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RETHINKING THE ROLE OF POLITICAL INFORMATION

MATTHEW S. LEVENDUSKY*

Abstract Political information is a central variable for the study of mass behavior; numerous theories argue that voters with more information behave fundamentally differently from those with less. Nearly all of the empirical support for these theories, however, comes from cross-sectional data. As a result, these findings are typically biased, and systematically overstate the effect of information on behavior. I demonstrate how to minimize these biases and more accurately estimate the effects of information using several different analytical techniques. These adjustments cause the estimated effect of information to shrink dramatically, often falling to one-half to one-quarter of its former size. I conclude by discussing the implications of my results for the study of political information and political behavior more generally.

Over the past generation, scholars have built an impressive body of knowledge about the effects of political information on voters' attitudes and behaviors. Informed citizens are more likely to engage in the behaviors that define "good" citizenship, such as voting, participating in politics, and being tolerant (Delli Carpini and Keeter 1996). Not only are these effects normatively important, they are also frequently substantively large, with well-informed citizens behaving quite differently from their uninformed counterparts. The conventional wisdom is that information matters, and it matters a great deal.

I disagree, and I demonstrate that the effect of information is considerably more modest than previous studies suggest. Because most previous research on the effects of information relies on cross-sectional data, it systematically overstates the extent to which information shapes behavior. Using panel data and a variety of analytical techniques, I demonstrate how generating more accurate

MATTHEW S. LEVENDUSKY is Assistant Professor of Political Science at the University of Pennsylvania, Philadelphia, PA, USA. The author thanks John Bullock, Daniel Gillion, Greg Huber, Marc Meredith, Paul Sniderman, the anonymous referees, and the editors for helpful comments. Any remaining errors are his own. *Address correspondence to Matthew Levendusky, Department of Political Science, 208 S. 37th St, Philadelphia, PA 19104, USA; e-mail: mleven@sas.upenn.edu.

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estimates of information's effects reduces the effects to one-half to one-quarter of their previous size.

These findings have significant implications for political behavior scholars. They parallel earlier studies demonstrating that cross-sectional estimates of the determinants of political behavior need to be treated with care, and panel data or natural experiments offer better leverage on how information and related variables shape behavior (Claassen 2008; Kam and Palmer 2008). This, in turn, has important ramifications for how scholars think about the consequences of information.

How Much Does Information Matter?

Political information[1] has become one of the most crucial variables for mass behavior research over the past generation. Scholars focus on political information for two reasons. First, it has a crucial normative role. Knowledgeable citizens are better participants in politics; they are better able to translate their abstract, underlying values into choices consistent with those preferences (Delli Carpini and Keeter 1996; Zaller 1992). Knowledgeable citizens are also, quite simply, better citizens. They are more tolerant of unpopular minorities (McClosky and Brill 1983), and more likely to participate in the political process (Junn 1991). They also hold attitudes that are more stable over time and are more tightly connected to one another and their underlying values (Feldman 1989). In short, information is the "currency of citizenship"—better-informed citizens promote a thriving, healthy democracy (Delli Carpini and Keeter 1996, 8).

Second, political information fundamentally changes how citizens behave. Many (if not most) contemporary theories of political behavior argue that more well-informed citizens behave differently than their less well-informed counterparts (see, among many others, Converse 1964; Gilens 2001; Sniderman, Brody, and Tetlock 1991; Zaller 1992). Indeed, "voter information is theoretically critical" for understanding citizens' choices and attitudes (Achen 1992, 198). While heuristic reasoning (and other kinds of low-information rationality) might seem to close the gap between more and less informed citizens, it frequently falls short of that goal. Heuristics sometimes help low-information citizens make "good" decisions (Boudreau 2009; Kuklinski et al. 2001; Popkin 1991; Rahn, Aldrich, and Borgida 1994), but in other settings, only the well-informed can effectively utilize heuristics (Kuklinski and Quirk 2000; Lau and Redlawsk 2001). Scholars therefore cannot engage in the study of mass political behavior without accounting for the role of political knowledge.

1. I use the terms "political information" and "political knowledge" interchangeably, and take them to mean the amount people know about politics (Delli Carpini and Keeter 1996, 10–11; see also Price 1999). Note that this is distinct from "political sophistication," which includes both information and how it is organized and related to other abstract ideas about politics (Luskin 1987).

Table 1. Previous Estimates of the Effects of Information

Author	Change in Information	Behavior	Approx. Effect Size
Bartels (1996)	Observed to Fully Informed Electorate	Aggregate Reagan vote share, 1980 election	6% decrease
Althaus (1998)	Observed to Fully Informed Electorate	Female support for spousal notification laws	20% decrease
Delli Carpini and Keeter (1996)	10th decile to 90th decile of information	Perform campaign activity	56% increase
Claassen and Highton (2006)	Uninformed to Informed	Effect on support for health-care reform	15% increase
Delli Carpini and Keeter (1996)	1-standard-deviation increase from the mean	Voter turnout	19% increase

NOTE.—Effect sizes are as reported in the text of the original article. See the appendix for details on these estimates.

In light of the importance of information, scholars now regularly include measures of it in models of political behavior and find large robust effects. Table 1 gives a summary of effect sizes from several recent works that use political information as a predictor of political behavior.

While the specific effect size varies by study, the key point to note is that these are large effects, often causing substantial changes in behavior as information changes. Indeed, comparing across a host of potential predictors of attitude stability, Delli Carpini and Keeter (1996) conclude that information has "by far" the strongest effect (233). Goren (1997) shows that ideology has almost no effect on the vote choice of low-information voters, but an enormous effect on the vote choice (see his figure 2). In light of the host of other variables known to powerfully shape political behavior (e.g., partisanship, ideology, mobilization by parties/candidates, etc.), these are impressive estimates. While it is certainly true that information sometimes has only a modest effect on behavior (Ansolabehere, Rodden, and Snyder 2008; Gabel and Scheve 2007; Lupia et al. 2007; Sekhon 2005), most of the literature argues that information has a substantial positive effect on behavior.

While not denying the theoretical and normative importance of information, I argue that scholars have systematically overestimated the effects of political information on behavior by relying primarily on cross-sectional studies.[2]

2. There are important exceptions that utilize experimental or panel data, such as Gilens (2001) and Barabas and Jerit (2008).

Using cross-sectional data to estimate the effects of information requires utilizing between-subjects comparisons: Respondent Y looks like respondent X, except Y is better informed, so I can use Y's behavior to estimate how X would behave if better informed (for a discussion of the relevant comparisons in the context of voting, see Sekhon 2005). Unfortunately, such comparisons are problematic. Because respondents are not randomly assigned to have high or low levels of political information, high- and low-information voters will differ systematically from one another (i.e., the groups will not be exchangeable). Informed and uninformed voters differ along a host of dimensions—more informed voters care more about politics, consume more media, are more politically engaged, and so forth (on the differences between more and less informed voters, see Price 1999). Because these factors also plausibly affect many political behaviors, omitting them introduces bias—perhaps a great deal of bias—into the estimate of information on behavior. One could reduce this bias by controlling for some of these omitted factors, but doing so is a Sisyphean-task. Not only is it almost impossible for researchers to control for all known differences (Althaus 2003; Bartels 1996), but also the researcher cannot control for unobserved differences between the groups. The problem, therefore, does not vanish by simply adding additional controls (see also Barabas and Jerit 2009).

This type of omitted variable bias should, on average, lead to overestimates of the effects of information on behavior (Wooldridge 1999). Given that the most significant omitted variables (political interest, media use, etc.) tend to be positively correlated with both information and the outcome variables of interest (turnout, political participation, and so forth), omitting them will on average inflate the effect of information on behavior. Previous estimates of information are therefore likely to be overstatements of the effects of information on behavior (see also Ansolabehere, Rodden, and Snyder 2008; Gabel and Scheve 2007; Goren 2004; Sekhon 2005).

As a result, cross-sectional estimates cannot differentiate the effect of information itself from the broader set of factors correlated with information. What the coefficient on "political information" in a standard cross-sectional model captures is not simply the effect of political information, but also the systematic differences between more and less informed respondents. It is therefore unclear how these cross-sectional results help scholars test theories about the effects of information.

Research Design

I use two different analytical techniques to ameliorate this omitted variable problem. First, I follow Kam and Palmer (2008) and use matching to help eliminate differences between more informed and less informed voters (for additional examples of matching in political science, see Barabas 2004; Epstein et al. 2005; Ladd and Lenz 2009).[3] Matching algorithms select two sets of citizens who are matched to have the same level of education, political interest, gender, and other key variables. After matching, the only difference between the two groups will be their levels of information; one group is considered a high-information set, and the other a low-information set. This matching process makes it "as if" information were randomly assigned, thereby approximating an experimental design (Dehejia and Sadek 2002). While matching does not replace experimental manipulation,[4] it represents a significant step in the right direction toward reducing omitted variable bias.

More specifically, I match more informed and less informed voters using the coarsened exact matching algorithm (Iacus, King, and Porro 2009); see the online appendix for the details of the procedure and the relevant balance tests. In the results reported below, I consider three different definitions of "more informed" voters: (1) voters above the mean level of information; (2) voters above the 33rd percentile of information (i.e., above the bottom third of the information distribution); and (3) voters above the 67th percentile of information (i.e., the top third of the information distribution). Together, these results will help illuminate how information affects voters at different points along the information distribution (and ensure that the results are not simply the product of an arbitrary definition of "more informed").

But matching, like any technique, is not perfect. To ensure that my results are not an artifact of this one method, I also use two different regression strategies to analyze the effects of information. First, I use panel survey data to decompose the effects of information into two sources: between-subjects and within-subject variation (Neuhaus and Kalbfleisch 1998). I do so by estimating the following equation:

$$y_{it} = \beta_0 + \beta_1(Info_{it} - \overline{Info_i}) + \beta_2\overline{Info_i} + \Gamma z_i + u_{it,} \qquad (1)$$

where y is the outcome of interest, $Info_{it}$ gives respondent i's level of political information at Wave t, $\overline{Info_i}$ is respondent i's average level of political information across all waves of the panel study, z is a vector of control variables, and u is an unobserved disturbance term. β_1 therefore gives the within-subject effect of information and β_2 gives the between-subjects effect. The within-subject effects should be substantially smaller than the between-subjects effects, because the within-subject estimates compare individuals to themselves, thereby avoiding the problematic between-subjects comparisons discussed above.

3. For relevant technical details on matching, see Sekhon (2005) and Ho et al. (2007).

4. For example, Arceneaux, Gerber, and Green (2006) show that if treatment and control differ on unobserved variables, then matching may not yield correct answers (see also Shadish, Clark, and Steiner 2008).

Second, I estimate a subject fixed-effects model:

$$y_{it} = \beta_0 + \beta_1 Info_{it} + \Gamma z_{it} + \alpha_i + u_{it}, \quad (2)$$

where α_i is a respondent-specific fixed effect and all other terms are as defined above for Equation (1). The fixed effects control for the fact that some subjects are more politically interested, consume more media, etc. If I compare this to the effect of information obtained from a version of Equation (2) without the fixed effects (i.e., $y_{it} = \beta_0 + \beta_1 Info_{it} + \Gamma z_{it} + u_{it}$), then I should find that the effect from the fixed-effects regression is substantially smaller, given that it controls for the time-invariant, between-subjects differences correlated with information.

Together, these two strategies (matching and regression analysis) should allow me to more accurately estimate the effects of information. One advantage of using both methods is that they use different techniques and assumptions to reduce omitted variable bias. Matching removes bias that is a function of the observables, but cannot control for unobserved differences. The regression models can remove unobserved heterogeneity, but only if it is stable across time. If these different methods generate similar answers, they will provide strong empirical evidence to support my theoretical argument.

Data and Outcomes

I use two datasets to test my hypotheses. First, I use the 1992-1994-1996 panel data from the National Election Study (NES), the standard benchmark data for political behavior. The limitation of the NES panel, however, is that it spans only a four-year window, and thus can speak only to the effects of short-term changes in political information. To study longer-term informational differences, I turn to the Youth-Parent Socialization Study (see Jennings, Stoker, and Bowers 2009). These data track high school seniors from the class of 1965, interviewing them first in 1965, then again in 1973, 1982, and 1997. Using these two datasets functions as a robustness check and ensures that my results are not limited to just one point in time. See Appendix A for data details.

While political information affects a wide variety of voter behaviors, this article focuses on the effects on voter turnout and political participation. Without a baseline knowledge of politics and public affairs, citizens cannot know what policies or candidates to support. There should therefore be a strong link between political knowledge and political participation (Delli Carpini and Keeter 1996; Junn 1991).

Operationally, voter turnout is simply whether the respondent voted in the last presidential election (assessed via self-reported turnout). I assess campaign participation using a five-item battery: Respondents are asked whether they (1) talked to others about why they should or should not support a given candidate or party; (2) wore a button or displayed a yard sign; (3) attended a meeting or rally; (4) donated money to a candidate or party; or (5) volunteered for a campaign. The three dependent variables related to campaign activity are based on the total number of participatory acts each respondent performs. The first dependent variable is a count of the total number of campaign activities performed by each respondent. Second, I code whether the respondent engages in any campaign activities at all, and third, I examine whether each respondent is an activist, where activists are those who perform two or more campaign activities (Delli Carpini and Keeter 1996; Junn 1991).

To measure political information, I use an index of items built from factual knowledge items included in all waves of the relevant surveys (see the online appendix for the relevant items). These items include knowledge of current political events (e.g., which party has the majority in the Senate), recognition of political figures (e.g., who is Newt Gingrich and what position does he hold), and "civics textbook"–type information (e.g., the length of the term of a U.S. Senator).[5]

In addition to political information, the models estimated control for other factors known to influence political participation: demographic factors (race, gender, birth cohort, income, and region), level of formal education, political mobilization by candidates and parties (for the campaign-activity regression), interest in politics, media use, the frequency of the respondent's political discussion, and strength of partisanship. I include these variables to control for other well-known determinants of behavior. While one obviously cannot control for every possible factor affecting these behaviors, these variables should help prevent obvious misspecification of the relationship between information and behavior.

Results: Matching Analysis

If my argument above is correct, the effect of information in the matched sample should be significantly smaller than in the raw data. Table 2 gives the results, reporting the average treatment effect for the treated (ATT) estimate of political information.

Before matching, there are large and statistically significant effects of information on all of the behaviors. But after matching, those effects almost always become significantly smaller, strongly supporting my theoretical argument. The effect of information *qua* information is rather modest when calculated properly.

Not only are the differences statistically significant, they also are substantively significant. Take, for example, those above the mean level of information in the 1992-1994-1996 NES data. Before matching, those with above-average levels of political information are 27 percent more likely to engage in a campaign activity (relative to those with below-average levels of political

5. As a robustness check, I re-estimated the models using the NES "interviewer rating" item (Zaller 1986) as the measure of political information and obtained similar results.

information). After matching, however, the difference shrinks to six percent—the effect of information in the matched data is less than one-quarter of the effect before matching (and the post-matching estimate is significantly smaller). Other items show a similar decline: Looking across that same row, the effect on voter turnout declines from 27 percent to eight percent, and the effect on being a campaign activist declines from 12 percent to five percent. Indeed, comparing across the outcomes, versions of the treatment, and datasets, one finds this same pattern: Effect sizes shrink considerably after matching. In general, the effects in the matched data are approximately one-half to one-quarter the size of the effects in the unmatched data. These results strongly support my argument: Once the effects of other factors are removed, the effect of information is quite modest.

Results: Regression Analysis

Using panel data to examine within-subject changes in information over time offers another way to estimate the effects of information. One concern with this approach, however, is that within-subject differences in information may simply represent noise rather than meaningful change. If this is the case, then it is unclear what the models discussed above are actually estimating.

Fortunately, panel data provide a way to test this claim. If I estimate

$$Info_{i3} = \beta_0 + \beta_1(Info_{i2} - Info_{i1}) + u_i \tag{3}$$

where the 1, 2, and 3 subscripts represent waves of the panel data and all other terms are as defined above in Equations (1) and (2), I can discern whether changes in information between periods 1 and 2 explain the level of information at time 3. If between-period differences are simply random noise stemming from measurement error, then the change between Waves 1 and 2 should have no predictive power to explain the level at Wave 3. But, if between-wave differences in information are actually measuring real shifts in information, then they should have predictive power: People who gain (lose) information between Waves 1 and 2 should have higher (lower) levels of political information at Wave 3. A positive and statistically significant β_1 indicates that information differences reflect real changes in information that persists over time.

Table 3 below displays estimates of Equation (3) using the 1992-1994-1996 NES data and the Youth-Parent Socialization data.

Wave 1 to Wave 2 changes in information strongly predict Wave 3 levels of political knowledge in both datasets: Respondents who become better informed between Waves 1 and 2 have higher levels of political knowledge at Wave 3. Within-subject information effects are not simply ephemeral random noise, but rather reflect real changes in information that persist over time.

Table 4 reports estimates of Equation (1) (decomposing information into within-subject and between-subjects effects), and table 5 reports the parallel

Table 2. Matching Results, 1992-1994-1996 National Election Study (NES) Data and Youth-Parent Socialization (YPS) Data (standard errors in parentheses)

Dataset	Turnout, Pre-Match	Turnout, Post-Match	Any Activity, Pre-Match	Any Activity, Post-Match	Activist, Pre-Match	Activist, Post-Match	Number of Activities, Pre-Match	Number of Activities, Post-Match
Top Third of the Information Distribution:								
NES	**0.20**	0.05	**0.25**	0.07	**0.12**	0.06	**0.48**	0.18
	(0.02)	(0.03)	**(0.03)**	(0.05)	**(0.02)**	(0.04)	**(0.05)**	(0.11)
Sig. Smaller?		Y		Y		Y		Y
N	1533	392	1713	392	1713	392	1713	392
YPS	**0.18**	0.02	**0.18**	−0.10	**0.22**	−0.10	**0.66**	−0.12
	(0.03)	(0.05)	**(0.04)**	(0.08)	**(0.04)**	(0.09)	**(0.11)**	(0.28)
Sig. Smaller?		Y		Y		Y		Y
N	887	115	887	115	887	115	887	115
Mean Level of Information:								
NES	**0.27**	**0.08**	**0.27**	0.06	**0.12**	0.05	**0.50**	**0.18**
	(0.02)	**(0.04)**	**(0.02)**	(0.05)	**(0.01)**	(0.03)	**(0.04)**	**(0.10)**
Sig. Smaller?		Y		Y		Y		Y
N	1533	330	1713	330	1713	330	1713	330
YPS	**0.18**	0.00	**0.19**	−0.10	0.19	−0.13	**0.62**	−0.19
	(0.03)	(0.06)	**(0.03)**	(0.10)	(0.03)	(0.11)	**(0.09)**	(0.31)
Sig. Smaller?		Y		Y		Y		Y
N	887	89	887	89	887	89	887	89

Continued

Table 2. *Continued*

Dataset	Turnout, Pre-Match	Turnout, Post-Match	Any Activity, Pre-Match	Any Activity, Post-Match	Activist, Pre-Match	Activist, Post-Match	Number of Activities, Pre-Match	Number of Activities, Post-Match
Lowest Third of the Information Distribution:								
NES	**−0.32**	0.02	**−0.27**	0.07	**−0.12**	−0.004	**−0.47**	0.05
	(0.02)	(0.07)	**(0.02)**	(0.06)	**(0.02)**	(0.03)	**(0.04)**	(0.09)
Sig. Smaller?		Y		Y		Y		Y
N	1533	218	1713	218	1713	218	1713	218
YPS	**−.016**	−0.14	**−0.15**	**−0.28**	**−0.11**	−0.19	**−0.35**	**−0.77**
	(0.03)	(0.13)	**(0.03)**	**(0.15)**	**(0.03)**	(0.13)	**(0.10)**	**(0.40)**
Sig. Smaller?		N		N		N		N
N	887	42	887	42	887	42	887	42

NOTE.—Cell entries are the average treatment on the treated for the NES and YPS datasets, with associated standard errors underneath. The row ''Sig. Smaller?'' notes whether the treatment effect is significantly smaller post-matching relative to pre-matching. Coefficients that can be distinguished from 0 (at the $\alpha = 0.10$ level, one-tailed) are given in **bold.**

Table 3. Lagged Information Differences Predict Future Information Levels (standard errors in parentheses)

	Wave 3 Information Level
NES Data:	
Intercept	**5.31**
	(0.11)
Wave 2 – Wave 1 Information Differences	**0.11**
	(0.06)
N	585
R-Squared	0.005
Youth-Parent Socialization Data:	
Intercept	**3.18**
	(0.06)
Wave 2 – Wave 1 Information Differences	**0.09**
	(0.04)
N	889
R-Squared	0.006

NOTE.—Cell entries are linear regression coefficients with robust standard errors in parentheses (estimates are from Equation (3)). Coefficients that can be distinguished from 0 are given in **bold**.

Table 4. Within-subject vs. Between-subjects Effects of Information (standard errors in parentheses)

	Turnout	Any Activity	Activist	Number of Activities
NES Data:				
Within-subject	0.002	−0.01	−0.001	−0.01
Effect	(0.01)	(0.01)	(0.01)	(0.02)
Between-subjects	**0.04**	**0.01**	**0.02**	**0.04**
Effect	**(0.01)**	**(0.003)**	**(0.01)**	**(0.01)**
Within Effect Smaller?	Y	Y	Y	Y
YPS Data:				
Within-subject	−0.004	0.001	**0.02**	0.05
Effect	(0.009)	(0.01)	**(0.01)**	(0.03)
Between-subjects	**0.02**	**0.05**	**0.06**	**0.21**
Effect	**(0.008)**	**(0.01)**	**(0.01)**	**(0.04)**
Within Effect Smaller?	Y	Y	Y	Y

NOTE.—Cell entries give the estimated OLS coefficients (from Equation (1)), with associated robust standard errors underneath. Full regression results are reported in the appendix. Coefficients that can be distinguished from 0 are given in **bold.**

Table 5. Subject Fixed Effects Estimates of the Effect of Information (standard errors in parentheses)

	Turnout	Any Activity	Activist	Number of Activities
NES Data:				
Information Effect (FE)	0.01 (0.01)	−0.006 (0.01)	−0.001 (0.01)	−0.002 (0.02)
Information Effect	**0.03** **(0.004)**	**0.01** **(0.004)**	0.004 (0.003)	**0.02** **(0.01)**
FE Estimate Smaller?	Y	N	Y	Y
YPS Data:				
Information Effect (FE)	−0.001 (0.009)	0.006 (0.01)	**0.03** **(0.01)**	0.06 (0.04)
Information Effect	**0.01** **(0.006)**	**0.03** **(0.007)**	**0.05** **(0.01)**	**0.15** **(0.03)**
FE Estimate Smaller?	Y	Y	Y	Y

NOTE.—Cell entries are OLS estimates with robust standard errors underneath. The rows labeled ''FE'' contain the estimates of Equation (2) with subject fixed effects; the other row set of estimates is the corresponding estimate without fixed effects. The ''FE Estimate Smaller'' indicates whether the estimate of information's effect with fixed effects is significantly smaller. Full regression results are reported in the appendix. Coefficients that can be distinguished from 0 are given in **bold.**

results for Equation (2) (which uses fixed effects to control for between-subjects differences in information). Full regression results are provided in Appendix B.

The results in both tables strongly confirm my theoretical argument. In table 4, the effects of within-subject changes in information are substantially smaller than the corresponding effect of between-subjects changes (approximately one-half the size), and these differences are highly statistically significant. Likewise, in table 5, which includes fixed effects to control for subject-specific factors, the effects of information are typically less than half of the size of the effect without the fixed effects (though both methods suggest quite small information effects). The effect of information itself (distinct from related factors) is relatively modest.

I also estimate two additional robustness checks reported in the online appendix. First, following the strategy used by Sekhon (2005), I used "gainers" (i.e., those who gain information between panel waves) to estimate the effect of information on behavior. Second, I used a difference-in-differences specification to examine how differences in information translate into differences in behavior. Both models confirm the findings reported above.

One might object to these specifications on the basis that short-term changes in a panel cannot be compared to longer-term differences captured by average values of information. After all, the latter reflects long-term differences in factors like socialization and political interest. But this is entirely the point: Using information levels from cross-sectional data does not allow us to disentangle information from any of these other factors, and instead conflates them. As I argue theoretically—and as the results show empirically—this systematically overstates the effects of information. If scholars want to isolate the effect of information (and not the effect of factors correlated with information), then the types of comparisons made here are preferable. The panel data strategy, for any other flaws it might have, moves scholars closer to the counterfactual of interest (how changes in information translate into changes in behavior).

My work parallels Kam and Palmer's (2008) analysis of the effects of education. They find that higher education has almost no effect on political participation once analysts properly adjust for differences between those who seek out higher education and those who do not. Much of the ubiquitous "education effect" found in a host of earlier cross-sectional studies is not an effect of education itself, but of other differences correlated with education.[6] A similar process is at work here: Part of what we think of as the effect of "political information" is the difference in interest, media use, motivation, etc., that differentiates more and less well-informed respondents. While political knowledge may be an important cause of political participation, its effect accounting for other variables is rather modest.

Conclusions and Implications

This article reassesses the relationship between political information and political behavior, particularly voter turnout and political participation. I argue that previous research, because it uses cross-sectional data, inflates the effect of information on political behavior due to omitted variable bias. After adjusting for this bias, information has a real but modest effect on turnout and campaign participation, rather than the large effects found in earlier studies.

While these are important findings, they are not without limitations. In particular, three stand out as especially important. First, these findings are not the "gold standard" of causally identified effects of information. To generate causal estimates, one would need some sort of randomized/natural experiment. Because I have not manipulated information or information levels (i.e., there is no random assignment), these estimates cannot be interpreted as causal. Second, endogeneity issues need to be addressed. In this

6. See Sondheimer and Green (2010) for a study finding an experimental link between education and voter turnout.

case, information gains cause turnout, but turnout may also cause information gains. Similarly, panel attrition might be correlated with information, so that low-information respondents might be more likely to drop out of the panel over time (which in turn could affect the estimates). Third, the current study examines the effects of information only on campaign behavior, but in theory, information affects a much broader class of behaviors and attitudes. Future work can explore whether these patterns extend to other domains.

These findings have straightforward empirical implications. First, scholars should treat cross-sectional estimates of the effects of political information with caution, given the problems identified here. Those interested in the effects of political information on behavior should focus instead on using panel data, natural experiments, or randomized experiments to explore the implications of information on behavior. These alternative strategies allow scholars to more precisely identify the effects of information on behavior without the contaminating effects of factors like omitted variable bias.

Second, scholars need to rethink the ingrained idea that information has large and sustained consequences for political behavior. While information matters, its effects are much smaller than many previous estimates suggest, and scholars need to adjust their thinking—and the conventional wisdom—accordingly. This also suggests that real-world efforts to increase the general public's level of information may well have quite modest payoffs. Calls to improve the public's understanding of the political process are as old as democracy itself, but my results suggest that simply increasing information will have only small effects. To see the larger effects observed in earlier studies, one would need to also change the factors correlated with information, which is no easy task.

This study also underlines both the challenge and the promise of theories of political information moving forward. These results show that the effects of general political information are much smaller than previous estimates suggest, thereby opening up the possibility that other factors and other kinds of information (e.g., policy-specific information; see Gilens 2001) matter to voters. Rather than saying that "general political information" shapes behavior, these results open the door to saying what information matters under what circumstances. While scholars have already begun to address this scholarly agenda (Boudreau 2009; Lupia and McCubbins 1998), more work undoubtedly remains to be done.

Appendix A: Data Sources

Youth-Parent Socialization Data

The data used here come from ICPSR study #4037, which contains a four-wave panel of data (additional details on the survey methodology are available in the ICPSR release notes; see also Jennings, Stoker, and Bowers 2009). The respondents were high school seniors when the initial interviews took place in 1965 (interviews took place between March and May), and subjects were then re-interviewed in 1973 (interviews took place between January and April), 1982 (interviews took place between May and August), and finally in 1997 (interviews took place between April and October).

The original sample was gathered by selecting a national probability sample of 1,669 high school seniors in 1965 distributed across 97 public and nonpublic schools (schools were selected with probability proportionate to size). All Wave 1 data were initially collected via face-to-face interviewing, and most interviews for Waves 2–4 were also done with face-to-face interviewing. For some subjects, when a face-to-face interview was not possible, a self-administered questionnaire was used (17 percent of Wave 2 interviews, 15 percent of Wave 3 interviews, and 0.9 percent of Wave 4 interviews were done with self-administered questionnaires; 48.6 percent of interviews in 1997 were done using computer-assisted telephone interviews; all remaining interviews were face-to-face).

The Wave 1 response rate of students within schools was 99 percent; later response rates were 81 percent (Wave 2), 84 percent (Wave 3), and 82 percent (Wave 4). The 935 respondents who composed the four-wave respondents in this dataset represent 56 percent of the original respondents from the first wave. Specific question wordings are given in the online appendix.

1992-1994-1996 NES Panel Data:

The 1992-1994-1996 NES Panel Data is a three-wave panel with five components. In 1992 and 1996, subjects completed a pre-election and a post-election survey; in 1994, subjects completed only a post-election survey. All interviews were conducted face-to-face, with two exceptions. First, there were a small number of cases where panelists moved to a location where no NES interviewers were working. In these cases, a phone interview was used to avoid dropping the respondents from the panel. Second, in the 1996 post-election wave, cases were randomly assigned to be interviewed either face-to-face or over the telephone. Details here come from the appendix to the 1992–1997 Combined File; see the NES website for additional details (www.electionstudies.org).

The sampling frame includes all U.S. citizens of voting age on or before election day, living in the lower 48 states, excluding group quarters, military housing (though not civilian housing on military bases), and institutions. The selection of individuals to interview within the sampling frame depends upon a multi-stage area probability design. First, broad geographic areas (such as counties/congressional districts) are selected, then housing clusters within the first stage are selected, then particular housing units, then respondents within households. The selection is random at all stages. Further details on the sampling procedure are available online at the NES website.

The data begin with 1,005 individuals who were interviewed in 1992 (for the pre-election wave, between 1 September 1992 and 3 November 1992; for the post-election wave, between 4 November 1992 and 13 January 1993). Of those individuals, 759 were re-interviewed after the 1994 election (between 9 November 1994 and 1 January 1995), along with 1,036 fresh respondents. In 1996, 545 of the original 1992 respondents were re-interviewed (the figure is 719 for the 1994 respondents; interview dates were 3 September 1996 through 4 November 1996 for the pre-election wave, and 6 November 1996 through 31 December 1996 for the post-election wave). There are 597 respondents interviewed in the 1992, 1994, and 1996 waves of the study; see variable VPARTIC in the data for more information.

The response rates for each wave for all respondents (including fresh respondents added for that wave) are 74 percent for 1992, 74 percent for 1994, and 71 percent for 1996. The re-interview rates are 77 percent in 1994 and 76 percent in 1996. Specific question wordings are given in the online appendix.

Appendix B: Full Regression Results

Table A1. Fixed Effect Models, 1992-1994-1996 NES Data (robust standard errors in parentheses)

	Turnout (FE)	Turnout	Activist (FE)	Activist	Any Activity (FE)	Any Activity	Number of Activities (FE)	Number of Activities
Political Information	0.01	0.03	−0.001	0.004	−0.006	0.01	−0.00	0.02
	(0.01)	(0.00)	(0.01)	(0.003)	(0.01)	(0.00)	(0.02)	(0.01)
Strength of Party ID	0.03	0.03	0.01	0.02	−0.00	0.02	0.03	0.06
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.04)	(0.02)
Lowest Third, Income	−0.01	−0.07	−0.02	−0.01	−0.08	−0.01	−0.12	0.00
	(0.04)	(0.02)	(0.03)	(0.01)	(0.05)	(0.02)	(0.08)	(0.04)
Highest Third, Income	−0.00	0.02	−0.06	0.05	−0.06	−0.00	−0.16	0.08
	(0.03)	(0.02)	(0.03)	(0.02)	(0.04)	(0.02)	(0.08)	(0.04)
Media Usage	−0.02	−0.00	0.01	−0.00	0.00	−0.01	0.01	−0.00
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.02)	(0.01)
Political Interest	0.10	0.14	0.05	0.09	0.14	0.19	0.20	0.36
	(0.03)	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)	(0.05)	(0.03)
Political Discussion	0.01	0.01	0.00	0.01	0.00	0.02	0.01	0.05
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)
Male		−0.05		0.01		0.06		0.06
		(0.01)		(0.01)		(0.02)		(0.03)
White		−0.00		0.01		0.02		0.07
		(0.02)		(0.02)		(0.03)		(0.04)
South		−0.06		0.02		−0.00		0.04

Continued

Table A1. *Continued*

	Turnout (FE)	Turnout	Activist (FE)	Activist	Any Activity (FE)	Any Activity	Number of Activities (FE)	Number of Activities
		(0.02)		(0.01)		(0.02)		(0.04)
Pre–New Deal Birth Cohort		0.22		−0.06		−0.19		−0.35
		(0.04)		(0.03)		(0.05)		(0.09)
New Deal Birth Cohort		0.17		−0.02		−0.09		−0.18
		(0.03)		(0.02)		(0.03)		(0.06)
Post–New Deal Birth Cohort		0.09		−0.01		−0.02		−0.07
		(0.02)		(0.02)		(0.03)		(0.05)
Baby Boomer Birth Cohort		0.09		−0.02		−0.03		−0.09
		(0.02)		(0.02)		(0.02)		(0.04)
Education		0.03		0.01		−0.00		0.02
		(0.00)		(0.00)		(0.01)		(0.01)
Mobilization			0.07	0.13	0.07	0.15	0.22	0.40
			(0.02)	(0.02)	(0.03)	(0.02)	(0.06)	(0.04)
Intercept	0.45	−0.05	−0.06	−0.31	0.11	−0.31	0.05	−0.98
	(0.10)	(0.05)	(0.07)	(0.04)	(0.11)	(0.05)	(0.20)	(0.10)
N	3077	2999	3070	2993	3070	2993	3070	2993
R-squared	0.025	0.225	0.022	0.122	0.027	0.155	0.033	0.184

NOTE.—Models are estimates of Equation (2), with and without fixed effects (columns labeled ‘‘FE’’ contain fixed effects). All estimates are OLS estimates.

Table A2. Fixed Effect Models, Youth-Parent Socialization Data (robust standard errors in parentheses)

	(1) Turnout (FE)	(2) Turnout	(3) Any Activity (FE)	(4) Any Activity	(5) Activist (FE)	(6) Activist	(7) Number of Activities (FE)	(8) Number of Activities
Political Information	−0.00	0.01	0.01	0.03	0.03	0.05	0.06	0.15
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.02)
Lowest Third, Income	−0.03	−0.04	−0.01	0.01	0.05	0.07	−0.02	0.11
	(0.03)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.11)	(0.09)
Highest Third, Income	0.04	0.00	0.02	0.02	0.05	0.04	0.02	0.07
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.10)	(0.09)
Partisan Strength	0.04	0.05	0.04	0.07	0.03	0.08	0.24	0.34
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.06)	(0.04)
Media Usage	−0.01	0.03	0.03	0.09	0.01	0.09	−0.06	0.30
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.07)	(0.06)
College Graduate	0.11	0.08	−0.00	0.09	0.21	0.10	0.48	0.32
	(0.06)	(0.02)	(0.07)	(0.02)	(0.07)	(0.03)	(0.22)	(0.08)
Male		0.02		0.04		0.02		0.11
		(0.02)		(0.02)		(0.02)		(0.08)
White		0.03		−0.04		−0.11		−0.28
		(0.03)		(0.04)		(0.05)		(0.15)
Constant	0.71	0.55	0.54	0.18	0.09	−0.10	0.69	−0.33
	(0.08)	(0.05)	(0.08)	(0.06)	(0.10)	(0.07)	(0.29)	(0.21)
Observations	1534	1534	1534	1534	1534	1534	1534	1534
R-squared	0.025	0.058	0.009	0.086	0.024	0.094	0.033	0.110

NOTE.—Models are estimates of Equation (2), with and without fixed effects (columns labeled "FE" contain fixed effects). The estimates are OLS estimates.

Table A3. Within-subject vs. Between-subjects Estimates of the Effect of Information, 1992-1994-1996 NES Data (robust standard errors in parentheses)

	(1) Turnout	(2) Activist	(3) Any Activity	(4) Number of Activities
Information, Within Effect	0.00	−0.00	−0.01	−0.01
	(0.01)	(0.01)	(0.01)	(0.02)
Information, Between Effect	0.04	0.01	0.02	0.04
	(0.01)	(0.00)	(0.01)	(0.01)
Lowest Third, Income	−0.05	−0.01	−0.01	−0.02
	(0.02)	(0.01)	(0.02)	(0.04)
Highest Third, Income	0.02	0.04	−0.01	0.05
	(0.02)	(0.02)	(0.02)	(0.04)
Male	−0.05	0.00	0.05	0.06
	(0.02)	(0.01)	(0.02)	(0.04)
White	−0.02	0.00	0.01	0.05
	(0.03)	(0.02)	(0.03)	(0.05)
South	−0.07	0.03	0.00	0.05
	(0.02)	(0.01)	(0.02)	(0.04)
Pre–New Deal Birth Cohort	0.23	−0.05	−0.18	−0.31
	(0.04)	(0.04)	(0.06)	(0.11)
New Deal Birth Cohort	0.19	−0.02	−0.09	−0.15
	(0.03)	(0.02)	(0.03)	(0.07)
Post–New Deal Birth Cohort	0.11	−0.00	−0.02	−0.05
	(0.03)	(0.02)	(0.03)	(0.06)
Baby Boom Birth Cohort	0.09	−0.01	−0.04	−0.08
	(0.02)	(0.02)	(0.02)	(0.05)
Media Usage	−0.00	−0.00	−0.01	−0.00
	(0.00)	(0.00)	(0.00)	(0.01)
Political Interest	0.14	0.09	0.19	0.34
	(0.01)	(0.01)	(0.02)	(0.03)
Political Discussion	0.01	0.01	0.02	0.04
	(0.00)	(0.00)	(0.00)	(0.01)
Education	0.03	0.01	−0.00	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Mobilization		0.12	0.13	0.35
		(0.02)	(0.02)	(0.04)
Intercept	0.02	−0.24	−0.23	−0.75
	(0.05)	(0.04)	(0.05)	(0.11)
Observations	3003	2997	2997	2997
R-squared	0.23	0.12	0.16	0.18

NOTE.—Models are estimates of Equation (1). The estimates are OLS estimates.

Table A4. Within-subject vs. Between-subjects Estimates of the Effect of Information, Youth-Parent Socialization Data (robust standard errors in parentheses)

	(1) Turnout	(2) Any Activity	(3) Activist	(4) Number of Activities
Information, Within Effect	−0.00	0.00	0.02	0.05
	(0.01)	(0.01)	(0.01)	(0.03)
Information, Between Effect	0.02	0.05	0.06	0.21
	(0.01)	(0.01)	(0.01)	(0.04)
Lowest Third, Income	−0.04	−0.00	0.05	0.02
	(0.02)	(0.03)	(0.03)	(0.09)
Highest Third, Income	0.01	0.02	0.04	0.04
	(0.02)	(0.03)	(0.03)	(0.09)
Partisan Strength	0.05	0.07	0.07	0.32
	(0.01)	(0.01)	(0.01)	(0.04)
Media Usage	0.02	0.08	0.06	0.15
	(0.01)	(0.02)	(0.02)	(0.06)
College Graduate	0.08	0.08	0.11	0.35
	(0.02)	(0.03)	(0.03)	(0.10)
Male	0.02	0.03	0.01	0.08
	(0.02)	(0.03)	(0.03)	(0.10)
White	0.02	−0.05	-0.12	−0.29
	(0.04)	(0.05)	(0.06)	(0.19)
Constant	0.54	0.18	-0.06	−0.14
	(0.06)	(0.07)	(0.08)	(0.25)
Observations	1534	1534	1534	1534
R-Squared	0.07	0.09	0.10	0.11

NOTE.—Models are estimates of Equation (1). The estimates are OLS estimates.

Supplementary Data

Supplementary data are freely available online at http://poq.oxfordjournals.org/.

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