Just How Addictive is Voting and Why?

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October 28th, 2004

This paper is a very rough draft of a work in progress. Comments regarding any facet of this project are most welcome. The method and the results are completed to my satisfaction, but I have struggled with packaging and presentation. As a result, the middle chapters are fairly polished, but the introduction and conclusion are little more than outlines. Comments regarding the framing of the topic would be greatly appreciated. I'm excited about the project; the trick is to get the reader to be moderately interested in it.

One of the most robust empirical regularities discovered in political science is that past voting behavior is a good predictor of future voting behavior. Three different mechanisms explain the correlation between past and future voting behavior. First, personal characteristics correlated with turnout, such as educational attainment and partisanship, endure and continue to increase or decrease turnout over subsequent elections (e.g., Campbell et al. 1960). Second, an individual's political context consistently exposes her to factors that reinforce turnout or abstinence each election cycle (e.g., Huckfeldt and Sprague 1995). Finally, there may be something in the act of voting itself that is self-reinforcing (e.g., Green and Shachar 2000; Plutzer 2002; Gerber, Green, and Shachar 2003). This paper concerns this last mechanism by estimating the extent to which voting exhibits consuetude, inertia, or is habit forming.

An earlier experiment conducted by Gerber, Green, and Shachar (2003) estimated that voting is 47% habit forming. That is, all things being equal, if a person votes in one election, she is 47 percentage points more likely to vote in the following election. By this estimate, habit formation rivals the magnitude of well-known correlates of voting such as education, income, and education. However, unlike those other relationships that have been studied extensively over the past fifty years, habit formation has not received any additional empirical support or attention.[1] If 47% of the propensity to vote is transferred to one election to the next, then habit formation presents a vast undiscovered terrain for voting behavior. This paper seeks to replicate the Gerber, Green, and Shachar finding through the analysis of eight additional field experiments.

In addition to replication, this paper seeks to contribute to the literature in two additional ways. First, it seeks to improve upon the methods used in prior experiments. While the 47% is unbiased, it does not take into account the dichotomous nature of voting. Both the initial

[1] The one replication experiment I could find was attempted by Michelson 2003. The results are discussed herein.

experimental mobilization and the subsequent follow-up election should be modeled using probit models. This paper replicates earlier findings and then pursues a theoretically correct two-stage endogenous probit model.

The second original contribution of the paper is to examine why voting is habit forming. Two hypotheses are tested: a) the act of voting engenders positive thoughts and thus reinforces its continued behavior; b) the act of voting changes self-perception and one becomes a "voter." In order to test the former, abstainers and voters in prior elections will be scrutinized for differences in habit formation. To test the latter, the voter inertia results will be broken down by age groups to determine whether habit formation is an age specific phenomenon.

The paper concludes that voting is indeed habit forming, but less so that previously thought. Furthermore, only people who have not voted in the election prior to the experiments exhibit habit formation. Thus, the hypothesis that the act of voting takes away the anxiety surrounding future instances of voting is supported. In contrast, no evidence is uncovered that age is associated with habit formation in any respect.

Outline:

1) Method: The experimental estimators to be used are derived and explained.
2) Mechanism: The competing hypotheses for the cause of habitual voting are explained and testable hypotheses are described.
3) Results: The results are presented.
4) Discussion.

Method

For at least forty years, scholars of political behavior have hypothesized that voting is habit forming. The literature concerning the political life cycle has explicitly invoked habit formation to explain increasing rates of turnout as a person ages (e.g., Milbrath 1965, p. 31; Verba and Nie 1972, p. 148; Miller and Shanks 1996, p. 17, 62). Unfortunately, isolating the direct and unique effect of habit is difficult, and until recently the evidence relied upon cross-sectional data and was weak (e.g., Brody and Sniderman 1977).

The problem with cross-sectional studies of voter inertia is that the researcher can never be sure that all enduring causes of voter turnout have been controlled for. An interest in politics or strong sense of duty that caused turnout in election T-1 would also raise turnout in election T. Many underlying causes of voting behavior can be measured and controlled for, but political motivations are complex, amorphous, and often obscured. It is always possible that unobserved heterogeneity accounts for the correlation between turnout at time T-1 and time T. Thus, cross-sectional studies will systematically over estimate the addictive quality of voting because an inability to account for all persistent causes of voting may be inadvertently attributed to the independent effect of inertia.[2]

Unfortunately, a similar critique also applies to habit formation studies utilizing panel data. Green and Shachar (2000) use an instrumental variables approach to correct for persistent unmeasured causes of voting. Plutzer (2002) introduces a latent growth curve analysis of turnout, which models the development of voting habits over time. Both approaches ultimately rest upon modeling assumptions that cannot be tested. The veracity of the assumptions is impossible to gauge without an unbiased external benchmark from which to judge estimates.

[2] Gerber, Green, and Shachar (2003) provide a formal explanation of this point (p. 542-543).

Controlled voter mobilization experiments can offer the necessary unbiased benchmark. A little algebra can demonstrate the nature of the estimator. Let the probability an individual votes in election E, P_E, be expressed by equation 1,

$$P_E = U + S_E + \partial P_{E-1}, \tag{1}$$

where U represents all underlying persistent causes of voting (measured and unmeasured) and S_E represents all election specific causes of the vote (measured and unmeasured). The degree to which turnout in election E-1, P_{E-1}, influences behavior in election E (i.e,, voting is habit forming) is captured by the parameter ∂.

Suppose that a researcher conducts a controlled voter mobilization experiment where subjects are randomly assigned to a treatment group, T=1, that receives encouragement to vote, and a control group, T=0, that receives no blandishments to vote. Equation 2 appends this externally imposed cause of voting behavior to our model where β is the extent to which the treatment boosts turnout.

$$P_E = U + S_E + \partial P_{E-1} + \beta T. \tag{2}$$

Suppose further that the researcher were to track the subjects through another election, E+1, with no further experimental interventions. The probability an individual votes could be expressed as follows:

$$P_{E+1} = U + S_{E+1} + \partial P_E. \tag{3}$$

Substituting equation 2 into equation 3 yields,

$$P_{E+1} = U + S_{E+1} + \partial (U + S_E + \partial P_{E-1} + \beta T). \tag{4}$$

Equation 4 can be split into the following two equations for the treatment and control groups,

$$P_{E+1}^{T=1} = U^{T=1} + S_{E+1}^{T=1} + \partial U^{T=1} + \partial S_E^{T=1} + \partial^2 P_{E-1}^{T=1} + \partial\beta$$
$$P_{E+1}^{T=0} = U^{T=0} + S_{E+1}^{T=0} + \partial U^{T=0} + \partial S_E^{T=0} + \partial^2 P_{E-1}^{T=0} \tag{5}$$

Since the treatment and control groups were randomly determined, $\lim_{n\to\infty}(U^{T=1} - U^{T=0}) = 0$,

$\lim_{n\to\infty}(S_E^{T=1} - S_E^{T=0}) = 0$, and $\lim_{n\to\infty}(P_{E-1}^{T=1} - P_{E-1}^{T=0}) = 0$. If $\text{cov}(S_{E+1}, T) = 0$, then $\lim_{n\to\infty}(S_{E+1}^{T=1} - S_{E+1}^{T=0}) = 0$

and $\partial = \frac{P_{E+1}^{T=1} - P_{E+1}^{T=0}}{\beta}$. Using equation 2 to solve for β yields the following estimate for ∂:

$$\partial = \frac{P_{E+1}^{T=1} - P_{E+1}^{T=0}}{P_E^{T=1} - P_E^{T=0}}. \tag{6}$$

In words, experiments can detect habit formation by dividing the experimental treatment effect in election E+1 by the treatment effect in election E.

The only potentially problematic assumption in the above analysis is $\text{cov}(S_{E+1}, T) = 0$. It is possible that the boost in turnout from receiving the treatment in period E will alter the election specific influenced an individual is exposed to in period E+1, S_{E+1}. For instance, political parties often target residents who voted in the past election for their mobilization campaigns. Such a process would imply $\lim_{n\to\infty}(S_{E+1}^{T=1} - S_{E+1}^{T=0}) > 0$ and the analysis would systematically overstate the degree to which voting is habit forming. In instances when increased turnout in time period E alters the environment in period E+1, it is hard to conceive of a way that turnout in election E+1 will be lower as a result of receiving the treatment. Thus, the habit estimator in equation 6 may over estimate the extent to which voting inertia exists, ∂. That is, in instances where $\text{cov}(S_{E+1}, T) > 0$, equation 6 only serves as an upper bound.

Admittedly, $\text{cov}(S_{E+1}, T) = 0$ is an untestable assumption. The nature of field work and length of time between experiment and the next election make it impossible to definitively rule out all connection between the assignment to treatment in time E and events in time E+1. However, the experimental estimator described in equation 6 has important advantages over the untestable assumptions of observational techniques. First, the experimental technique makes only one assumption. Observational studies of habit formation need to make the same assumption, that behavior in election E does not alter the political context in election E+1, plus a host of other assumptions. In a related point, the assumption also is clear and upfront. A reader can consider the description of the political context of the experiment and judge the validity of the assumption in a particular instance. Using observational techniques, discerning the roles that selection bias, measurement error, and model misspecification play in the analysis is difficult. Finally, unlike observational techniques, the experimental bias is probably only positive, so even in the worst case scenario where $\text{cov}(S_{E+1}, T) > 0$ the reader can take away a useful upper bound on the extent to which voting is habit forming.

It should also be noted that the mechanisms by which $\text{cov}(S_{E+1}, T) \neq 0$ are mediated by voting in election E. The campaigns do not know of, nor are they concerned with, the implementation of the treatment by the researcher. If one views a shift in the electoral environment as a mechanism for voter inertia rather than a confounding factor (Gerber, Green, and Shachar 2003, p. 548), then there is little reason to be concerned about bias in the estimator. However, "habit" implies the behavior perpetuates itself by an internal mechanism, be it psychological or physical. The strategic targeting algorithms used by political campaigns are external to the act of voting and are best considered unintended consequences of voting rather than a mechanism for habit formation. Thus, when contemplating experiments studying the habit formation of voter turnout, the reader should pay attention to how the political context may change in response to a person's decision to abstain or not.

Gerber, Green, and Shachar (2003) used an identical experimental estimator when they examined whether subjects assigned to the treatment group in the 1998 New Haven experiment (Gerber and Green 2000) voted at higher rates in 1999 than the subjects assigned to the control group. More specifically, Gerber, Green, and Shachar find that people who voted in the 1998 Congressional election were 46.7 percentage points more likely to vote in the 1999 New Haven municipal election than people who did not vote in 1998, all things being equal.[3] The experimental estimate comports well with prior observational studies (50% in Green and Shachar 2000). However, the standard error associated with the experimental estimate is large (20%). After speaking with the political parties in New Haven, the authors concluded that the political parties did very little canvassing in 1999 and making the assumption $\mathrm{cov}(S_{99}, T_{98}) = 0$ reasonable.

[Add description and explanation of the two-stage endogenous probit model.]

The size of the experimentally detected effect is extremely large, nearly as large as the effect of age and education. The magnitude of the result should trigger skepticism and scrutiny. Given all the attention that age and education have received with regards to voter turnout, it would be surprising if nearly every political scientist overlooked a cause of voter turnout equally influential. Furthermore, the precise congruence between the observational study and the experimental finding would imply that very little unobserved heterogeneity exists, which seems somewhat unlikely. Finally, the result has not been replicated experimentally. Michelson (2003) followed up on the results from her 2001 Dos Palos study and found no mobilization effect to speak of in 2002. Given the level of uncertainty associated with the Gerber, Green, and Shachar estimate, large-scale replication of the result is in order before the 47% habit effect is accepted at

[3] "All things being equal" is an apt and accurate phrase for randomized experiments.

face value. The next section discusses reasons why voting may be habit forming and derives testable hypotheses.

Mechanism

Gerber, Green, and Shachar (2003, p. 548) posit four possible mechanisms by which voting might be habit forming. First, "the political environment reinforces one's level of political participation … because parties and interest groups have an incentive to focus their attention on active voters." In other words, the person who votes in an election is more likely to be mobilized in future elections by political groups than the person who abstains in an election. Notice that this mechanism for habit formation relies upon a person's response to further blandishments to vote rather than the act of voting itself. By this explanation, when the outside parties cease to mobilize the individual, her rates of voter participation will return to "normal" levels. Voter inertia is not the behavior explained; rather, Gerber, Green, and Shachar provide a mechanism for a change in political context ala Huckfelt and Sprague. As noted above, interference from outside groups in response to voting behavior constitute a source of bias for the analysis. In short, a change in political context is not an explanation as to why voting behavior might be self-reinforcing.

The second reason voting might be habit forming is because it might "alter certain broad psychological orientations known to influence voter turnout, such as feelings of civic obligation, levels of partisanship, or interest in politics." If voting made a person more attached to a political party or interested in politics more generally, then these factors could lead to an increased propensity to vote in future elections.

One testable implication of this hypothesis is that voting should be habit forming only among those individuals whose scores on certain survey measures increased in response to blandishments to vote. For people whose sense of civic obligation or partisanship remained flat, there should be no downstream effect from voting detected. Unfortunately, not one of the few post-election surveys conducted after a voter mobilization experiment has detected a change in psychological profile. This null finding implies that either voting does not meaningfully alter the psychological propensity to vote or that the survey instruments used did not measure the change. In either case, there currently exists no dataset with variance across psychological profiles by which to test this hypothesis.

Third, the act of voting alters "conative attitudes towards voting, that is, positive or negative attitudes about engaging in the act of voting itself. The registered nonvoter may regard going to the polls with a certain amount of apprehensiveness." The examples provided by Gerber, Green, and Shachar are "Will I know how to work the voting machine?" and "Will I know which line to stand in?" The basic idea is that fear of the unknown is a negative attitude associated with voting for neophytes and the act of voting can replace these negative thoughts with experience and positive associations.

A testable implication of this mechanism is that voting should be more habit forming among people who have not voted before. Conversely, people who have voted in the past should have little apprehension about the process and receive little new reassurance from voting in a particular election. Thus, subjects who voted in the election prior to the experiment, E-1, should exhibit less inertia than subjects who abstained in the election prior to the experiment. By splitting the sample by past abstention and voting, evidence for this mechanism can be uncovered.

The final proposed mechanism for voter inertia is a change in self-perception. "Going to the polls confirms and reinforces one's self-image as a civic-minded, politically involved citizen. The more one votes, the more one comes to regard going to the polls as 'What people like me do on election day.'" That is, voting becomes an outward and public expression of a self-conception (and the same holds true for abstention). The vast majority of individuals will not think of themselves consciously or explicitly in terms of voters and non-voters, but voting is one component of how individuals conceive of their relationship with the broader community.

Presumably, a person's self-perception is more malleable earlier in life. Voting in a particular election is more likely to change the self-conception of an 18 year old than of an 80 year old. An 80 year old has a wealth of life experience and one more data point is unlikely to alter a person's Bayesian posterior view of himself. Thus, voting should be more habit forming among younger voters than older voters. To test this hypothesis, the voter mobilization experiments can be partitioned and analyzed by age groups.

Thus, of the four mechanisms for voter mobilization proposed by Gerber, Green, and Shachar, one is a confounding factor rather than a mechanism, another cannot be tested, and the other two can be explored using the data available. The next section describes eight separate experiments that provide evidence of habit formation.

Data

Three types of experiments, differing both in design and in time period, are used to replicate the habit finding. The first set of voter mobilization experiments were conducted during municipal elections in 2001 across six cities (see Green, Gerber, and Nickerson 2003). Households containing registered voters were randomly placed into a treatment group that received a short knock on the door, and a control group that received no attention from the mobilization campaign. In five of the cities, the treatment group voted at a higher rate rather than the control group and are, therefore, suitable candidates for detecting habit formation.[4]

Site 1: *Bridgeport, CT*. Bridgeport is a racially diverse, low-income urban area that votes overwhelmingly Democratic. In 2001, there was very little political activity since the only election was school board and most races were entirely uncontested. As a result, there were few competing political activities and the experimental voter mobilization was not drowned out by other noise. The electoral slate in 2002 was slightly more full and featured an uncompetitive House race (won 64% to 36%) and a gubernatorial election that was never in serious doubt (56% to 44%). As a result, Bridgeport received very little political attention in 2002, and there is little reason to believe that voting in the 2001 school board elections altered the contact subjects in the treatment group had with political parties.

Site 2: Columbus, OH. The areas canvassed in Columbus during the 2001 experiment were near the Ohio State University campus and consisted almost entirely of students living off-campus. Such areas are generally ignored by political parties because of the low rates of turnout and great difficulty in contacting residents. The gubernatorial and US House races were not hot contested and decided by 30 and 20 points respectively. Thus, Columbus is the site least likely to exhibit a correlation between assignment to the treatment group in 2001 and exposure to political campaigns in 2002.

[4] One cannot measure the degree to which the boost in turnout persists in the city that did not experience an increase in turnout, Raleigh, NC. Finding no mobilization effect at time E places a zero in the denominator of the habit estimator.

Site 3: Detroit, MI. Just as in 2001 where Detroit featured a tightly contested mayoral election, 2002 again featured a high profile and close election. While the US Senate and House races were all landslides (both primary and general elections), the election for governor was decided by only 3%. Generally, the closeness of the election would raise suspicion that members of the treatment group who voted in the 2001 mayoral election would be contacted preferentially by the gubernatorial campaigns. However, the Qualified Voter File (QVF) for the state of Michigan does not record turnout in municipal elections. The two vendors that the Michigan Democratic Party primarily relies upon for voter lists utilizes the QVF for information on turnout and does not collect turnout for each city.[5] While Republican voter files may have contained such information, it is extremely unlikely that Dick Posthumus's campaign devoted any voter mobilization efforts in the largely black areas of Detroit. Thus, it is safe to assume that neither party targeted members of the treatment group who voted the prior year.

Sites 4 and 5: Minneapolis and St. Paul, MN. In 2001, both cities featured competitive local elections with moderate rates of turnout. While the gubernatorial and House races were uncompetitive in 2002, the race for the Senate was highly charged and very competitive. Before and after Wellstone's death, both parties were engaging in intensive mobilization of their supporters and reaching out to undecided voters. Since the Secretary of State records individual level turnout for every election held in Minnesota, it is reasonable to assume that both parties knew who voted in 2001 and targeted such people preferentially. The only reason political parties may not have targeted prior voters is that Minnesota allows same day registration and get out the vote efforts may have targeted neighborhoods rather than individuals. It is likely that

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[5] Files from both vendors were obtained for an evaluation of the Michigan Democratic Party's Youth Coordinated Campaign. The files contained lengthy voter history for statewide elections, but no data for off-year municipal elections.

campaigns employed strategies targeting both individuals and neighborhoods. Thus, the habit estimates derived for both Minneapolis and St. Paul should be viewed as upper bounds rather than unbiased estimators.

The second type of voter mobilization experiments differed from those just described in several important ways (see Nickerson 2004). First, the experiments occurred during the 2002 primaries, just two months prior to the general election. Thus, a habit effect would have less time to degrade and one would expect to see larger estimates of turnout inertia. Second, the population targeted was households containing exactly two registered voters. This population is relatively more stable and affluent than households featuring only one or more than three registered voters. Finally, the experiment utilized a recycling drive as a placebo. Households were randomly assigned to receive either a get out the vote appeal (GOTV) or encouragement to recycle. The rate of turnout for the households who received the recycling treatment was compared with turnout among the households receiving the GOTV appeal.[6] Thus, every household in the analysis actually received one of the treatments and non-application is not a source of noise for the analysis. The two placebo controlled experiments were small in scope, but the efficient design ensured a large intent-to-treat effect to be tracked over time.

Site 6: Denver, CO. While neither the Senate nor the House primaries were particularly close, the general election for the US Senate was close, decided by only 5 percentage points (51% to 46%). Since the primary was held August 13, there was plenty of time for the winning campaigns to update their voter history files were they so inclined. Given that voters who participate in the

[6] There was no detectable treatment effect from the recycling placebo. Even if there were a boost in turnout from receiving the recycling message, it would not interfere with the habit analysis since both the treatment and placebo conditions would exhibit voter inertia.

primary almost always vote in the general election, it is not obvious that a campaign would spend extra resources mobilizing primary voters. Similarly, voters participating in a partisan primary are unlikely to vote for the candidate of the opposite party and may not have received special attention from either party prior to the election. However, such strategies by the political parties cannot be ruled out and the Denver habit estimate should be viewed as an upper bound.

Site 7: Minneapolis, MN -- Placebo. Despite taking place within the same city, there is no overlap between subjects in the 2001 and 2002 experiments in Minneapolis. The types of neighborhoods targeted were very different and constitute two disjoint populations. The 2001 primaries were not competitive and occurred late in the season, September 10th. Given the closeness of the general Senatorial race, it is possible that both campaigns obtained and acted upon every available piece of information. Thus, members of the treatment group who voted in the primary may have been targeted by a campaign for persuasion and mobilization and the estimate of inertia should be subjected to scrutiny and taken as an upper bound.

The third type of replication experiment presented here randomized precincts rather than individuals (see Arceneaux 2004). During a 2003 municipal election, ACORN canvassed predominantly black neighborhoods with Kansas City in favor of a sales tax increase, whose proceeds were slated to benefit the public transit system. Twenty-eight precincts were targeted and half of these precincts were randomly selected to receive ACORN canvassing. The four-week canvassing program employed 75 predominantly black canvassers between the ages of 18 and 25. The first three weeks of the campaign focused upon education and persuasion. The final week went through the treatment precincts again encouraging voter turnout.

The precinct-level randomization introduces a wrinkle into the statistical analysis. Individuals living within the precincts are likely to have correlated baseline rates of voter turnout. Standard errors will be artificially small should this correlation not be accounted for. Robust variance estimators can easily account for the intra-cluster correlation (Donner and Klar 2000). The net effect of the robust variance estimators is a reduction in the effective N of the experiment, which reduces statistical power. However, the habit estimates derived from the Kansas City voter mobilization experiment are unbiased.

None of the seven experiments is large by itself, but pooled together will yield an estimate as precise as the large New Haven experiment conducted by Gerber and Green. The major point of concern is whether political campaigns targeted prior voters preferentially created a positive correlation between assignment to the treatment group and election specific causes of voting in the 2002 general election, $\mathrm{cov}(S_{2002}, T) > 0$. So, the specific habit estimates obtained for both Minneapolis experiments, St. Paul, and Denver may be inflated slightly. Thus, if there is a bias to the analysis, it is that it over-estimates the extent to which voting is habit forming. The next section analyzes and discusses the results.

Results

The results of the seven experiments are summarized in Table 1. When examining a secondary, downstream consequence of an experimental intervention, the level of certainty surrounding a derived estimate will always be low. The relatively small scale of each experiment means that no one study is particularly informative. Even in the follow-up to the 1998 voter mobilization experiment, which involved 25,200 subjects, the standard errors were just over 20%. Only by pooling together multiple experiments can gains in precision be made. Thus, columns 10 and 14 are the ones to which the reader should pay the most attention.

In every one of the new experiments, subjects in the treatment group were more likely than the subjects in the control group to vote in the election following the experiment. The difference in turnout ranged from as small as 0.2% in St. Paul to as large as 2.5% in Bridgeport and Denver (row 10). Given that the standard error is larger than the estimate in many cases, the specific numbers should be interpreted cautiously. However, the fact that all seven experiments produce a positive treatment effect in a much later election strongly suggests that habit formation is occurring.

The individual estimates for habit formation (row 11) vary from a low of 5% in St. Paul (column 6) to a high of 64% in Bridgeport (column 2). The variability is hardly surprising since the results depend upon the quotient of the immediate intent-to-treat effect (row 7) and the downstream difference in 2002 (row 10). But once each set of experiments is pooled together, the derived estimates are more precise than those obtained by Gerber, Green and Shachar (2003). Pooling together the results from the eight new habit experiments described above, the independent effect of habit is estimated to be 21% with a standard error of 15% (column 10). That is, all things being equal, a person who votes in the prior election is 31% more likely to vote in the next election.

The results from all known habit formation experiments (i.e., the eight reported here; Gerber, Green, and Shachar 2003; Michelson 2003) for voter turnout are reported in column 14. Using precision weight averages, it is estimated that voting creates a habit effect of 29% (with a standard error of 12%). In other words, the results from the 1998 New Haven experiment may be more than one-third too high. Admittedly, the new estimate of voter inertia is within one standard error of the old estimate of voter inertia, but it appears that voter inertia is less of a factor than previously believed.

The precision of this estimate can be improved by modeling the dichotomous nature of the voting decision at both stages of the process. Once dummy variables are included in the analysis to account for differences in baseline rates of voting in different cities and elections, a pooled two-stage endogenous probit model can estimate voter inertia (see Table 2). The model yields an estimated habit effect of 1.1 probits with a standard error of only 0.3 – well beyond any traditional threshold for statistical significance. That is to say, a person who votes in a given election is one standard deviation more likely to vote in the next election. Figure 1 interprets the estimate graphically. The black line represents a person's baseline propensity for voting had she abstained in the last election. The red line represents the person's estimated probability of voting had she voted in the last election. Voter inertia pushes a person with a 20% likelihood of voting to vote 60% of the time in the next election. Habit compels a person with even odds of voting (50-50) to vote 86% of the time. As a point of comparison, 42% of the people between the ages of 18 and 21 reported voting in the 2000 NES, compared to 75% of those people polled between the ages of 60 and 80. In other words, getting a young person to vote in an election will make her behave like a voter 50 years her senior. Voter inertia is a powerful determinant of turnout.

Pooling together all of these experiments makes it possible to attempt more fine-grained analysis. In particular, one can test the hypothesis that voting is habit forming because experience helps to alleviate anxiety about the process of voting.[7] If anxiety about the process drives voter inertia, then one would expect that people who abstained in the election prior to the experiment (E-1) would exhibit more experimental inertia than those subjects who vote

[7] Gerber, Green, and Shachar (2003) advance the self-confidence hypothesis (p. 548) and even break down their analysis by past voters and abstainers (p. 546, Table 3), but never make the last step and test the hypothesis.

regularly. To test this hypothesis, the sample for each experiment was divided into those who had voted in the election prior to the experiment and those who had abstained in that election. A probit model was then run for both abstainers and voters in each city.[8] Table 3 reports the estimated habit coefficient for each model.

In eight out of the eleven experiments, the abstainers exhibited more voter inertia than people who voted in the previous election (see column 3). If there were no difference between prior abstainers and voters, then the odds of seeing such a pattern due to random chance are 8 in 100. There are two ways to pool across these experiments using precision weighted averages. Pooling across each one of the experiments it is estimated that people who voted in the last election exhibit 2.2 probits less voter inertia than abstainer (s.e. = 0.4). Individuals who have voted before exhibit virtually no habit formation, whereas people who did not vote in the election prior to the experiment develop a massive voting habit. A move of 2.2 probits will turn a 40% voter into a 97% voter. As a point of comparison, the 2000 NES found individuals without a high school diploma reported voting 40% of the time compared to 86% of those with graduate degrees. So, voting in one election will make a neophyte voter behave like a highly educated voter, but it has no effect upon an experienced voter. Thus, it appears that conative attitudes are a major cause of voter inertia.

Not surprisingly, the story is somewhat more mixed with regards to the self-identity hypothesis. In short, I can't find a single robust result. Any age result that is uncovered falls apart when the result is compared across experiments or the age categories are adjusted. How do I present a null finding like this? I have sliced and diced the tables dozens of different ways. The lack of a stable finding indicates that the self-perception hypothesis is unlikely to be true.

[8] The non-linear estimator is essential in this setting. The rates of turnout among abstainers who failed to vote in the experiment are very low and turnout is equally high among individuals who voted in the prior election and the experimental election. Thus, linear estimators are unstable and yield estimate habit coefficients outside [0,1].

Conclusion

The eight experiments presented in this paper constitute strong evidence that voting is not as habit forming as the 47% previously found. A better estimate, based upon more data, is one-third smaller, 29%. Furthermore, this paper has corroborated the finding using a theoretically correct two-stage endogenous probit estimator.

Habit remains an important cause of voter turnout. Twenty-nine percentage points is almost exactly the difference in turnout between households with less than $10,000 in income (53%) and turnout in households with more than $100,000 in income (81%). Age, education and income are well known correlates of voting that have received considerable attention over the years. In contrast, inertia is an equally important cause of voting, but has strangely received less attention.

It appears as if voting is habit forming because people are more comfortable with the process after having successfully accomplished it. Voting was hugely addictive among individuals who had not participated in the election prior to the experiment. In contrast, there was virtually no evidence that past voters exhibited voter inertia. There also appeared to be no relationship between age and habit formation. This null-finding suggests that voting is not habit forming as a result of changes in self-perception.

The strength of the conative attitude finding suggests a shift in emphasis for future research. Earlier it was suggested that surveys could be used in conjunction with voter mobilization experiments to look for changes in attitudes associated with high rates of voting such as interest in politics. Testing this hypothesis remains a worthy goal. However, such a survey also ought to ask questions probing why voting is habit forming among non-voters, but not prior voters. Is it because of an increased familiarity with the process? Carefully worded survey questions might be able to tease out why abstainers and voters behave so differently. The empirical tests in this paper are suggestive, but hardly detailed or conclusive.

Another interesting question deserving further research is whether some types of elections exhibit voter inertia more than others. The United States has a bewildering array of elections for citizens to participate in. Is the magnitude of the inertia the same between primary and general elections, congressional and municipal elections, municipal and Presidential elections? It is possible that getting into the habit of voting in Presidential elections does not translate into getting into the habit of voting in local races.

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Table 1

	2001 General Election					2002 Primary Experiment using Placebo		2003 Municipal	Pooled New Results	G, G, & S (2003)	Michelson (2003)		All Experiments
	Bridgeport	Columbus	Detroit	Minneapolis	St. Paul	Denver	Minneapolis	Kansas City		New Haven	Latino	White	Pooled
Control N	963	1324	2598	919	1104	279	191	4779		10073	297	756	
Control Turnout 2001	9.7%	8.2%	42.7%	26.1%	37.6%	39.1%	16.2%	29.1%		47.6%	13.8%	25.8%	
Treatment N	937	1157	2596	904	1104	283	203	4933		15137	466	1240	
Treatment Turnout 2001	13.6%	9.6%	44.8%	29.1%	42.2%	47.7%	27.1%	33.5%		49.4%	18.5%	28.2%	
Immediate Difference	3.9% (1.5)	1.4% (1.1)	2.2% (1.4)	3.0% (2.1)	4.6% (2.1)	8.6% (4.2)	10.9% (4.1)	4.4% (0.9)		1.8% (0.6)	4.7% (2.8)	2.4% (2.1)	
Control Turnout 2002	21.2%	11.7%	41.6%	12.7%	59.5%	76.3%	32.5%	18.8%		38.8%	37.8%	60.1%	
Treatment Turnout 2002	23.7%	12.1%	42.5%	13.4%	59.7%	78.8%	34.0%	19.1%		39.8%	38.6%	57.5%	
Downstream Difference	2.5% (1.9)	0.4% (1.3)	0.8% (1.4)	0.7% (1.6)	0.2% (2.1)	2.5% (3.5)	1.5% (4.8)	0.2% (0.8)		1.0% (0.6)	0.9% (3.6)	-2.6% (2.3)	
Habit (2SLS)	64.4% (43.0)	27.4% (83.9)	39.1% (52.4)	22.0% (51.6)	5.1% (43.9)	28.4% (37.7)	14.1% (43.6)	5.1% (30.1)	21.2% (15.3)	46.7% (20.2)	19.7% (74.6)	-105.0% (148.8)	29.4% (12.0)
P-Value (1 tailed)	0.07	0.37	0.23	0.33	0.45	0.23	0.37	0.77	0.08	0.02	0.79	0.48	0.01

Numbers in parentheses represent standard errors.
Pooled values were calculated using precision weighted averaging (conditional shrinkage estimation generates a similar result).
* The Gerber, Green, and Shachar experiment involves multiple treatments groups. The differences in turnout simply report the differences between the pure control group and subjects assigned to at least one treatment group. The two-stage least squares analysis takes into account the full complexity of the experimental design.

Table 2 Two-Stage Endogenous Probit Estimate of Habit Formation

Seemingly unrelated bivariate probit — Number of obs = 52234; Wald chi2(23) = 6978.73; Prob > chi2 = 0.0000

Log likelihood = -58590.464

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
votedEp1						
INERTIA	**1.0981**	**.29508**	3.72	0.000	.5197835	1.676483
city1	-.8213772	.0828272	-9.92	0.000	-.9837155	-.6590388
city2	-1.257512	.0888473	-14.15	0.000	-1.431649	-1.083374
city3	-.6070961	.0423505	-14.34	0.000	-.6901015	-.5240907
city4	.5828498	.0703736	8.28	0.000	.4449201	.7207795
city5	-1.285386	.0335622	-38.30	0.000	-1.351167	-1.219606
city6	-1.480812	.0501883	-29.51	0.000	-1.57918	-1.382445
city7	-.3651029	.0814035	-4.49	0.000	-.5246508	-.205555
city8	.0972026	.0548986	1.77	0.077	-.0103967	.2048019
city9	-.5835279	.0837184	-6.97	0.000	-.7476129	-.4194429
city10	-.757511	.0513807	-14.74	0.000	-.8582154	-.6568067
_cons	-.1154247	.1134467	-1.02	0.309	-.3377762	.1069267
votedE						
mailings	.0104537	.006848	1.53	0.127	-.0029682	.0238756
persngrp	.0989905	.0126526	7.82	0.000	.0741919	.123789
city1	-.945867	.0465808	-20.31	0.000	-1.037164	-.8545703
city2	-1.094589	.0446959	-24.49	0.000	-1.182191	-1.006987
city3	.0987446	.0321854	3.07	0.002	.0356624	.1618268
city4	.0901416	.0596318	1.51	0.131	-.0267345	.2070177
city5	-.230262	.0300833	-7.65	0.000	-.2892242	-.1712999
city6	-.3344963	.0413417	-8.09	0.000	-.4155245	-.253468
city7	-.7207361	.0603894	-11.93	0.000	-.8390971	-.6023751
city8	-.3567047	.0404141	-8.83	0.000	-.4359148	-.2774945
city9	-.5115348	.0758195	-6.75	0.000	-.6601383	-.3629313
city10	.2434565	.0292246	8.33	0.000	.1861773	.3007358
_cons	-.3060794	.0277368	-11.04	0.000	-.3604424	-.2517163
/athrho	.1888113	.1712222	1.10	0.270	-.146778	.5244007
rho	.1865992	.1652604			-.145733	.4810893

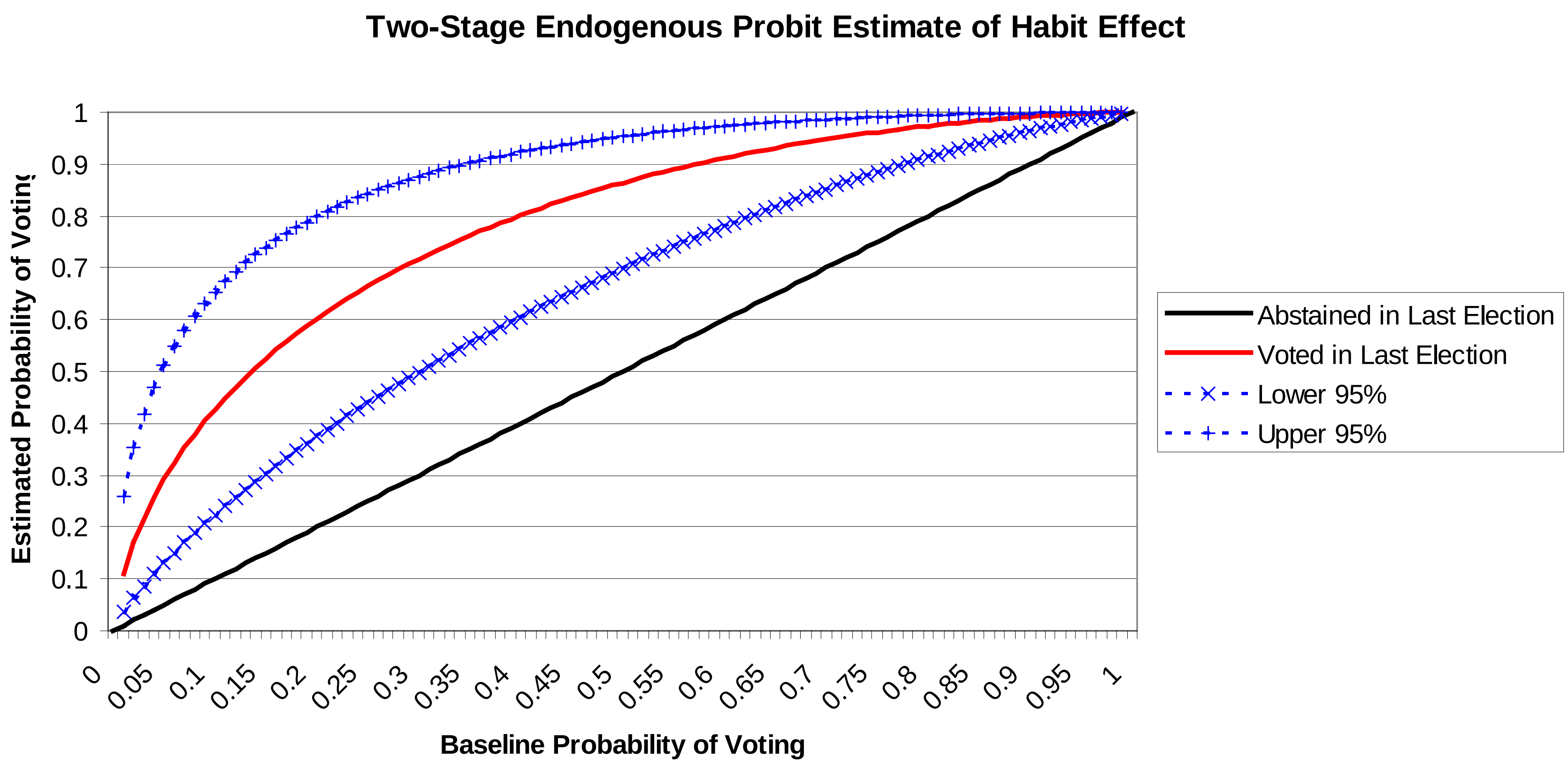
Likelihood ratio test of rho=0: chi2(1) = 1.17292 Prob > chi2 = 0.2788

Table 3 Habit formation by past experience voting

	Habit Effect Abstainers	Habit Effect Voters	Difference
Bridgeport – 2001	3.2 (3.1) 1076	0.6 (1.2) 824	2.6 (3.3)
Columbus – 2001	0.3 (2.7) 1425	0.5 (1.6) 1056	-0.2 (3.1)
Detroit – 2001	2.0 (2.1) 2088	0.7 (3.3) 3106	1.3 (3.9)
Minneapolis – 2001	-0.6 (1.0) 791	0.4 (1.6) 1032	-1.0 (1.9)
St. Paul – 2001	3.6 (0.2) 403	-0.4 (0.8) 1806	4.0 (0.8)
Denver Placebo – 2002	2.5 (0.3) 232	0.2 (1.8) 330	2.3 (1.8)
Minneapolis Placebo – 2002	2.2 (3.4) 165	-0.9 (0.9) 229	3.1 (3.5)
Kansas City Precinct – 2003	-0.0 (1.0) 3433	0.5 (0.7) 6279	-0.5 (1.2)
Dos Palos – Latino	3.3 (6.2) 256	1.4 (1.0) 527	1.9 (1.0)
Dos Palos – White	0.8 (99.9) 411	-0.8 (0.5) 1585	1.6 (100.0)
New Haven – 1998	1.8 (0.6) 18674	-0.6 (0.3) 6526	2.4 (0.7)
Pooled Difference (Casewise)			**2.2** (0.4)

Top number represents the estimated habit coefficient in probits.
Numbers in parentheses represent standard errors.
Bottom numbers are the N.

Figure 1


Two-Stage Endogenous Probit Estimate of Habit Effect
Estimated Probability of Voting
Baseline Probability of Voting
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1
0.05
0.15
0.25
0.35
0.45
0.55
0.65
0.75
0.85
0.95
Abstained in Last Election
Voted in Last Election
Lower 95%
Upper 95%