

# Testing the Reciprocal Effects of Campaign Participation

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**Abstract** Questions persist regarding the robustness of cross-sectional estimates of effects of variables that are themselves endogenous to the participation process. On one hand, the consequences of working on a campaign have interesting implications for democratic society. Less benign, however, is the possibility that failure to control for reciprocal processes leads to biased estimates of the causes of campaign participation. I use a panel of Democratic and Republican contributors interviewed following each of the past three presidential elections (1996, 2000, and 2004) to explore the relationships between campaign participation and three variables typically parameterized as predictors of participation: receiving a contact, ideological extremism, and strength of party identification. The effect of strength of party identification on campaign participation proves robust; however, I find that nearly all of the associations between contacts and participation and ideological extremism and participation appear to extend from, not into, participation and past participation.

**Keywords** Campaign participation · Contact · Partisanship · Ideological polarization · Panel data · Structural equations · Reciprocal causation · Extremism

## Introduction

While there have been numerous empirical studies of the causal determinants of voting behavior and other acts of political participation, political scientists have virtually ignored the *consequences* of such activity for the individual. (Finkel 1985, p. 891, emphasis in original).

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The literature investigating the cross-sectional sources of variation in campaign participation ranks among political science's most developed (e.g. Rosenstone and Hansen 1993, Verba et al. 1995, and the citing literature). These studies describe the workings of important models of campaign activism, such as the socio-economic status (SES) model, the civic skills model, and the strategic mobilization model, answering many, if not most, questions about why some individuals get involved in electoral campaigns while others do not. However, disentangling habitual participation from freshly inspired participation poses problems when estimating the cross-sectional predictors of participation. And it appears this concern is non-trivial because most campaign activists are returning to activity, not participating in their first campaign (Carsey and Layman 1999; Miller and Jennings 1986; Rapoport and Stone 1994). Moreover, being an activist can affect the very attitudes thought to cause campaign activism and, as the opening quote illustrates, relatively little attention has been paid to these reciprocal processes. On one hand, the consequences of working on a campaign have interesting implications for democratic society. For example, Finkel (1985) demonstrates external efficacy not only inspires participation, but participation also inspires external efficacy. Less benign, however, is the possibility that failure to control for reciprocal processes leads to biased estimates of the causes of campaign participation.

Though participation in political campaigns is relatively rare (most forms of campaign participation tracked in the National Election Studies involve less than 10% of the electorate), most scholars agree activists wield far more influence in elections and national policy than their numbers warrant. For example, Carmines and Stimson (1989) and Carmines and Woods (2002) describe activists as catalysts of evolutionary change in public opinion. Hence a study of activists focuses on a relatively small, but extremely important, subgroup of the electorate. Activists' disproportionate influence derives from a basic difference between electioneering and voting: campaign activism, unlike voting, does not have an upper bound on volume. While one person, one vote governs influence in the voting booth, few such restrictions exist prior to elections. If one conceives of campaign participation as more intense political expression—compared to voting alone—it is not surprising that activists also tend to be more intense in their political attitudes. Focusing on the predictors of activism, the previous literature identifies important differences between the composition of campaign activists compared to the composition of the mass public and raises concerns about representational fairness and equality (Green and Guth 1989; Verba et al. 1995; West 1988). And more recently, unusual extremes of political activity and political attitudes, coupled with high levels of influence, have led some political scientists to question whether democratic societies can sustain high levels of civic involvement without threatening other democratic values, such as tolerance (Mutz 2006).

But questions related to direction of causation, and especially the consequences of campaign participation, have received less attention. Understanding the consequences of activism is important statistically because reciprocal causation can lead to biased estimates of the effects of predictors. Substantively, a study of the consequences of activism reveals new dimensions to recruitment and the attitudes associated with campaign participation. For example, the normative implications of

ideologically extreme activists are quite different if extremism is a product of, not a reason for, participation. But whether activists come to campaigns with extreme attitudes or whether their attitudes intensify during the campaign, remains an open question. In either case, in a cross-section, activists still appear more intensely ideological when compared to the general public. But surely ideological extremism deriving from involvement in the democratic process need not arouse as much normative concern as extremism deriving from religious fanaticism or racial bigotry.

In what follows, I use a panel of Democratic and Republican contributors interviewed following each of the past three presidential elections (1996, 2000, and 2004) to explore the relationships between campaign participation and three variables typically parameterized as predictors of participation: receiving a contact soliciting campaign support, ideological extremism, and strength of party identification.<sup>1</sup> These three were selected because each appears robustly related to campaign participation; but in contrast to stable characteristics, such as race or sex, their exogeneity vis-à-vis the participatory process is highly dubious (Leighley 1995). Using a three-wave panel design I am able to estimate simultaneous structural equations models to test hypotheses about direction of causation. This technique promises to shed new light on questions about direction of causation because, while cross-sectional research designs can be employed in the study of reciprocal causation, “success of these methods... depends on the model satisfying several restrictive assumptions” (Finkel 1995, p. 23). The chief difficulty of using instrumental variables to “purge” models of reciprocal effects lies in finding variables that are related to the regressor in question, but unrelated to the dependent variable. “Three-wave and multiwave panels, however, can be estimated by imposing fewer constraints on the causal parameters” (Finkel 1995, p. 24). Estimating systems of structural equations simultaneously enables me to investigate, rather than purge, the reciprocal processes.

To preview my results, the association between strength of party identification and participation proves robust. However, I find that nearly all of the associations between contacts and participation, and ideological extremism and participation appear to extend from, not into, participation and past participation. These findings have important implications for the way political scientists study campaign involvement and I conclude by returning to the theme of potential trade-offs

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<sup>1</sup> The data come from Walter J. Stone’s and Ronald B. Rapoport’s, *Party Leadership and Presidential Selection Survey*. Prior to the 1996 presidential election, they obtained samples of contributors to the Democratic and Republican National Committees who had received a direct mail appeal between October 1994 and October 1995 (the average contributor in these groups contributed less than \$50). These were surveyed following the elections in 1996, 2000, and again in 2004. To be sure, not every activist contributes to the national parties, but it is difficult to conceive of a better group to sample if one wants a sample that is representative of activists in the mass public. Convention delegates, for example, are probably more involved than activists in the mass public, on average, because being a delegate requires an arduous selection procedure and convention attendance requires expensive travel. Lists from individual candidates enumerate activists in the mass public, but the groups may be unique in idiosyncratic ways related to the specific candidates. In short, the *Party Leadership and Presidential Selection Survey* follows a unique approach to gain new insights into a group that is difficult to enumerate.

between civic involvement and democratic values, such as moderation and tolerance. But because it runs contrary to a voluminous empirical literature, I want to make the case early that the null contact → participation finding is both plausible and interesting. First, the panel research design departs from the cross-sectional task of distinguishing non-activists from activists. Rather, in panel research, one attempts to distinguish patterns of change shared among the variables. Focusing on change makes problems associated with measuring contact more important because the people who wrote checks or spent an evening at a campaign headquarters making phone calls all had contact with campaigns (by definition). So one reason to expect lackluster associations in panel analyses has to do with the difficulty activists have distinguishing between involvement-related connections they have with campaigns and solicitation contacts they have prior to working for campaigns. While this measurement problem would tend to inflate the estimated association in a cross-section, it will tend to attenuate the association in a panel because of lack of change in contact among participants who increase or lower, but do not quit, campaign activism.

More generally, the three-wave panel design enables me to correct for random measurement error (note the contact measurement problem discussed above would be non-random) and test the robustness of relationships running from each “predictor” to participation having specified lagged effects, cross-lagged effects, and reciprocal effects. Similarly detailed attention to direction of causation and the relationship between efficacy and participation inspired interesting studies of each (see Finkel 1985 and the citing literature). This study of political contacts and two central political attitudes, likewise, reveals interesting avenues for future participation studies.

### **Predicting Participation and Dealing with its Consequences**

Rosenstone and Hansen (1993) estimate recruitment (receiving a contact from the parties) increases the likelihood of attempting to persuade others how to vote by nearly 12%, donating money nearly 7%, and working on a campaign nearly 5%. They estimate the analogous effects of strength of party identification at 8%, 5%, and 3%, respectively. Verba et al. (1995) report similar effects for recruitment and strength of party identification in their models predicting overall participation (a nine point additive scale) and they also find extremeness of abortion attitudes exert a statistically significant effect. Likewise Saunders and Abramowitz (2004), Gershtenson (2002), and Verba and Nie (1972) find ideological extremism exerts statistically significant effects on campaign participation. To be sure, political attitudes (measured by strength of party identification and ideological extremism) and recruitment all appear to be motivational factors in individuals’ participation calculus and the cross-sectional associations are impressive.

However, it seems equally plausible that political involvement affects one’s attitudes and the extent of one’s contacts with parties and candidates. Indeed anyone who has mailed a check to a political campaign knows that the act is followed by a

marked increase in the volume of mail and phone calls.<sup>2</sup> And, in fact, the empirical evidence of causation in the opposite direction is also quite impressive. These studies reveal potential bias by demonstrating that predictors of participation are themselves affected by participation. But, as the opening quote suggests, these studies also pose interesting questions about the political implications of participation. For example, Brady et al. (1999) find that the single best predictor of recruitment is participation in past campaigns. In fact the standardized regression coefficient for past participation is more than double the next most robust predictor, education.

Turning to the attitudinal effects of participation, Freie (1997) experimentally exposed a group of students to campaign work and compared change in their political attitudes to a control group of students who did not work on campaigns. He found dramatically more attitudinal change among the campaign workers. McCann (1995) finds that helping the nomination campaign for extreme candidates leads to ideological polarization in participants. More generally, social psychologists have long held that participation in groups leads to attitude polarization (Isenberg 1986; Sunstein 2000, 2002).

Finally, even the exogenous status of party identification appears not to be sacrosanct. Reflecting on work comparing the processes that give rise to the acquisition and the intensification of partisanship, Claggett concludes, “[I]f a theoretical understanding of the development of partisanship is to be developed, we need to... begin to test explicit hypotheses about the processes which lead individuals to acquire or intensify a party tie” (1981, p. 212). Although party identification is often characterized as the unmoved-mover of politics, even the “Michigan School” distinguishes between acquisition of party identification and intensity of identification, and posits that the latter varies with political involvement. Granted, the authors of *The American Voter* maintained that political experiences would strengthen identification throughout individuals’ lives because their political experiences would, in turn, be colored by partisanship (Campbell et al. 1960, pp. 161–164). But the intensity dimension was not characterized as unmovable and, in fact, Converse published extensively demonstrating that the intensity of party identification was not fixed within the context of the historical period in which an individual came of age; rather he showed that intensity of party identification increased with political experience.<sup>3</sup> Of course it can be difficult to disentangle life stage effects from experiential effects, but Wong (2000) develops a research design to exploit the fact that immigrants lack previous experiences even though they span the range of life stages. She finds political exposure to be the key variable determining intensity of party attachments. On the whole then, it seems clear that each of these variables has been studied as both a cause and an effect of participation.

Unfortunately, the reciprocal nature of these relationships may pose problems vis-à-vis obtaining unbiased estimates in cross-sectional data. In a cross-section it is difficult to tell whether people who were contacted participated because of the

<sup>2</sup> See Koger et al. (2005) for an interesting study of the dissemination of donors’ contact information.

<sup>3</sup> Political experience was proxied by age. See Converse (1969, 1976).

contact or whether they were contacted because they were more likely to participate (Brady et al. 1999). In other words activists may have participated even if they had not been contacted. To the extent this is the case, but parties and candidates tend to call past participants just to be safe, cross-sectional estimates of the effect of contacting may be biased, overestimating the effect of contacting. Fortunately, alternative research strategies exist for exploring causal paths. For example, Finkel (1985) demonstrates how studying direction of causation more deliberately can illuminate interesting relationships among variables by using a three-wave panel and estimating simultaneous structural equations specifying the reciprocal relationships between participation and two types of efficacy, internal and external. He demonstrates both types of efficacy inspire participation and that participation, in turn, inspires external efficacy. However, once the reciprocal effects are specified, participation appears not to inspire internal efficacy.

While the reciprocal relationship between campaign participation and contact, and campaign participation and ideological extremism have received some attention in the literature, I can find no previous work regarding the reciprocal relationship between campaign participation and strength of party identification. Furthermore, none of these three reciprocal effects has received as much scrutiny as the reciprocal relationship between campaign participation and efficacy (Finkel 1985). For example, Abramson and Claggett (2001), Brady et al. (1999), and Grant and Rudolf (2002), each deploy an instrumental variable technique in attempt to obtain unbiased estimates of the effect of contacts on campaign participation. And, while each find statistically significant, albeit smaller, effects; the use of instrumental variables in cross-sectional data requires several important assumptions about the instrument: it must be highly predictive of X, it must be causally unrelated to Y, and it must be uncorrelated with the error term (see Finkel 1995, p. 33). While these studies illuminate new information about the participation/contact relationship that previous cross-sectional studies did not, a study that is not dependent on an instrumental variable approach (and the attendant assumptions) promises to reveal still more about the relationships.

McCann's (1995) investigation of the relationships among participation and ideological extremism comes closer to the level of scrutiny deployed in Finkel's analysis, but several important questions remain unanswered. McCann's study focuses primarily on nomination politics and campaign activity for unusually extreme candidates. McCann (1995) uses a panel study to estimate simultaneous equations of the effect of participating in the nomination phase on ideological extremism and vice-versa, and he finds evidence of a robust path from participation to extremism while the path in the other direction fails to reach statistical respectability. However, the present study involves representative samples of Democratic and Republican contributors (rather than caucus attendees) and the panel spans three presidential elections. Hence this investigation will test whether the McCann findings generalize to participation for more conventional candidates over the course of multiple general elections.<sup>4</sup>

<sup>4</sup> In addition, the three-wave models enable me to study both the reciprocal *and* cross-lagged effects while also correcting for measurement error.

How critical are direction of causation questions? Leighley, reviewing the state of the participation literature, comments on the importance of deploying panel data to explore questions relating to direction of causation,

Our reliance on cross-sectional survey data in studying participation has precluded development of participation models wherein participation decisions are made over time... [and] our current models require strong assumptions regarding *causal ordering* as well as the validity and reliability of individuals' self-reported incentives for participating... A more ambitious research agenda, however, would be to develop panel data on individuals' attitudes, opportunities (i.e., mobilization) and incentives to participate, along with their experiences with prior participatory acts (e.g. knowledge gain, satisfaction). This would provide not just more data, but a fundamentally *different* type of data, for the different questions which must now be addressed. (1995, 198, first emphasis added)

In what follows, I emulate Finkel's study of campaign participation and efficacy by using a three-wave panel study and estimating simultaneous structural equations specifying the reciprocal relationships and correcting for measurement error.

**Data and Methods**

Using Walter J. Stone's and Ronald B. Rapoport's, *Party Leadership and Presidential Selection Survey*, I analyze longitudinal variation in participation in a panel consisting of representative samples drawn from the databases of contributors to the Democratic and Republican National Committees. This sample was interviewed following the elections in 1996, 2000, and again in 2004.<sup>5</sup> To measure participation in each wave, I form additive scales combining respondents' reported involvement (convincing friends, working in an office, canvassing, contributing money, writing letters of support, and other activities) in a variety of possible campaigns (congressional candidates, presidential candidates, and/or the party ticket). Since the survey allows me to track six activities for each of three

<sup>5</sup>

Year	DNC N	DNC response rate	RNC N	RNC response rate
1996	666	46.4%	789	39.9%
2000	419	62.9%	494	62.6%
2004	167	39.9%	216	43.7%

For an analysis of attrition and potential response bias, see Appendix A. Lisrel's full information maximum likelihood estimation option enables me to use all respondents who participated in all three waves of the study, for an N of 383. While which party individuals identify with is not a factor in my models, I note that the ratio of Democrats to Republicans in these data approximates the analogous ratio of Democratic contributors to Republican contributors in the NES (in most presidential election years, 1952–2004). I pool them in my analyses, but I note that the partisan balance in this sample reflects the partisan balance of individual contributors in the electorate. For more information see *The Presidential Selection Study* website: <http://pss.iga.ucdavis.edu/>

campaigns, maximum election involvement for each set of partisans is 18 activities (though the observed maximums do not exceed 15).<sup>6</sup> Contact in each wave takes on values of 0 or 1 based on the respondent's report of having received a personal or phone contact from a party or from a candidate during the campaign. Ideological extremism and strength of party identification each range from 0–3 and were formed by folding seven-point scales at their midpoints.<sup>7</sup>

To explore the relationships among these variables, I estimate three sets of structural equations for each variable paired with participation (e.g. participation and contact, participation and ideological extremism, and participation and strength of party identification). The participation and contact models are diagrammed in Fig. 1 (the models for the other two pairs are identical in specification). In Panel A of Fig. 1, c96, c00, and c04 each represent the contact variable from a wave of the study and serve as a single indicator for their latent variables,  $\eta_1$ ,  $\eta_2$ , and  $\eta_3$ . Likewise p96, p00, and p04 each represent the participation variable from a wave of the study and serve as a single indicator for their latent variables,  $\eta_4$ ,  $\eta_5$ , and  $\eta_6$ .<sup>8</sup> The use of observed indicators, measured at three different times, enables me to correct for random measurement error in obtaining the structural effects linking the latent variables.<sup>9</sup>

While each of the relationships between participation and each of the other three variables (contact, ideological extremism, and strength of party identification), their

<sup>6</sup> While the combined scale no doubt misses some differences in the determinants of each act, I ran separate models for “convincing friends” and “contributing money” and the estimates were very similar to the models based on the combined scale. I report only the results of the combined scale analyses because the underlying processes that I study do not appear to differ dramatically across the various acts and because total involvement captures variation in participation in ways that separate models of each act would miss. For example, an individual could forego contributing money one year, but still increase participation by adding other acts.

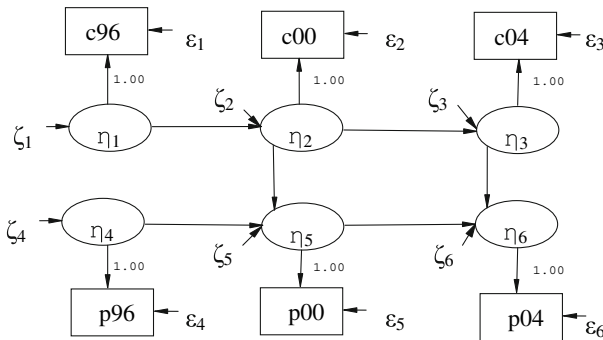
<sup>7</sup> See Appendix B for descriptive statistics.

<sup>8</sup> The error variances of the observed indicators are represented by the  $\varepsilon$  terms and the structural disturbances of the latent variables are represented by the  $\zeta$  terms. I follow the single indicator procedure for three-wave data described in Wiley and Wiley (1970). Wiley and Wiley identify the single indicator model by constraining the error variances to equality in each wave  $\varepsilon_1 = \varepsilon_2 = \varepsilon_3$  and  $\varepsilon_4 = \varepsilon_5 = \varepsilon_6$  and setting each of the structural coefficients linking the latent variables to their observed indicators to one (which also ensures the units of the latent variables retain the units of the observed indicators). In addition, I impose several “consistency” constraints to gain additional degrees of freedom. Finkel (1995) recommends setting the structural coefficients to equality when panel waves are equally spaced (as these are). For example, I set the structural coefficient linking c00  $\rightarrow$  p00 equal to that of c04  $\rightarrow$  p04. I do the same for the lagged effects (e.g. c96  $\rightarrow$  c00 is set equal to c00  $\rightarrow$  c04), the cross-lagged effects, and the reciprocal effects.

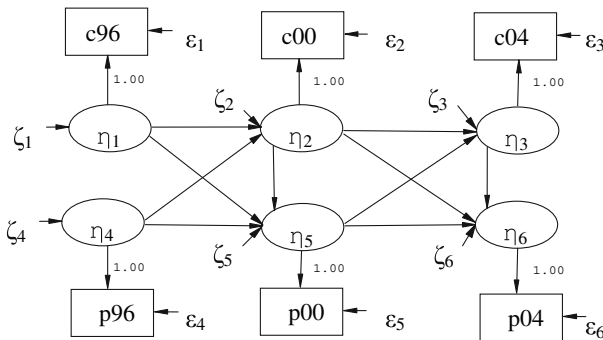
<sup>9</sup> Finkel (1995) describes this approach to handling measurement error problems,

When an indicator with random measurement error is used as an independent variable in a regression equation, the result is biased estimation of the true causal effect of the latent variable...[a]lthough measurement error leads to serious problems in panel analysis, it can also be handled much more easily than in the cross-sectional context... In panel designs, the repeated measurement of the indicators over time increases the power of this [error correction] approach considerably, as additional waves of data provide more information with which to estimate relevant structural and measurement coefficients. In fact, measurement properties and structural effects in models with only one indicator of a latent variable can be estimated with at least three waves of data, and thus all multiwave panel models can be treated as variants of the multiple indicators [error correction] approach (47–49).

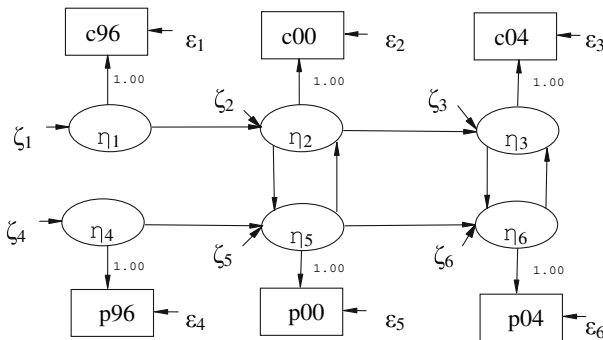
Panel A: One-Way Effect Model



Panel B: Cross-Lagged Effects Model



Panel C: Reciprocal Effects Model



**Fig. 1** Path diagrams of three-wave, error correction models testing the conventional effect of contact on participation

lags, cross-lags, and reciprocal effects are interesting substantively, I focus on testing the robustness of the structural coefficients leading into participation. In Fig. 1, Panel A, the cross-lagged effects and the reciprocal paths leading into contact are all constrained to zero. In this model the direction of causation is assumed; thus the first model approximates cross-sectional models of participation, in many respects. However, the three-wave panel is exploited to correct for random measurement error, as described above. Hence the test in Panel A, which I call the “one-way effect model,” indicates whether the cross-sectional relationships are biased by random measurement error in the variables. In the second panel of Fig. 1, I remove the constraint on the cross-lagged effects. I call the model in Panel B the “cross-lagged effects model,” but note that I have added cross-lagged effects to the one-way model. Hence the models are nested, with the cross-lagged model serving as a less constrained version of the one-way model. In the third panel of Fig. 1, I remove the constraint on the reciprocal path leading into participation. I call the model in Panel C the “reciprocal effects model,” and like the cross-lagged model, it is a less constrained version of the one-way model.

The key test, then, is whether the path leading into participation remains robust in each of the three models pictured in Fig. 1. More formally, the models are nested, therefore fit can be compared statistically to determine whether relaxing constraints significantly improves model fit.<sup>10</sup> The tests will indicate whether cross-lagged effects or reciprocal effects significantly improve model fit. Moreover, sensitivity to specification changes in the one-way effect will indicate whether omission of cross-lagged effects or reciprocal effects lead to biased estimates.

## Contact Results<sup>11</sup>

Turning first to the relationships between contact and campaign participation, the one-way effect model in Table 1, appears to confirm a significant relationship running from contact to campaign participation. The unstandardized coefficients (1.12, constrained to equality across waves), more than twice exceed their standard errors and indicate receiving a contact during the election campaign increases

<sup>10</sup> Fit for each model is measured by the chi-square statistic, with higher values (relative to the number of degrees of freedom) indicating worse fit (see Finkel 1995). The models are compared by subtracting the chi-square of the unconstrained model from that of the constrained model. The degrees of freedom for this comparison are equal to the difference of the degrees of freedom of the two models (Bollen 1989). The null hypothesis in these tests is that model fit for both the constrained and unconstrained models are equal.

<sup>11</sup> Tables 1–3 present the structural effects for each of the three model specifications shown in Figure 1, for participation paired with each of the other three variables. In each table the structural coefficients are divided into stabilities, cross-lagged effects, and reciprocal effects. The stabilities reflect the relationship between each variable and its lag in the previous period. The cross-lagged effects reflect the relationships between each variable and previous values of the other variable. Finally, the reciprocal effects include synchronous effects running both from participation to the other variable and from the other variable into participation. Since the path leading into campaign participation represents the traditional, cross-sectional association between the variables, the one-way model and the cross-lagged model only include half of the reciprocal effects—those running into participation. However, reciprocal effects in both directions are estimated in the third column of each table, the reciprocal effects model.

**Table 1** Three-wave models with measurement error for the contact-campaign participation relationship, 1996–2000–2004

	One-way Effect model	Cross-lagged Effects model	Reciprocal Effects model
<i>Stabilities<sup>a</sup></i>			
C <sub>96</sub> → C <sub>00</sub>	.49/.54*** (.15)	.24/.25 (.19)	.27/.29 (.17)
C <sub>00</sub> → C <sub>04</sub>	.57/.54*** (.15)	.26/.25 (.19)	.30/.29 (.17)
P <sub>96</sub> → P <sub>00</sub>	.66/.71*** (.07)	.66/.71*** (.07)	.69/.75*** (.07)
P <sub>00</sub> → P <sub>04</sub>	.74/.71*** (.07)	.74/.71*** (.07)	.79/.75*** (.07)
<i>Cross-lagged effects<sup>b</sup></i>			
C <sub>96</sub> → P <sub>00</sub>	–	.03/.15 (.17)	–
C <sub>00</sub> → P <sub>04</sub>	–	.03/.15 (.17)	–
P <sub>96</sub> → C <sub>00</sub>	–	.13/.03** (.01)	–
P <sub>00</sub> → C <sub>04</sub>	–	.14/.03** (.01)	–
<i>Reciprocal effects<sup>c</sup></i>			
C <sub>00</sub> → P <sub>00</sub>	.18/1.12** (.35)	.10/.46 (.37)	.04/.21 (.29)
C <sub>04</sub> → P <sub>04</sub>	.18/1.12** (.35)	.10/.46 (.37)	.04/.21 (.29)
P <sub>00</sub> → C <sub>00</sub>	–	–	.17/.03** (.01)
P <sub>04</sub> → C <sub>04</sub>	–	–	.17/.03*** (.01)
$\chi^2$ (df)	39.10(10)***	30.05(8)***	31.87(9)***

*Notes:* The entries are standardized/unstandardized, structural coefficients. The models were estimated using Lisrel 8.8, full information maximum likelihood estimation, with missing values (for respondents who returned surveys in all three waves). N for all models is 383. Standard errors in parentheses. \*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$ . All tests two-tailed

<sup>a</sup> Contact<sub>96</sub> → Contact<sub>00</sub> constrained to equal Contact<sub>00</sub> → Contact<sub>04</sub> and Participation<sub>96</sub> → Participation<sub>00</sub> constrained to equal Participation<sub>00</sub> → Participation<sub>04</sub>

<sup>b</sup> Contact<sub>96</sub> → Participation<sub>00</sub> constrained to equal Contact<sub>00</sub> → Participation<sub>04</sub> and Participation<sub>96</sub> → Contact<sub>00</sub> constrained to equal Participation<sub>00</sub> → Contact<sub>04</sub>

<sup>c</sup> Contact<sub>00</sub> → Participation<sub>00</sub> constrained to equal Contact<sub>04</sub> → Participation<sub>04</sub> and Participation<sub>00</sub> → Contact<sub>00</sub> constrained to equal Participation<sub>04</sub> → Contact<sub>04</sub>

campaign activism by more than one campaign activity. The stability coefficients are all statistically significant, in the first model, and they reveal that participation is a good deal more stable than contact.

The stabilities reported in the cross-lagged model in Table 1 (results in the middle column) demonstrate that when past participation is allowed to influence subsequent contact, the stability of contact diminishes considerably. However, feedback processes have no impact on the stabilities of participation. Consistent with changes in stabilities, the cross-lagged effects reveal statistically significant paths from past participation into contact, but the paths from past contact into participation are statistically zero. Of even greater interest, the synchronous path from contact into participation no longer attains statistical respectability.

Turning to the last column, the reciprocal effects model reveals a pattern of results that is very similar to those of the cross-lagged effects model. Again, the stability of contact has diminished considerably compared to the stabilities in evidence in the one-way model, while the stability of participation actually increases slightly under the reciprocal effects specification. Furthermore, the path from contact into participation remains diminished and statistically indistinguishable from zero while the path in the opposite direction appears quite robust.

Finally, the statistical comparisons between the one-way model and each of the other two models reveal that relaxing the constraints significantly improves fit. The difference between the chi-square for the one-way model and that of the cross-lagged model is 9.05 with 2 degrees of freedom. The critical value with 95% confidence is 5.99, so I can reject the null hypothesis that the constrained model fits the data as well as the unconstrained model. The difference between the chi-square for the one-way model and that of the reciprocal effects model is 7.23 with 1 degree of freedom. The critical value with 95% confidence is 3.84, so I can again reject the null hypothesis that the constrained model fits the data as well as the unconstrained model.

Substantively the causal processes appear a bit different than those suggested in cross-sectional studies. Comparing the synchronous effects from contact to participation across the three columns, the cross-lagged and reciprocal effects models reveal bias in the one-way estimate. Failure to control for the effect of past participation on contact leads to overestimates of the synchronous effect from contact to participation. Likewise failure to control for the reciprocal effect of participation on contact leads to overestimates of the synchronous effect from contact to participation. Of course the cross-lagged model makes sense, given that past participation is sure to cement one's name in a party's or a candidate's database and, therefore, likely to increase subsequent contact. The reciprocal effect, on the other hand, makes less sense if contact is assumed to precede participation temporally. But unfortunately the two variables are measured at the same point in time, making the temporal assumption dependent upon respondents' abilities to accurately recall contacts. I suspect difficulty accurately distinguishing pre-participation contacts from the many contacts volunteers have with campaigns as a part of their involvement in campaigns drive this reciprocal process. But the finding is still important, even if it is fundamentally about problems studying the relationship. Future studies of campaign participation will likely need to reevaluate how to pose contact questions.<sup>12</sup>

<sup>12</sup> It might also be worth noting that these problems are unique to activism—the contact questions probably work quite well in terms of turnout because voting does not involve extensive interaction with a party or candidate.

## Strength of Party Identification Results

Table 2 presents the structural effects for the relationships between strength of party identification and campaign participation. The stabilities associated with the one-way model reveal that strength of party identification is considerably more stable than contact and a bit more stable than participation. The synchronous effects from strength of party identification into participation are robust and indicate that the four unit range of strength party identification is associated with about 1 additional campaign activity.

Turning to the cross-lagged model, the stabilities of both variables change only marginally with the specification change. Also the synchronous effects from strength of party identification into participation are substantially larger indicating that the four unit range of strength party identification is associated with about 5 additional campaign activities. More perplexing are the negative feedback processes described by the cross-lagged effects. The minimal effects leading from past participation into strength of party identification appear to support the notion that partisanship, even the intensity dimension, is an unmoved mover. However, the negative relationship between past strength of party identification and subsequent participation is unexpected. I suspect the negative feedback is a sort of regression effect. Strong partisans tend to do as much as they can and, relative to weak partisans, strong partisans are more likely to decrease participation in the next election simply due to the gravitational pull of the mean. Those who are above average in participation are more likely to decrease subsequently than those who are below average in participation. Once this regression effect is specified, the synchronous effect of strength of party identification is even more impressive. Furthermore the explanatory power of the model is improved significantly having specified the cross-lagged effects. The difference between the chi-square for the one-way model and that of the cross-lagged model is 9.28 with 2 degrees of freedom, so I can reject the null hypothesis that the constrained model fits the data as well as the unconstrained model.

Finally, the estimates in the last column reveal that the other reciprocal paths—those leading from participation into strength of party identification—matter very little. The reciprocal effects leading into strength of party identification are statistically zero, while the reciprocal effects leading into participation are almost identical to the analogous effects in the one-way model. Also, I cannot reject the null hypothesis that the one-way model fits the data as well as the reciprocal effects model. The difference between the chi-square for the one-way model and that of the reciprocal effects model is only .48 with 1 degree of freedom.

## Ideological Extremism Results

The results in Table 3 for ideological extremism reveal significant stabilities in the first two models and little else. Ideological extremism appears to be almost

**Table 2** Three-wave models with measurement error for the strength of PID-campaign participation relationship, 1996–2000–2004

	One-way Effect model	Cross-lagged Effects model	Reciprocal Effects model
<i>Stabilities<sup>a</sup></i>			
S <sub>96</sub> → S <sub>00</sub>	.80/.87*** (.06)	.82/.90*** (.06)	.80/.87*** (.06)
S <sub>00</sub> → S <sub>04</sub>	.88/.87*** (.06)	.90/.90*** (.06)	.88/.87*** (.06)
P <sub>96</sub> → P <sub>00</sub>	.69/.75*** (.07)	.72/.80*** (.07)	.68/.74*** (.07)
P <sub>00</sub> → P <sub>04</sub>	.78/.75*** (.07)	.84/.80*** (.07)	.78/.74*** (.07)
<i>Cross-lagged effects<sup>b</sup></i>			
S <sub>96</sub> → P <sub>00</sub>	–	–.35/–1.08* (.53)	–
S <sub>00</sub> → P <sub>04</sub>	–	–.40/–1.08* (.53)	–
P <sub>96</sub> → S <sub>00</sub>	–	–.05/–.02 (.01)	–
P <sub>00</sub> → S <sub>04</sub>	–	–.05/–.02 (.01)	–
<i>Reciprocal effects<sup>c</sup></i>			
S <sub>00</sub> → P <sub>00</sub>	.10/.28** (.09)	.45/1.27* (.51)	.09/.25* (.10)
S <sub>04</sub> → P <sub>04</sub>	.10/.28** (.09)	.47/1.27* (.51)	.09/.25* (.10)
P <sub>00</sub> → S <sub>00</sub>	–	–	.02/.01 (.01)
P <sub>04</sub> → S <sub>04</sub>	–	–	.02/.01 (.01)
$\chi^2$ (df)	21.41(10)*	12.13(8)	20.93(9)*

*Notes:* The entries are standardized/unstandardized, structural coefficients. The models were estimated using Lisrel 8.8, full information maximum likelihood estimation, with missing values (for respondents who returned surveys in all three waves). N for all models is 383. Standard errors in parentheses. \*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$ . All tests two-tailed

<sup>a</sup> Strength of PID<sub>96</sub> → Strength of PID<sub>00</sub> constrained to equal Strength of PID<sub>00</sub> → Strength of PID<sub>04</sub> and Participation<sub>96</sub> → Participation<sub>00</sub> constrained to equal Participation<sub>00</sub> → Participation<sub>04</sub>

<sup>b</sup> Strength of PID<sub>96</sub> → Participation<sub>00</sub> constrained to equal Strength of PID<sub>00</sub> → Participation<sub>04</sub> and Participation<sub>96</sub> → Strength of PID<sub>00</sub> constrained to equal Participation<sub>00</sub> → Strength of PID<sub>04</sub>

<sup>c</sup> Strength of PID<sub>00</sub> → Participation<sub>00</sub> constrained to equal Strength of PID<sub>04</sub> → Participation<sub>04</sub> and Participation<sub>00</sub> → Strength of PID<sub>00</sub> constrained to equal Participation<sub>04</sub> → Strength of PID<sub>04</sub>

as stable as strength of party identification, but the synchronous effect leading into participation is statistically zero. Relative to cross-sectional studies, the one-way model suggests that much of the apparent association between

**Table 3** Three-wave models with measurement error for the ideological extremism-campaign participation relationship, 1996–2000–2004

	One-way Effect model	Cross-lagged Effects model	Reciprocal Effects model
<i>Stabilities<sup>a</sup></i>			
E <sub>96</sub> → E <sub>00</sub>	.85/.88*** (.05)	.85/.88*** (.05)	.85/.88*** (.05)
E <sub>00</sub> → E <sub>04</sub>	.86/.88*** (.05)	.86/.88*** (.05)	.87/.88*** (.05)
P <sub>96</sub> → P <sub>00</sub>	.70/.78*** (.07)	.71/.78*** (.07)	.71/.78*** (.07)
P <sub>00</sub> → P <sub>04</sub>	.81/.78*** (.07)	.81/.78*** (.07)	.81/.78*** (.07)
<i>Cross-lagged effects<sup>b</sup></i>			
E <sub>96</sub> → P <sub>00</sub>	–	–.05/–.15 (.33)	–
E <sub>00</sub> → P <sub>04</sub>	–	–.05/–.15 (.33)	–
P <sub>96</sub> → E <sub>00</sub>	–	.06/.02 (.01)	–
P <sub>00</sub> → E <sub>04</sub>	–	.06/.02 (.01)	–
<i>Reciprocal effects<sup>c</sup></i>			
E <sub>00</sub> → P <sub>00</sub>	–.05/–.15 (.10)	–.01/–.04 (.33)	–.07/–.24* (.11)
E <sub>04</sub> → P <sub>04</sub>	–.05/–.15 (.10)	–.01/–.04 (.33)	–.08/–.24* (.11)
P <sub>00</sub> → E <sub>00</sub>	–	–	.06/.02* (.01)
P <sub>04</sub> → E <sub>04</sub>	–	–	.06/.02* (.01)
χ <sup>2</sup> (df)	14.89(10)	10.42(8)	11.06(9)

*Notes:* The entries are standardized/unstandardized, structural coefficients. The models were estimated using Lisrel 8.8, full information maximum likelihood estimation, with missing values (for respondents who returned surveys in all three waves). N for all models is 383. Standard errors in parentheses. \*  $p < .05$ ; \*\*  $p < .01$ ; \*\*\*  $p < .001$ . All tests two-tailed

<sup>a</sup> Extremism<sub>96</sub> → Extremism<sub>00</sub> constrained to equal Extremism<sub>00</sub> → Extremism<sub>04</sub> and Participation<sub>96</sub> → Participation<sub>00</sub> constrained to equal Participation<sub>00</sub> → Participation<sub>04</sub>

<sup>b</sup> Extremism<sub>96</sub> → Participation<sub>00</sub> constrained to equal Extremism<sub>00</sub> → Participation<sub>04</sub> and Participation<sub>96</sub> → Extremism<sub>00</sub> constrained to equal Participation<sub>00</sub> → Extremism<sub>04</sub>

<sup>c</sup> Extremism<sub>00</sub> → Participation<sub>00</sub> constrained to equal Extremism<sub>04</sub> → Participation<sub>04</sub> and Participation<sub>00</sub> → Extremism<sub>00</sub> constrained to equal Participation<sub>04</sub> → Extremism<sub>04</sub>

ideological extremism and participation is an artifact of random measurement error. And the cross-lagged effects model only weakens the relationship further (though the formal model comparison indicates no significant improvement in fit).

Turning to the last column in Table 3, the difference between the chi-square for the one-way model and that of the reciprocal effects model is 3.83 with 1 degree of freedom, only one one-hundredth below the critical value for 95% confidence. However, contrary to the association found in cross-sectional studies, the path leading into participation is negative and the path leading into ideological extremism is positive. Although the effects of ideological extremism on participation run counter to those reported in previous cross-sectional studies, this pattern of effects is consistent with the one described in McCann (1995). In his study the path leading from participation into ideological extremism was positive and the path going in the opposite direction was negative. Participation appears to lead to ideological extremification, and controlling for that process, extreme individuals actually participate a bit less than moderate ones. However, the lackluster model comparisons suggest a more cautious interpretation might be in order. Both participation and ideological extremism reveal considerable temporal stability and only weak associations with each other.

## Discussion and Conclusions

Having investigated the direction of relationships among participation and three variables that have been robust predictors of participation in previous cross-sectional studies, I am now in a position to discuss how these predictors of participation fared after modeling random measurement error, cross-lagged effects, and reciprocal effects. First, with respect to the contact-participation relationships, I uncover processes that likely bias cross-sectional estimates. While the one-way contact-participation model shows a significant association, even after correcting for random measurement error, the diminished and imprecise effect in the cross-lagged effects model and the reciprocal effects model indicates failure to specify these processes leads to overestimates of the synchronous effect of contact on participation. Granted, it is important to remember the sample only includes activists and it is entirely possible that the synchronous association between contact and participation would be stronger among less seasoned activists. But it is equally important to remember that most activists in any given election are repeat players, like the respondents considered here, and that the parties invest a lot of resources contacting repeat players. A practical implication of this work is to suggest parties and candidates might do well to focus more energy on recruiting new activists, knowing activists in their database are almost equally likely to return with or without additional encouragement.

Another potential problem these findings uncover relates to the way contacts are measured. Asking respondents whether they were contacted by a party or candidate during the campaign probably works quite well for studying questions related to

turnout, even if both turnout and contact are measured during the same survey. However, learning about whether campaign activists were recruited is a more thorny enterprise. Unfortunately, participating in a campaign involves a lot of contact with the campaign in question. Moreover, participation can take place at any point in time before the election making it difficult to assess contact and participation in separate survey waves. This is not a problem with turnout, because most people vote in private, on election day, and contacts they have with people at polling places are not likely to be confused with contacts from campaigns (because the polling places are not operated by the parties or candidates). At the very least, questions about campaign activists' recruitment probably need to be worded more clearly to help respondents disentangle the contacts they had with the campaign as participants from the contacts they may or may not have received asking them to get involved in the first place.

Of the three participation-variable pairings, the cross-sectional association between strength of party identification and participation seems least affected by relaxing constraints on cross-lagged effects or reciprocal effects. In terms of the cross-sectional estimates, my cross-lagged effects model indicates failure to control for cross-lagged effects actually attenuates the synchronous effect of strength of party identification on participation (if there is a cross-sectional bias, it is in the direction of underestimation). If there is a surprise in Table 2, it is that campaign participation fails to strengthen partisanship. Combined these results suggest parties need to cultivate strong party attachments and that attachments are unlikely to strengthen exclusively in the context of campaign activity (e.g. perhaps parades and ice cream socials are important party-building activities). However, as a caveat, I need to recognize that this may be a ceiling effect, since the sample only includes seasoned activists, and activists tend to be strong partisans. This raises an interesting empirical question for future research. If one could construct a similar panel consisting only of first-time activists, it seems plausible that participation might intensify partisanship among newcomers, but that the effect might decay as they become seasoned activists.

Finally, the robustness of cross-sectional ideological extremism effects on participation appear highly suspect among habitual activists. If anything, consistent with findings from social psychology (see Isenberg 1986; Sunstein 2000, 2002), participation appears to polarize ideology. For scholars concerned about activists' high levels of influence and democratic representation, evidence that ideological extremism does not stimulate additional participation among habitual activists should be reassuring. However, for those concerned about democratic values, these results are, at once, both reassuring and troubling. On one hand, activists' high levels of influence are less worrisome *vis-à-vis* tolerance since ideological extremism does not seem to inspire additional activism. On the other hand, it appears that greater civic involvement comes at the expense of political moderation. Hence even if extremists do not self-select into the influential ranks of the activists, civic involvement appears to polarize their attitudes nonetheless. Yet I want to emphasize that this study uncovers something new about activists. Namely, their additional ideological extremism is acquired in service to democratic politics. Hence a final area of future research suggested here is exploring whether ideological

extremism acquired in service to democratic politics has different effects (e.g. on tolerance) than ideological extremism acquired in, for example, service to religious zealotry.

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## Appendix A: Analyzing Attrition

In Table A1 I describe respondents and nonrespondents, on each of my four variables, in the 2000 wave and the 2004 wave, by comparing their baseline 1996 responses. Fortunately, I only detect a potential problem with one of the eight comparisons. The 95% confidence intervals associated with the mean 1996 participation reports of respondents in 2000 compared to the mean 1996 participation reports of nonrespondents in 2000, do not overlap. However, the difference of the two means is only .40. While the difference is statistically significant, on a scale with a theoretical maximum of 18, the difference is less significant substantively. Further mollifying concern about response bias, the confidence intervals associated with the mean 1996 participation reports of respondents in 2004 compared to the mean 1996 participation reports of nonrespondents in 2004, do overlap. Finally, I detect no statistically significant response bias on the other variables.

Variable	Response group	Mean	95% CI	N
1996 participation*	2000 nonrespondent	2.04	1.88–2.21	542
	2000 respondent	2.44	2.29–2.59	913
1996 participation	2004 nonrespondent	2.21	2.07–2.33	1072
	2004 respondent	2.53	2.31–2.76	383
1996 contact	2000 nonrespondent	.75	.71–.79	542
	2000 respondent	.79	.77–.82	913
1996 contact	2004 nonrespondent	.78	.75–.80	1072
	2004 respondent	.77	.73–.81	383
1996 strength of PID	2000 nonrespondent	2.51	2.45–2.58	527
	2000 respondent	2.49	2.44–2.54	891
1996 Strength of PID	2004 nonrespondent	2.50	2.45–2.54	1046
	2004 respondent	2.51	2.43–2.59	372
1996 ideological extremism	2000 nonrespondent	1.71	1.64–1.78	510
	2000 respondent	1.73	1.68–1.77	880
1996 ideological extremism	2004 nonrespondent	1.69	1.65–1.74	1022
	2004 respondent	1.79	1.71–1.86	368

\* The 95% confidence intervals for respondents and nonrespondents do not overlap

## Appendix B: Descriptive Statistics

Variable*	Observations	Mean	SD	Min.	Max.
1996 participation	383	2.53	2.23	0	11
2000 participation	383	2.40	2.28	0	15
2004 participation	383	2.94	2.31	0	15
1996 contact	383	.77	.42	0	1
2000 contact	383	.70	.46	0	1
2004 contact	383	.76	.43	0	1
1996 Id. Ext.	368	1.79	.70	0	3
2000 Id. Ext.	371	1.85	.70	0	3
2004 Id. Ext.	381	1.82	.74	0	3
1996 Str. PID	372	2.51	.76	0	3
2000 Str. PID	373	2.41	.85	0	3
2004 Str. PID	378	2.50	.82	0	3

In order to assess the ideological extremism and strength of party identification variables for possible measurement problems (especially concern about their high means), I compared them to identical scales constructed using National Election Studies respondents who reported making a campaign contribution (I pooled the 1996, 2000, and 2004 surveys). The means reported above in samples drawn from contributors to the national parties were very similar to the analogous NES means

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