

Updating Voters: How voters act as if they are informed*

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Abstract

Using new robust methods for making causal inferences from survey data, I show that voters in U.S. Presidential elections are able to use cues and heuristics to act as if they are informed. More precisely, when voters are matched on baseline characteristics, voters who change information state vote no differently on election day than voters who do not. Before election day, however, voters who change information state do differ in their vote intentions when similarly matched. The election campaign appears to provide the necessary cues to make the differences vanish by election day. This evidence is consistent with a growing body of formal and experimental work which shows that voters are able to use cues, heuristics and other information shortcuts to act as if they are informed. Thus, much of the widespread worry on the part of democratic theorists and activists about the poor information state of voters is misplaced. The methods used are more robust than the traditional regression approach and are well suited to making causal inferences from observational data. The analysis relies on data from the National Election Studies particularly the 1972-1974-1976, 1980, and 1992-1994-1996 panels.

“The people should have as little to do as may be about the government. They lack information and are constantly liable to be misled.” —Roger Sherman, June 7, 1787 at the Federal Constitutional Convention (Collier 1971)

“In a crowd men always tend to the same level, and, on general questions, a vote, recorded by forty academicians is no better than that of forty water-carriers.”
—Gustave Le Bon, *The Crowd* (1896, 200)

1 Introduction

When James Wilson of Pennsylvania suggested that senators be directly elected by the people, not a single member of the Federal Constitutional Convention in Philadelphia rose to support him (Smith 1956). Moreover, he was almost alone in supporting the direct election of the president. With few notable exceptions, such as Jefferson who was not at the Federal Convention, most of the American founding fathers had a deep suspicion, and in some cases fear, of popular sovereignty. There were several reasons for this suspicion, but a strong belief in the ignorance of the general population and hence the likelihood of them being misled was a prominent one.

Even in our demotic age, concerns about the competence of the general public remain. The level of concern in the academic literature appears to wax and wane. For example, in the aftermath of World War II, a war which resulted (in part) because of the *election* of the Nazi party by Germans, the concern waxed. Notable and influential work highlighted the dangers of the political influence of mass societies (e.g., Kornhauser 1959) and emphasized the instability of democratic regimes and the need for political order by authoritarian means (e.g., Huntington 1968). In our current post 9/11 age, internationally at least, it appears the concern is generally waning as the exporting of democratic institutions has become to be seen as a security necessity.

There has been a division about concerns about democracy in mature democracies and

concerns about new democracies. This division was most clear during the cold war where in the name of defending democracy, various authoritarian regimes were given significant support and some fledgling democracies were actively undermined. This division existed and in part still exists because of a belief that certain societies are not ready for democracy—that democracies are unable to represent the enlightened preferences of citizens to the extent that democratic decisions in these societies would undermine not only our interests but their interests as well.

This manuscript argues that citizens in mature democracies are able to accomplish something that citizens in new and fledgling democracies are unable to: inattentive and poorly informed citizens are able to vote like their better informed neighbors and hence need to pay little attention to political events. A lot of institutional change has occurred since the time of the founding fathers, not the least of which is the creation of organized political parties, interest groups and public opinion polls. And there are theoretical reasons to believe that these new institutions allow poorly informed citizens to behave as if they are better informed than they are. But these are precisely the institutions which are either lacking or poorly developed in new democracies.

The results presented in this manuscript are striking and offer both positive and negative news for the state of democratic society. On the one hand, the results suggest that the concerns which are raised by scholars, politicians and activists about how little attention Americans pay attention to political events such as campaigns may be misplaced. The little attention which most citizens pay is one of the benefits they have of living in a mature democracies where their lack of attention does not appear to harm the quality of their vote relative to their more attentive neighbors. On the other hand, the results support the concerns that many, especially realist foreign policy advocates, have about the efficacy, stability and dangers of new democracies. There is a qualitative difference in the role which information and attentiveness plays in elections in mature and immature democracies. And to the extent to which inattentive voters are unable to perform information arbitrage, one

of the key assumptions of the mass politics literature is satisfied.

This manuscript is organized as follows. In the next section, the role of information in elections is discussed. After which the data are talked about. Then the causal model of inference which is used is generally outlined. The following two sections specifically discuss the US and Mexico cases. The next section briefly out describes the results from the other datasets analyzed. The final section concludes.

2 Survey Data and Information

Survey research is strikingly uniform in its conclusions regarding the ignorance of the public. Berelson, Lazarsfeld, and McPhee (1954) argue voters fall short of classic standards of democratic citizenship. Campbell, Converse, Miller, and Stokes (1960) arrive at similar conclusions. Subsequent work did establish that information levels fluctuate over time (Bennett 1988), but no one disputes the long-established fact that most voters are politically ignorant (Althaus 1998, 2003; Converse 1964; Delli Carpini and Keeter 1996; Neuman 1986; Zaller 1992), and the argument that voters are inadequately informed given classical ideals of democratic citizenship has not been seriously challenged (e.g., Adams 1973 [1805]; Bryce 1995 [1888]; Habermas 1996 [1964]).

Although the fact of public ignorance has not been forcefully challenged, the meaning of this observation has been (Sniderman 1993). One approach is to claim that with limited information voters or collectives of voters can nevertheless make rational decisions. Some claim poorly informed individuals may still act in a sophisticated fashion because they make efficient use of signals (Alvarez 1997; Page and Shapiro 1992; Popkin 1991; Sniderman, Brody, and Tetlock 1991).¹ Even though individuals are poorly informed, political and electoral institutions may allow voters to make decisions that are much the same as they would make

¹Others rely on results related to the Condorcet Jury Theorem to claim that errors committed by individual voters will cancel out in the aggregate (Miller 1986; Wittman 1989). For a criticism of the usual jury theorem results see Austen-Smith and Banks (1996) and Berg (1993).

if they had better information. For instance, McKelvey and Ordeshook (1985a,b) suggest that polls and interest group endorsements may perform such cuing functions.

Analyzing individual level National Election Studies (NES) data, Mebane (2000) and Mebane and Sekhon (2002) provide surprisingly strong evidence for the existence of a large-scale noncooperative equilibrium among eligible voters in American presidential and House elections, with the equilibrium being supported by all eligible voters having rational expectations about the election outcome. The analysis, using survey data, extends and refines the evidence of the existence of such a large-scale noncooperative equilibrium previously produced using aggregate time series data in connection with models of the American political economy (Alesina and Rosenthal 1995, 1989; Alesina, Londregan, and Rosenthal 1993). Mebane (2000) and Mebane and Sekhon (2002) contradict much of the public opinion literature. They assume that voters behave strategically with rational expectations and common knowledge. Such assumptions require voters to act as if they are better informed than they actually are.

Notwithstanding the forgoing, the existing literature provides little empirical evidence for the contention that cues and heuristics are sufficient to produce election results that match what would happen if voters were well informed (Bartels 1996). My strategy for analyzing this question is to examine whether voters who change information state behave differently from voters who do not. For causal inference, comparing across information levels (instead of changes in information levels) is not a good approach. In the next section I explain in detail why this is the case.

Using propensity score matching and new robust methods, I show that when voters are matched on baseline characteristics, voters who change information state vote no differently on election day than voters who do not. Before election day, however, voters who change information state do differ in their vote intentions. This effect is present in September but insignificant by October. The election campaign appears to provide the necessary cues to make the differences vanish by election day. I am hardly the first to note people learn during

the course of a campaign (e.g., Alvarez 1997; Johnston, Blais, Brady, and Crête 1992), but this is, to my knowledge, the first analysis to show that information effects on vote choice vanish by election day.

3 Information

Since we are interested in estimating the causal effect of change in the level of information, we require panel data to conduct the analysis. We also obviously need good measures of information, the outcome of interest, and a set of baseline variables sufficient to make the unconfoundedness assumption reasonable. The NES rarely collect panel data, and before 1968, the information questions are of poor and highly variable quality. In particular, the 1956-1958-1960 panel is unusable because the 1956 survey does not contain sufficient information indicators.

With these restrictions in mind, three different NES datasets are analyzed. Two of the three datasets extend across elections. These two datasets allow us to examine if changes in information state over a medium period of time have causal effects. These datasets are the 1972-1974-1976 and 1992-1994-1996 panel studies. The third dataset consists of a four wave panel which was conducted during the 1980 campaign. This allows us to examine if short term changes in information have an effect on vote intentions and voting behavior. All three datasets allow us to examine separately if the effect of information is present in the September or October before an election and if it is present on election day. In all three datasets we will find significant information effects in September but in neither October nor November.

For both of the three wave panels, baseline is defined as the first presidential election observed which is either 1972 or 1992 (post election survey). Treatment is defined as the change in information state from the first presidential election to the midterm election (post election survey). And the outcomes of interest occur in the second observed presidential

election, 1976 and 1996. The two year gap between baseline and the treatment variables keep issues of post treatment bias to a minimum and the similar two year gap between the definition of treatment and outcomes helps to minimize issues of endogeneity.

For the 1980 four wave panel study, baseline is defined as the first wave which was conducted between January 22 and February 25. Treatment is defined as change in information state between the first and second wave which was conducted between June 4 and July 13. The two outcome points of interest are the third wave which was conducted between September 2 and October 1 and the fourth wave which was conducted between November 5 and November 25.²

In the 1996 survey, whether a respondent was interviewed in September or October was randomly assigned. More precisely, four random replicates were used in the pre-election survey. They were released as follows: September 3, September 12, September 26, and October 10. Therefore, I consider the September observations for the 1996 survey to be those which come from the first two replicates and the October observations to be those which come from the last two replicates.³ In the 1980 survey, the September observations are from the third panel wave which was conducted between September 2 and October 1. There were no interviews in October. The 1976 survey poses a problem because the month respondents were questioned was not randomized in the pre-election survey, and randomization tests show ($p=0.000$) that whether a respondent was interviewed in September or October was *not* random. Moreover, the 1976 pre-election survey did not begin interviewing respondents until well into September: September 17. Therefore, for the 1976 survey, pre-election results broken down by month are not presented.

To keep the discussion simple, I present results for a binary outcome and a binary treatment. The results for multinomial outcome and treatment are generally the same but the presentation is significantly more complicated. Moreover, dividing the treatment into mul-

²In an analysis not reported, the effect of changing information states between the second and third waves on the fourth wave was also examined. No such effect was found to exist.

³If respondents from the first two replicates were not interviewed by September 26, they are not included in the data analysis. If these deleted respondents are included, the results do not significantly change.

multiple categories decreases the likelihood of finding a significant effect because the data are being chopped up into smaller parts—for example, there are fewer legitimate matches and the common support assumption forces one to drop more observations. And given that my hypothesis is that there is *no* information effect by election day, dropping observations would bias the case in my favor.

The problem of measuring political information would be difficult if domain specific knowledge was important and was separate from general political knowledge (Iyengar 1986). There is some evidence which supports the contention that domain specific knowledge is important (Holbrook, Berent, Krosnick, Visser, and Boninger 2004; Iyengar 1990; McGraw and Pinney 1990). But domain specific measures have difficulty producing effects which are not explained by general knowledge measures (Zaller 1986). It is apparently the case that people who are informed about one political issue are also highly likely to be informed about others (Price and Zaller 1993).

General measures of information do almost as well as a special domain specific battery of 27 information questions which was used in the 1985 NES pilot study (Zaller 1986). These general measures include the correct placing of political parties and candidates on the left-right dimension and naming of elected officials. Another general measure is to rely on the interviewer's judgment of how informed the respondent is. This is a five point scale which ranges from "very high" information to "very low." At least in face-to-face interviews with considerable political content, these ratings have been shown to have high reliability and comparability with more involved measures (Zaller 1985, 1986).

Zaller (1986) concludes that interviewer ratings "are highly effective as measures of political information" (18). These ratings have an estimated reliability of 0.78 while the 1985 pilot study scale which consisted of 27 items, a scale which Zaller calls the Cadillac information scale, has an estimated reliability of 0.89 (Zaller 1986). This Cadillac is not available outside of the pilot study, and most of the information scales available in the NES have reliabilities of about 0.8 to 0.85. These are very high reliabilities for as Bartels's points out the

usual seven-point issue scales have reliability coefficients of about .4 to .6. The interviewer scale apparently is not biased by the respondents' race, gender, education and income (Zaller 1985). If systematic biases like these did exist, they would not be a problem for my analysis because individuals are matched on all of these characteristics.

I use both the interviewer ratings of information and Zaller's information measure (Zaller 1992, forthcoming) which largely consists of factual information questions⁴ and interviewer ratings. Not surprisingly given the high degree of correlation between the measures (indeed, Zaller heavily weights the interviewer ratings), my results are substantially the same regardless of which measure I use. Because I need to make comparisons over time (fortunately a relatively short period of time), the question arises of how to compare the Zaller information measure across time.⁵ Given this, it appears to be simpler to either use the interviewer ratings or to construct a measure using a subset of the factual questions in Zaller's measure which are the same in the two periods in question. This modified Zaller measure yields the same substantive results as those reported here based on the interviewer information ratings. Bartels (1996, 2003) also relies on these interviewer ratings.

Some may be concerned that panel attrition is a significant issue. And indeed there are reasons to be concerned that panel attrition is correlated with information measures. But concern about panel attrition should not be exaggerated. Bartels (1999) presents evidence that panel attrition only rarely leads to significant bias with NES data.

In any case, multiple imputation provides a straightforward way in which to correct for panel attrition. Because the NES surveys include fresh (non-panel) samples, a relatively general correction for attrition is possible. This correction, developed by Hirano, Imbens, Ridder, and Rubin (1998) and called the additive nonignorable (AN) model, allows for attrition based on lagged variables and for attrition based on contemporaneous variables.

⁴These factual questions largely consist of queries about placing candidates and parties on the left/right dimension and correctly naming public officials and naming the majority parties in the House and Senate.

⁵The naive approach of making the Zaller index comparable over time by looking at what quantile of the measure the respondent falls into has been tried, and the substantive results are the same as those reported here.

The former type of attrition is called *selection on observables* because it relies on the *missing at random* assumption of Rubin (1976) and Little and Rubin (1987), and the latter type of attrition is called *selection on unobservables* because attrition partly depends on variables that are not observed for respondents who drop out. The former was developed by Rubin (1976) and the latter by Hausman and Wise (1979). When the AN correction is applied, my substantive results do not significantly change. Because imputation does not change the substantive results, the non-imputed results are presented in this paper.⁶

4 Causal Inference

The goal is to estimate the causal effect of information on vote choice. Making causal inferences from NES data is a difficult task. Many (if not most) of the measures known to be correlated with vote choice are largely endogenous. But one of the virtues of the NES is that more than fifty years of experience has gone into constructing measures which are correlated with vote choice and other politically salient variables. And much of the variance of vote choice is soaked up by the usual kitchen sink models. Because soaking up variance is not the same thing as explanation, I use a methodological approach which relies on this feature of the surveys and coherently deals with the problems of endogeneity and selection associated with observational data.

The NES lend themselves to analysis by the Rubin causal model and associated methods (Holland 1986; Rosenbaum 2002; Rubin 1974, 1978, 1990) because they contain a large set of relevant covariates which help to make one of the key identifying assumptions of the approach tenable.⁷ And importantly, at least some NES studies contain a panel component. The Rubin model conceptualizes causal inference in terms of potential outcomes under treatment

⁶Imputation is done using **Splus** code developed by me which adapts Schafer's (1997a; 1997b) imputation code for the purposes of correcting for panel attrition using the AN model for categorical data. Schafer's code is available at <http://www.stat.psu.edu/~jls/misoftwa.html>.

⁷Although it is often called the Rubin causal model, it goes back to at least Fisher and Neyman (e.g., Fisher 1935; Splawa-Neyman 1990 [1923]).

and control, only one of which is observed for each unit. A causal effect is defined as the difference between an observed outcome and its counterfactual (Sekhon 2004a).

Let Y_{i1} denote the vote choice (i.e., the outcome) when voter i changes information state (i.e., is in the treatment regime), and let Y_{i0} denote the vote choice when voter i does not change information state (i.e., is in the control regime). The causal inference problem is a missing data problem because both Y_{i1} and Y_{i0} cannot be observed—a given voter cannot both change and not change information state at the same time. Let T_i be a treatment indicator: 1 when i is in the treatment regime and 0 otherwise. The observed outcome for observation i is then $Y_i = T_i Y_{i1} + (1 - T_i) Y_{i0}$. The treatment effect for observation i is defined by $\tau_i = Y_{i1} - Y_{i0}$.⁸

In principle, if assignment to treatment is randomized, the inference problem is straightforward because the two groups are by construction drawn from the same population. The observed and unobserved baseline variables of the two groups are balanced; treatment assignment is independent of all baseline variables. This occurs with arbitrarily high probability as the sample size grows. With the independence assumption, the missing data problem is simple to resolve. Following Dawid's (1979) notation, $\{Y_{i0}, Y_{i1} \perp T_i\}$. Hence, for $j = 0, 1$

$$E(Y_{ij}|T_i = 1) = E(Y_{ij}|T_i = 0) = E(Y_i|T_i = j)$$

Therefore the (average) treatment effect can be estimated by:

$$\begin{aligned} \tau &= E(Y_{i1}|T_i = 1) - E(Y_{i0}|T_i = 0) \\ &= E(Y_i|T_i = 1) - E(Y_i|T_i = 0) \end{aligned} \tag{1}$$

It is possible to estimate Equation 1 because observations in the treatment and control groups are exchangeable. In the simplest experimental setup, individuals in both groups are equally likely to receive the treatment, and hence assignment to treatment will not be asso-

⁸For comparability with the literature, the notation here is the same as in Dehejia and Wahba (1999).

ciated with anything which may also affect one’s propensity to vote in a particular fashion. Even in an experimental setup, much can go wrong which requires statistical correction (e.g., Barnard, Frangakis, Hill, and Rubin 2003; Imai forthcoming). In an observational setting, unless something special is done, the treatment and non-treatment groups are almost never balanced.

With observational data, the treatment and control groups are not drawn from the same population. Thus, a common quantity of interest is the average treatment effect for the treated (ATT):

$$\tau|(T = 1) = E(Y_{i1}|T_i = 1) - E(Y_{i0}|T_i = 1). \quad (2)$$

Unfortunately, Equation 2 cannot be directly estimated because Y_{i0} is not observed for the treated. Progress can be made if we assume that the selection process is the result of only observable covariates denoted by X_i . In particular, we assume that treatment assignment is strongly ignorable given X (Rosenbaum and Rubin 1983)—i.e., $\{Y_{i0}, Y_{i1} \perp T_i\} | X_i$.

Then, following Rubin (1974, 1977) we obtain

$$E(Y_{ij}|X_i, T_i = 1) = E(Y_{ij}|X_i, T_i = 0) = E(Y_{ij}|X_i, T_i = j). \quad (3)$$

Therefore, conditioning on the observed covariates, X_i , the treatment and control groups have been balanced. The average treatment effect for the treated can then be estimated by

$$\tau|(T = 1) = E\{E(Y_i|X_i, T_i = 1) - E(Y_i|X_i, T_i = 0) | T_i = 1\}, \quad (4)$$

where the outer expectation is taken over the distribution of $X_i|(T_i = 1)$ which is the distribution of baseline variables in the treated group.

The most straightforward and nonparametric way to condition on X_i is to exactly match on the covariates. This is an old approach going back to at least Fechner (1966 [1860]), the father of psychophysics. This approach of course fails in finite samples if the dimensionality

of X is large. And if X consists of more than one continuous variable, exact matching is inefficient: matching estimators with a fixed number of matches do not reach the semi-parametric efficiency bound for average treatment effects (Abadie and Imbens 2004).

An alternative way to condition on X_i , is to match on the probability of being assigned to treatment—i.e., the propensity score.⁹ If one matches on the propensity score, one will obtain balance on the vector of covariates X_i (Rosenbaum and Rubin 1983).

Let $p(X_i) \equiv Pr(T_i = 1|X_i) = E(T_i|X_i)$. This defines $p(X_i)$ to be the propensity score. If we assume that $0 < pr(T_i|X_i) < 1$ and that $Pr(T_1, T_2, \dots T_N|X_1, X_2, \dots X_N) = \prod_{i=1}^N p(X_i)^{T_i} (1 - p(X_i))^{(1-T_i)}$, then as Rosenbaum and Rubin (1983) prove,

$$\tau|(T = 1) = E \{ E(Y_i|p(X_i), T_i = 1) - E(Y_i|p(X_i), T_i = 0) \mid T_i = 1 \},$$

where the outer expectation is taken over the distribution of $p(X_i)|(T_i = 1)$.

Since $p(X_i)$ is generally unknown, it must be estimated. While the logistic model is often used, a variety of semi-parametric approaches may be used instead. But it is important to keep in mind that none of the coefficients of this model are of interest and individual coefficients do not need to be estimated consistently. Indeed, the propensity model does not need to be uniquely identified—i.e., the coefficient estimates need not be unique for a given propensity value.

There are eight important implications of the forgoing that I wish to highlight. First, the modeling portion of the estimator is limited to the model of $p(X_i)$. Estimation of this model requires **no** knowledge of the outcome. Once balance has been obtained, any outcome can be estimated. Hence, one can conduct model selection (which in this case centers on achieving balance in the X variables) without ever observing what the estimated outcome is under the various models. This is an incredibly important virtue of this approach, and it cannot be stressed enough given the problems of data mining in observational work. Unlike

⁹The first estimator of treatment effects to be based on a weighted function of the probability of treatment was the Horvitz-Thompson statistic (Horvitz and Thompson 1952).

in the regression case, there is a clear standard for choosing an optimal model; it is the model which balances the covariates, X . All of the propensity models in this paper were chosen before any outcome was estimated.

Second, the key assumption required is that no variable has been left unobserved which is correlated with T_i and with the outcome. This is called the unconfoundedness assumption. If such a variable exists, there is hidden bias. The rich set of covariates in the NES data make this assumption more plausible than in most observational datasets. But all observational work is open to the criticism of hidden bias. Fortunately, rigorous sensitivity tests are available to determine how robust the results are to hidden bias (Rosenbaum 2002, 105–170).

Third, no implausible and untestable exclusion assumptions are required as is necessary with instrumental variable and related structural approaches such as Heckman selection models (Heckman 1979) and models based on Judea Pearl’s work on causality (Pearl 2000). Fourth, once the propensity score has been matched on (or if one uses direct matching), any outcome quantity of interest can be simply calculated, be it the mean or any quantile (Rosenbaum 1999). Fifth, no functional form is implied for the relationship between treatment and outcome. No homogeneous causal effect assumption has been made; the causal effect may vary with the propensity score (and hence with values of X_i). Sixth, as noted before, since we are interested in a lower dimensional representation of X_i , in particular $p(X_i)$, we do not need to estimate consistently any of the individual parameters in our propensity model. Seventh, use of this approach (unless considerable adjustments are made) requires the assumption that “the observation on one unit should be unaffected by the particular assignment of treatments to the other units” (Cox 1958, §2.4). Rubin (1978) calls this “no interference between units” the stable unit treatment value assumption (SUTVA). SUTVA implies that the potential outcomes for a given voter do not vary with the treatments assigned to any other voter, and that there are no different versions of treatment. The first part of the SUTVA assumption is true by construction in the NES because the sampling

procedure all but ensures that none of the individuals in the sample know each other. For example, only one person is interviewed in each household.¹⁰

Finally, because we are taking a conditional expectation, we need to decide what to condition on. If we condition on too little, our estimates are confounded and therefore biased. If, however, we condition on variables which are not baseline variables but the result of treatment, we obtain biased estimates because of post-treatment bias (Rosenbaum 2002).

Without data over time, it is all but impossible to decide what is baseline and what is post treatment. An example of the problem can be seen by examining Bartels's (1996) cross-sectional analysis of the effect of information on voting in U.S. presidential elections. In his models, he does not condition on variables such as partisanship and ideology which are usually included in vote models because he reasonably conjectures that these variables may be the result of information. But income and education are included which could also be causally related to how informed one is. Because the estimated vote models exclude variables which are known to be important for the vote, such as partisanship and ideology, the models do not fit the data well and many extreme y -misclassification errors are present. With such outliers, inferences based on maximum-likelihood are generally inconsistent while robust estimators support reliable inferences (Cantoni and Ronchetti 2001; Hampel, Ronchetti, Rousseeuw, and Stahel 1986; Huber 1981; Mebane and Sekhon 2004). When we rely on robust estimates, no information effects are found.¹¹ The robust estimator used is a new robust binary logistic model developed by me (Sekhon 2004b) which combines the down weighting of y -misclassification outliers of the conditionally unbiased bounded-influence approach of Kunsch, Stefanski, and Carroll (1989) with a high breakdown point Mallows class estimator for down weighting x -outliers (Carroll and Pederson 1993). See Appendix A for details.

The key point from the Bartels example is not that robust estimation changes the answer, but that in cross-sectional data it is all but impossible to decide what is baseline and what

¹⁰There is the issue that cluster sampling is used instead of simple random sampling, but that surely is an unimportant distinction given the size of the sample and population.

¹¹The information effects also vanish if we include partisanship, ideology, first order interactions and simply use maximum likelihood.

is not. The only possible hope would be to write down competing formal models and to empirically test them. This would, however, be a highly parametric approach.

When attempting to estimate causal effects, examining changes over time greatly reduces (but does not eliminate) the problem of deciding what is baseline and what is not. A variety of other estimation problems also become easier. For example, the unconfoundedness assumption becomes more tenable because previous values of the outcome of interest can be conditioned on. This is old advice. Indeed, it is present in Yule’s (1899) classic analysis of pauperism in England which is the first recognizably modern use of regression in the social sciences.

The question then, is whether voters who change information level—i.e., voters who become either less or more informed—behave any differently than those who do not. If information is causally important, changes in the level of information should have an effect on behavior. The outcome of interest is change in vote from baseline.

With this methodological setup, I will be able to detect if changes in the level of information change the mean probability of voting for a particular choice and if changes in the level of information result in increased choice volatility. The question of information affecting the variance of political opinions has been previously explored using heteroskedastic choice models in the work of Alvarez and Brehm (1995; 1997; 1998; 2002). And Franklin (1991) uses a heteroskedastic model to explore variations in uncertainty due to candidate campaign behavior.

5 Estimation and Results

5.1 The Setup

The setup is first discussed for the 1972-1974-1976 and 1992-1994-1996 panels. Let t denote the time of the survey. Let $t = 1$ denote the baseline year, which is 1972 for the first panel and 1992 for the second. Let $t = 2$ denote the middle year, 1974 and 1994. Let $t = s$

denote the September of the terminal year (e.g., September 1996). Let $t = o$ denote the October of the terminal year. And let $t = p$ denote the post election survey for the terminal year.

Let $I_{i,t}$ denote the information state of voter i at time t . A voter is considered to be informed if s/he is measured to be either “highly” or “very highly” informed by the interviewer information rating.¹² Let $I_{i,t} = 1$ if voter i at time t is informed, $I_{i,t} = 0$ otherwise.

Since we are interested in estimating the causal effect of changing information levels, voter i is considered to be in the treatment regime if $I_{i,t_1} \neq I_{i,t_2}$, then $T_i = 1$. Because the effect of increasing information level may be substantially different from the effect of decreasing information level, the causal effect of increasing information level (i.e., $I_{i,t_1} < I_{i,t_2}$) is estimated separately.¹³

Let $V_{i,t}$ denote voter i 's vote intention or choice at time t . V equals 1 if the voter prefers the Republican presidential candidate, V equals 2 if the voter prefers the Democratic candidate, V equals 3 if the voter prefers a third party candidate, and V equals 4 if the voter is undecided or if the voter did not vote. The outcome is denoted by $Y_{i,t}$, and $Y_{i,t} = 1$ if $V_{i,1} \neq V_{i,t}$, where t may equal one of the three outcome time points: s, o, p .

For the 1980 panel survey, the same structure is used except that the time periods are obviously much closer to each other. The baseline time period, $t = 1$, corresponds to the first panel wave which was conducted between January 22 and February 25. The second wave, $t = 2$, was conducted between June 4 and July 13. And the third wave corresponds to $t = s$ which was conducted between September 2 and October 1. There were no interviews in October so for the 1980 panel $t = o$ is not defined. The fourth and final wave was conducted in November and corresponds to $t = p$.

¹²As noted earlier, Zaller's information measures have also been used, and the results remain substantively unchanged.

¹³The causal effect of decreasing information level has also been estimated for each of the panels. These results are not presented because they do not add anything over what is learned from the two quantities discussed in this paragraph.

Table 1 presents the total number of respondents who increased or decreased information state in the various datasets under consideration. The table does not include observations lost due to panel attrition but recovered by imputation. The table makes clear the rationale for first examining respondents who change information level. When the information movements are disaggregated, the number of observations becomes rather small for some months. But note that the null information results are found for October and November—the months with the second and first largest number of observations—not for the month with the fewest observations which is September.

5.2 Balance

In order to correct for confounding of observed variables in treatment assignment, a combination of exact and propensity score matching is done. Because most all of the variables measured in the NES are discrete, it is theoretically possible to use exact matching only, but the curse of dimensionality soon gets one: there simply are too many variables and too few observations to do this. So instead, a hybrid approach is used. Exact matching is used for baseline partisan identification and whether the respondent voted for the Republican presidential candidate in the baseline survey. Both variables are highly correlated so exact matching on both is not an unrealistic demand. These variables are of such importance in American politics that exactly matching on these variables gives one good balance for most every other variable measured by the NES. But the balance can be significantly improved by using propensity score matching for the other variables in the NES. In principle, every variable which is measured in the baseline survey should be balanced. Even if a given variable is not itself causally related to outcome, something which is correlated with it may be so it is safest to achieve balance on everything.

Although a kitchen sink approach has been tried for the propensity model, it has been found that putting the following variables into the propensity model achieves balance on not only these variables but for all of the other variables in the NES which have been

examined. These variables are: the 7-point liberal-conservative ideology scale, education, home ownership status, retired, housewife, union membership, black, east, south, west, religion, age, family income, the changing list of seven point policy scales and retrospective economic evaluations. All variables except for age, education and income are included in the propensity model as fully factored indicator variables.¹⁴

The more highly educated are less likely to change information state as are homeowners, retired people and (in the 1972-1974-1976 panel) union members. Political independents are more likely to change information state than others—they are also generally less informed.

One-to-one nearest-neighbor matching with replacement is used because it provides the best balance, and because theory dictates that one-to-one matching minimizes the expected bias.¹⁵ Moving to one-to-three or one-to-five matching (both of which generally provide smaller variance estimates) does not make any of the insignificant results reported here become significant.

Both univariate and multivariate balance is evaluated. First, univariate balance is judged by two non-parametric tests. The first, the Wilcoxon rank sum test (Wilcoxon 1945), is well suited for testing for differences in the first moment and its properties have been extensively studied (Hettmansperger 1984). This test is equivalent to the Mann and Whitney test (Mann and Whitney 1947). For paired binary data, I use the McNemar test of marginal homogeneity (McNemar 1947).

Univariate tests are not sensitive to the relationships between variables. So the main

¹⁴Age is included as is the square of age. Education is included (as measured by years of schooling) and an indicator variable for whether the respondent has more than the median years of schooling. Family income is ranked and divided up into five quantiles, and an indicator variable is included for each quantile. Observations with missing values are not dropped, but matched on. Thus, the usual seven point scales are really eight point scales. First order interactions are added for those variables found to be unbalanced without them.

¹⁵Matching is done using a **R** (<http://www.r-project.org/>) wrapper that I developed for Abadie's and Imbens's Matlab matching code. For my results, Abadie and Imbens standard errors are substantively the same as those obtained from estimating a simple regression on the matched data. **R** software for checking for univariate and multivariate balance of the covariates before and after matching was developed by me. Abadie's and Imbens's code is available at <http://emlab.berkeley.edu/users/imbens/estimators.shtml>, and my **R** wrapper and balance checking code is available from me.

test for balance is to compare the two propensity distributions (for treated and control) using the Kolmogorov-Smirnov test for equality. This is a nonparametric test based on the Kolmogorov distance between the two empirical distribution functions (Knuth 1998; Wilcox 1997). Another multivariate test is offered by running a logistic model in which the dependent variable is treatment assignment and all of the baseline variables are entered on the right hand side.¹⁶ If the residual deviance of this model is significantly less than the null deviance, there is evidence against balance.

The multivariate test results reported in Table 2 show that before matching there is a great deal of imbalance and that after matching balance has been obtained. The table presents the p -values for both the Kolmogorov-Smirnov and likelihood-ratio tests for equality of the propensity score densities of those who changed information state and of those who did not. For the 1990s panel, the Kolmogorov-Smirnov test yields a p -value of 7.503×10^{-8} before matching and a p -value of 0.977 after matching. This ridiculously low p -value should not be interpreted literally, but the conclusion is clear: the two groups are profoundly unbalanced before matching and are balanced afterwards. For the 1980s panel, the test yields a p -value of 9.557×10^{-9} before matching and a p -value of 0.929 after matching. For the 1970s panel, the test yields a p -value of 4.943×10^{-13} before matching and a p -value of 0.523 after matching. The likelihood ratio tests tell the same substantive story.

Figure 1 displays the densities of the propensity scores for the voters who changed information level and for those who didn't. The figure visually confirms what the formal tests just demonstrated: the densities of the two groups are markedly different before matching and after matching are almost exactly the same. There is common support between both groups in each dataset, and so there is no need to drop any of the treated observations. With observational data, it is all too rare to have such common support (Heckman, Ichimura, Smith, and Todd 1998).

¹⁶The test was also run including all first order interactions, and the inferences were the same.

5.3 Results

Table 3 presents the ATT estimates for the effect of changing information levels on changing vote intention or choice. There is a significant effect in September but none in October or November. In all surveys, the standard errors are larger than the estimated effects for all months except September. The estimated effect in September is largest in the 1990s panel in which voters whose level of information changed between 1992 and 1994 were 36% more likely to change their partisan vote intention between 1992 and September 1996 than voters whose level of information did not change. The estimated causal effect in this panel is more than three times larger than the causal effect estimated in the 1980 panel, but these two effects are impossible to compare because the changes being considered in the 1980 panel occur over a much shorter time-period.

Although the September effect for the 1980 panel is smaller than for the 1990s panel, it is still substantively large. In 1980, voters whose information level changed between January and June were 10% more likely to change their partisan vote intention from January to September than voters whose information level did not change. Recall that because the 1976 NES survey did not randomize whether respondents were interviewed in September or October, monthly pre-election estimates are not presented for 1976. For completeness, I have estimated for the 1976 pre-election survey (which interviewed respondents between September 17 and November 1) the effect of changing information between 1972 and 1974. This estimate is 0.00143 with a standard error of 0.0493. Given the results for the other panels, an insignificant estimate is to be expected because most of the pre-election respondents were interviewed in October and because no interviews at all were conducted in the first half of September.

Table 4 displays the estimates for the causal effect of increasing the level of information. These results are consistent with the results presented in Table 3. There is no evidence for information effects in October and November, but there is evidence for information effects in September. In the 1992-1994-1996 panel, respondents whose level of information increased

were 43% more likely to change their September vote intention from baseline than those whose level of information did not change. Although this point estimate is about 7% larger than that obtained for the causal effect of any change in the level of information, these two estimates are not statistically different. In the 1980 panel, voters whose level of information increased between January and June were 12% more likely to change their partisan vote intention from January to September than voters whose level of information did not change. This estimate is statistically indistinguishable from the estimate for the causal effect of any change in information which is 10%. As before, all of the standard errors for the results in October and November are larger than the corresponding point estimates.

All of these results tell a consistent story. By election day, there is no effect of information on changing one's vote. These results show that between September and election day something significant changes in American political life which negates the effects of information on voting behavior.

The null results for October and November show that changes in the level of information neither change the candidate voters are likely to choose nor change the stability of their vote choices. Even if increased information has no effect on the probability of voters choosing a particular candidate—e.g., if informed voters are no more likely to choose one particular party over another, increased information may nevertheless increase the stability of their choices (Alvarez and Brehm 2002). If this were the case, there should be significant negative estimates in Table 4 because the outcome variable is an indicator variable for voter i changing her partisan vote intention in 1996 from what it was in 1992. But since there are no significant estimates in October and November, we can conclude that changing information state does not alter the stability of voters' vote intentions nor does it alter what candidate they prefer.

6 Conclusion

Although I present no evidence of the mechanisms by which voters are able to perform information arbitrage, there are many implications of their ability to do so. This is an important demonstration of the power of electoral institutions in the U.S. which along with markets are the primary methods by which individual preferences are aggregated. I conjecture that information effects would be present even on election day if the electoral institutions which provide cues were less mature. Thus, I plan to search for such effects in democracies with more rudimentary democratic institutions such as Russia. The conjecture is that as democracies mature, citizens no longer need to stay informed in order to act as if they are. Our current state of relative political disengagement is a high achievement and not a social failing to be lamented as is so often done. Given opportunity costs, it is unreasonable to advocate that all citizens should be politically well informed since they can rely on institutions, such as polls and interest groups (McKelvey and Ordeshook 1985a,b), to make choices as if they are informed.

Notwithstanding the forgoing, the results in this paper should not be taken to imply that voters make choices which are in some general sense optimal. Voting is a simple act which in American politics is, for most voters, a binary choice: Republican or Democrat. Political parties work hard to project a consistent “brand image” which further simplifies the decision for voters. Also, the information measures available in surveys do not discriminate between individuals who are *highly* expert and informed about narrow issues (say issues related to the regulation of particular companies and industries) and those who are not. In any case, such expert voters are an extremely small proportion of the population; almost certainly too small of a proportion for samples of the size of the NES to include a significant number of such voters.

Much of the work of government involves highly technical issues which are not discussed in general political discourse—such as the issue of who benefits from the government debt guarantee for Fannie Mae and Freddie Mac and the extent to which such a guarantee con-

stitutes a moral hazard. There is no evidence in this paper that the public understands, or even knows of, such issues. Thus, one should not conclude from these results that voter behavior, political rhetoric, and policy outcomes would be no different if every American voter knew the details of such issues. But such a level of expertise and information is so unrealistic that it is nearly impossible to conjecture what politics would look like with such people or how such a world could come about. This paper does answer the tractable question of whether changes in the level of information of the magnitude commonly observed have any effect on vote intentions on election day. The answer to this question, contrary to the existing literature, is a resounding no.

A Bartels Replication

Table 5 is a replication of Bartels’s (1996) Table 2 (p. 209). I extend Bartels’s analysis to include results for the 1968 and 1996 NES surveys. When I use maximum likelihood (ML) estimation, the substantive inferences from the replication are exactly the same as those made by Bartels. Using ML, I find significant information effects in the same years that Bartels does. The table also displays estimates from robust estimation. The robust estimator used is a new robust binary logistic model developed by me (Sekhon 2004b) which combines the down weighting of y -misclassification outliers of the conditionally unbiased bounded-influence approach of Kunsch et al. (1989) with a high breakdown point Mallows class estimator for down weighting x -outliers (Carroll and Pederson 1993).¹⁷ Given the categorical nature of NES variables, there are no x -outliers. Model selection is done using the theory developed by Cantoni and Ronchetti (2001). My robust estimation computer code is written in **R** (<http://www.r-project.org/>) and is available upon request.

Using ML, one finds significant information effects in four of the eight elections examined. Using robust estimation, however, significant information effects are never found.

As stated in the main text, Bartels does not include variables such as partisanship and ideology which are usually included in vote models because these variables may be the result of information. The estimated vote models leave out variables which are known to be important for the vote. It is not surprising then that the models do not fit the data well and that many extreme y -misclassification errors are present. In the presence of such outliers, inferences based on ML are generally inconsistent while the robust estimator supports reliable inferences (Cantoni and Ronchetti 2001; Hampel et al. 1986; Huber 1981; Mebane and Sekhon 2004). Hence, it is unsurprising that the ML and robust estimation results differ.

¹⁷ML binary logistic models yield the same substantive inferences as obtained by Bartels’s binary Probit models.

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Table 1: People Who Changed Information State (no imputation)

Year	September	October	November
1976			
Total observations	337	449	794
Change Information State	99	122	221
Information State Δ Up	62	60	122
1980			
Total observations	563		563
Change Information State	126		126
Information State Δ Up	66		66
1996			
Total observations	98	148	246
Changed Information State	21	46	67
Information State Δ Up	14	27	41

The table does not include observations lost due to panel attrition but recovered by imputation. Each cell displays the number of respondents in that category. The counts are restricted to respondents from the panel surveys. November observations are from the post-election surveys. In 1980, no interviews were conducted in October.

Table 2: Multivariate Tests for Balance, Before and After Matching

Data	Before Matching		After Matching	
	K-S	LR	K-S	LR
1992-94-96	7.503×10^{-8}	0.00257	0.977	0.779
1980	9.557×10^{-9}	0.0310	0.929	0.865
1972-74-76	4.943×10^{-13}	0.0102	0.523	0.888

The p -values in the K-S column are for the Kolmogorov-Smirnov test for equality of the propensity score densities of those who changed information state and of those who did not. The p -values in the LR columns are for the LR test of deviance (null deviance minus residual deviance) based on the list of covariates described in the text.

Table 3: Estimated Effect of Changing Information State

Data	September	October	November
1992-94-96	0.357 (0.137)	0.0444 (0.0915)	-0.0161 (0.0828)
1980	0.102 (0.0432)		-0.0309 (0.0515)
1972-74-76			0.0293 (0.0464)

Table 3 presents the ATT estimates of change in information level on changing one's vote from baseline. In 1980, no interviews were conducted in October. The 1976 pre-election survey did not randomize the month respondents were interviewed so monthly estimates are not presented. See text for details.

Table 4: Estimated Effect of Increasing Information State

Data	September	October	November
1992-94-96	0.429 (0.141)	0.0385 (0.122)	0.0541 (0.109)
1980	0.121 (0.0392)		0.0304 (0.0719)
1972-74-76			0.0176 (0.0480)

Table 4 presents the ATT estimates of increasing information level on changing one's vote from baseline. In 1980, no interviews were conducted in October. The 1976 pre-election survey did not randomize the month respondents were interviewed so monthly estimates are not presented. See text for details.

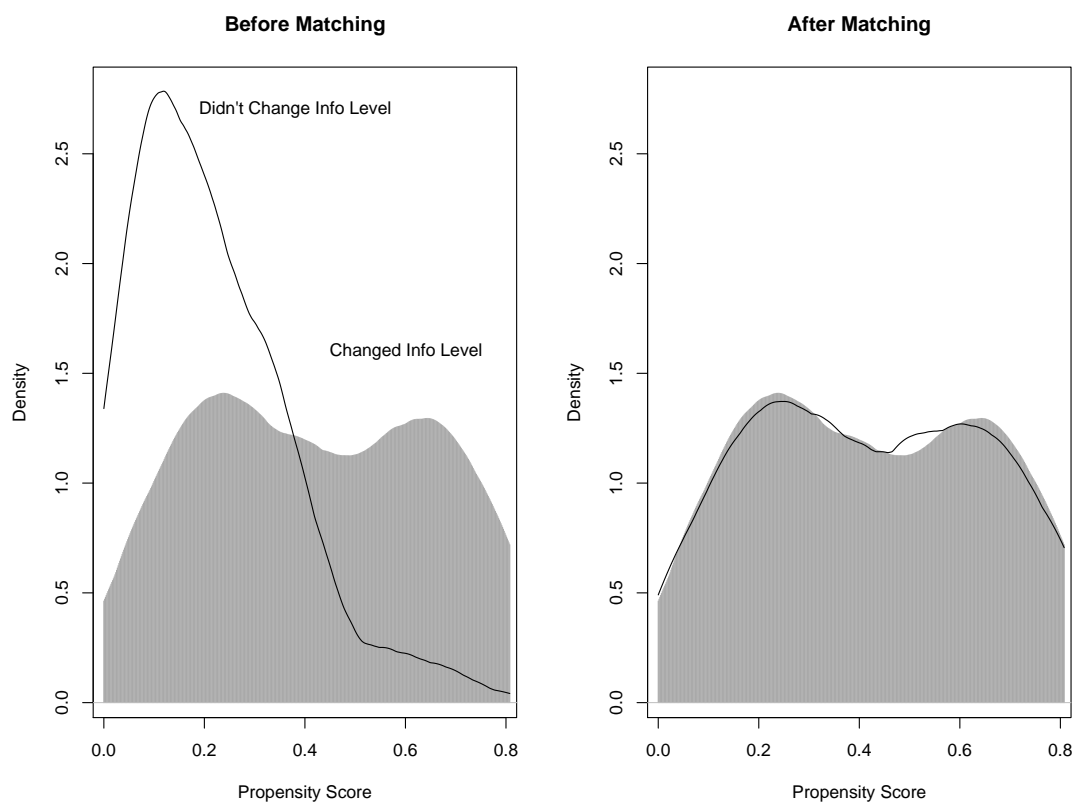
Table 5: Maximum and Robust Likelihood Ratio Tests for Deviations from Fully Informed Voting, 1968–1996

Year	<i>p</i> -value for LR test for likelihood with and without information effects	<i>p</i> -value for robust LR test for robust likelihood with and without information effects
1968	0.394	0.387
1972	0.0118	0.117
1976	0.218	0.682
1980	0.537	0.656
1984	0.0224	0.381
1988	0.287	0.346
1992	0.0216	0.485
1996	0.00549	0.965

Replication of Bartels's (1996) Table 2 plus replication with robust estimation. All *p*-values are obtained from χ^2 statistics with 21 degrees of freedom.

Figure 1: Densities of Propensity Scores Before and After Matching

1992-1994-1996 Panel



1980 Panel

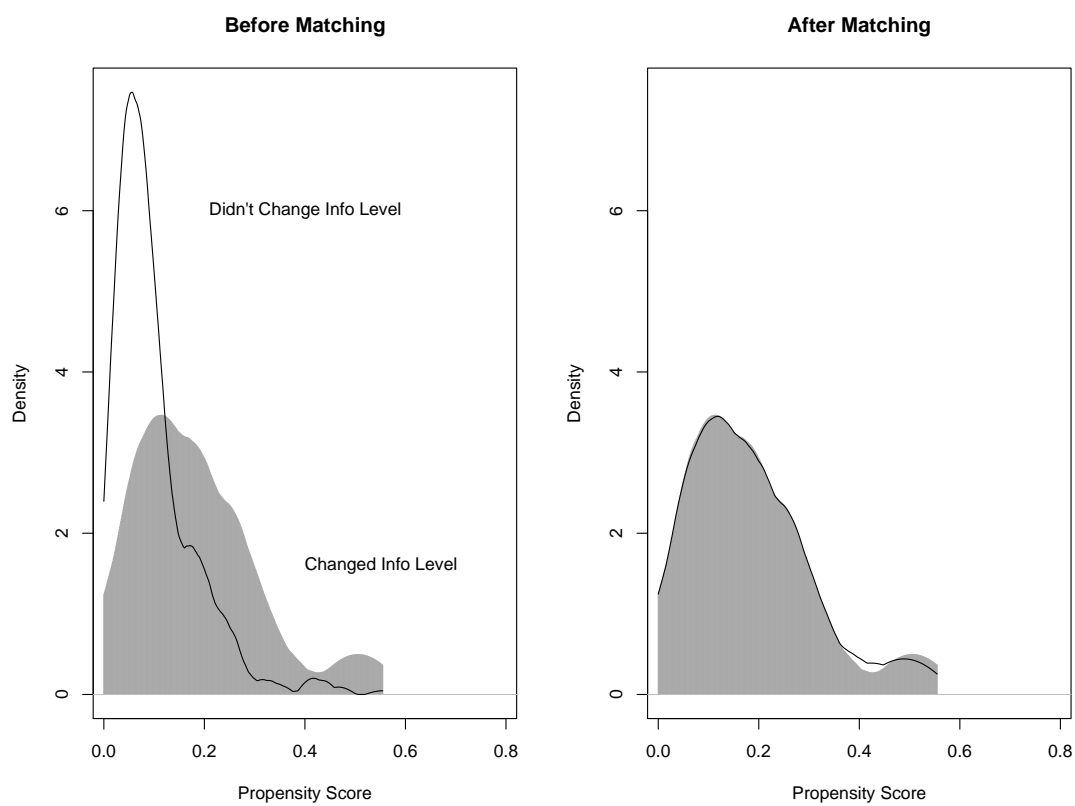


Figure 1 (continued)

1972-1974-1976 Panel

