0.0020	
2002	<b>0.0833</b>	<b>0.0882</b>	-0.0003	366	<b>0.0719</b>	0.0221	0.0029	1013
	0.0256	0.0368	0.0020		0.0229	0.0405	0.0018	
United Kingdom								
2001	<b>0.2367</b>	<b>0.2437</b>	-0.0004	2679	<b>0.1722</b>	<b>0.1576</b>	0.0009	1916
	0.0089	0.0194	0.0011		0.0107	0.0249	0.0014	
United States								
2000	<b>0.0661</b>	0.0414	0.0006	3112	0.0174	-0.0135	0.0008	3609
	0.0102	0.0333	0.0007		0.0108	0.0226	0.0005	

Note: Main cell entries are OLS estimates. Robust standard errors below. Coefficients in bold significant at .10 or better.

Figure 1. Dynamics of Partisan Bias in NEP by Information Level by Election

