

THE ROLE OF HABIT IN VOTING:  
MAKING AND BREAKING HABITUAL VOTERS

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## **Abstract**

Political scientists repeatedly find that a person's turnout in past elections strongly predicts her future turnout. Scholars often interpret the association between past and future turnout as evidence of "habitual voting." However, the discipline has no widely accepted conceptual definition or empirical measure of habitual voting, and few systematic investigations of the mechanisms responsible for habitual voting. In this dissertation, I draw on research in psychology to propose a new theoretical framework for understanding turnout focusing on habit. The framework describes the processes that govern the formation of voting habits over time, the defining features of voting habits, and the factors that "make" and "break" voting habits.

I test the propositions in the dissertation using data from an original panel survey of Minnesota registered voters conducted before and after the 2010 election in conjunction with state information on respondents' turnout over a six-year period. The data are first used to develop and validate a direct measure of the psychological features of voting habits. I use the measure to investigate the causal relationships among voting over time, changes in mood, efficacy, and geographic residence, and the strength of voting habits, among other factors. In the final chapters of the dissertation, I examine the different effects of mobilization messages and early voting policies on habitual and non-habitual voters.

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## Chapter 1

### Understanding Voter Turnout

Why do people vote? Political scientists have sought to answer this question for at least a century. A voluminous body of research demonstrates that voting is associated with myriad individual level and contextual factors. Individuals who are more educated, are interested in politics, live closer to a polling place, attend church regularly, and become involved in civic organizations are more likely to go the polls. Citizens who strongly prefer one candidate to another or who perceive a race as close also vote at a higher rate. Members of certain demographic groups are more likely to vote, including whites, the elderly, and individuals with higher incomes. Additionally, several contextual factors, including the strictness of states' voter registration requirements and strength of regional participatory norms, are associated with higher turnout rates.

Although the numerous scholarly findings on turnout contribute enormously to the understanding of why people vote, they are far from satisfying. Reviewing the turnout literature, Harder and Krosnick (2008, 525) conclude that "an individual citizen's turnout behavior is a joint function of his or her social location, his or her psychological dispositions, the procedures involved in voting, and events that occur at the time of each election." The existing understanding of turnout is inadequate in part because scholars have not sufficiently examined interdependence among the factors in Harder and Krosnick's (2008) "function." Researchers do not understand the precise manner by which the numerous determinants of turnout influence each other and turnout. For example, an individual's educational attainment and general interest in politics are both strongly associated with turnout across decades of empirical research. But what are the

specific processes by which educational attainment and political interest shape each other over time to boost the likelihood of voting? Few studies have systematically examined these causal processes (but see Delli Carpini and Keeter 1996).<sup>1</sup>

One empirical finding in the literature is particularly robust. Beginning with the authors of *The American Voter* (Campbell et al. 1960, 92-3), political scientists have repeatedly found that a person's turnout in past elections is strongly related to her future turnout. Voting once increases the likelihood of voting in the next election by as much as 50 percent (Gerber, Green, and Shachar 2003). Recent research suggests that the prior voting effect is significantly less than 50 percent in typical electoral settings (Denny and Doyle 2009; Meredith 2009; Dinas 2012). Nonetheless, among the individual level factors frequently studied in empirical turnout research, voting in previous elections represents one of the strongest predictors, if not the strongest predictor, of future turnout.

Scholars often interpret the influence of past turnout on future turnout as evidence of "habitual voting."<sup>2</sup> Going to the polls might induce psychological changes that perpetuate voting over time, regardless of the election-specific motives. For example, Gerber et al. (2003, 548-9) offer four potential explanations for the prior turnout effect, including three related to psychological processes attributed to habit. Voting might bolster one's self-image as a civic-minded person and subsequently motivate future turnout, among other processes (Gerber et al. 2003, 548). Scholars have drawn on

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<sup>1</sup> Political scientists have also not yet confirmed that education has a causal effect on turnout. As discussed below, researchers using observational and experimental designs have concluded both that educational attainment influences turnout and that it has no causal effect (see Berinsky and Lens 2011; Tenn 2007; Sondheimer and Green 2010; Kam and Palmer 2008).

<sup>2</sup> Numerous terms are used to describe the past turnout effect, including "consuetude" (Green and Shachar 2000), "inertia" (Plutzer 2002), and "over-time persistence" (Meredith 2009) in voting. "Habit" remains the most commonly used term, despite some misgivings about its applicability to voting (see Meredith 2009, 189).

research in psychology to begin further theorizing and testing initial propositions about the processes underlying repeated voting (Aldrich, Montgomery, and Wood 2011; Valentino 2009). However, the discipline still has no widely accepted conceptual definition of “habitual voting,” and few systematic investigations of the mechanisms—psychological or otherwise—responsible for the past turnout effect.

This dissertation clarifies those mechanisms. I draw heavily on research in psychology on habitual behaviors to summarize the processes thought to govern the formation of habits over time. Applying this understanding to voting, I propose a new theoretical framework for understanding turnout focusing on habit. The five key components of the framework are: an individual’s frequency of prior election voting; the setting (e.g., time and place) in which voting occurs; the psychological rewards experienced as a result of voting such as mood changes; how automatically and efficiently an individual can vote; and the centrality of voting to an individual’s identity. The relationships among these factors help to explain over-time persistence in turnout. An important feature of the theoretical framework is that it is dynamic: it clarifies the causal influences among factors at different stages in the voting habit formation process.

The theoretical framework in the dissertation expands the scope of turnout scholarship considerably to include many previously unexamined psychological factors. But the theory also incorporates determinants of turnout traditionally emphasized by political scientists, like campaign contact, strength of preferences for candidates, and interest in politics. These factors should influence voting less and less as voting becomes more habitual (see Aldrich et al. 2011). Although the traditional political science factors influence turnout in many complex ways, I conceptualize many as “intentions” (or

determinants of “intentions”). As habit researchers have long understood, the influence of intentions on future behavior fades as a behavior becomes more habitual.

Given the centrality of habit to understanding turnout, empirical researchers need a valid and reliable measure of the psychological attributes of voting habits. I emphasize in the dissertation that two psychological changes result as a person begins to gradually vote frequently in the same context over time. These are the definitional features of voting habit strength: Election Day actions become more automatic to perform, and voting becomes more central to one’s self-concept. In Chapter 3, I develop a direct measure of these features using survey self-reports. I adapt the measurement strategy used by Verplanken and Orbell (2003), who develop a habit strength index appropriate for daily and weekly activities. The dissertation’s voting habit strength index incorporates new and more discriminating items related to identity as a voter, uses items that reflect electoral settings, and eliminates items that refer explicitly to frequency. I validate the voting habit strength index using data from an original panel survey of Minnesota residents conducted in 2010, in conjunction with state voter file information on respondents’ turnout over a six-year period. I use a confirmatory factor analysis to test propositions about the two-factor structure, and carry out a response time analysis to validate the subscale related to voting automaticity. I establish predictive validity for the index too, finding that it significantly predicts turnout independently of a range of similar constructs.

In Chapter 4, I test the key propositions in the theory of habitual voting. I utilize the original panel survey data that includes a range of election-related and psychological process-related variables. The panel format gives me unique leverage for estimating the

causal effects of voting on changes in habit strength, psychological rewards, and other constructs. I begin by testing key propositions about the relationships between the strength of voting habits, prior voting frequency, and the voting setting (e.g., the place and time of day the person votes). The analysis proceeds by examining the effects of voting on a number of psychological rewards; and the reciprocal effects of the rewards on future voting. The results demonstrate for the first time that voting generates mood-related changes, increases in internal political efficacy, and decreased anxiety associated with Election Day activities. Finally, I examine the hypotheses about how key political factors are moderated by the strength of voting habits.

In the second half of the dissertation, I use the theory of habitual voting to generate propositions about the effects of mobilization messages and electoral policies that should “make” and “break” voting habits. I examine the results of an original randomized experiment in Chapter 5 to assess the effects of “rewards” and “actions-planning” messages on turnout for habitual and non-habitual voters separately. The analysis offers some practically relevant results, including how non-habitual voters can be demobilized by certain messages read several days before an election, and the disconnect between self-reported likelihood of voting and actual voting behavior for non-habitual voters. Chapter 6 presents a research design for a proposed analysis of the effects of early voting policies on individuals with strong and weak voting habits. I offer specific propositions about what “convenience” and “convenience-boosting” electoral policies mean in terms of psychological automaticity and identity as a voter. I suggest that early voting is likely not convenient for the majority of non-habitual voters, and outline specific methods for examining this empirically.

Since habit is a psychological disposition, and has been studied extensively by psychologists, the interdisciplinary reliance throughout the dissertation is appropriate and even necessary. Any study of habitual voting that ignores the research on habit in psychology is likely to generate propositions that do not pass empirical muster, or divert attention to aspects of voting that are not much related to habit. In some sense, the reliance on psychological factors to explain turnout is nothing new in political science. Political scientists have frequently relied on constructs such as political efficacy (internal and external), interest in politics, strength of preferences for a particular candidate, and perceived closeness of the race, among others.

My ultimate goal is still to answer the question at the start of the dissertation, “Why do people vote?” It seeks to do this by instead answering, “Why do people *repeatedly* vote?” The attention to repeated behavior does not mean that the theory and analyses will have no bearing on what makes people vote in the first place. Understanding the factors that propel voting at later stages of the habit formation process provides information about what the non-habitual voters are missing. For example, I will show that for people who have stronger voting habits, voting is more central to their identities. Therefore, to help motivate first-time voters, we might tell them “you are the kind of person who votes.” Rogers, Fox, and Gerber (2012) suggest that self-referent identity labels like this could be effective ways of boosting turnout. They might be even more effective for non-habitual voters. In the original question about why people vote, “people” is not a homogenous category. As I emphasize in this last example, and throughout the dissertation, individuals have varying strengths of voting habits, and the determinants of turnout are often different for those with strong and weak habits.

In the remaining portion of Chapter 1, I review the key theoretical perspectives and empirical findings in prior studies on voter turnout. I then describe their four primary deficiencies, and how understanding the role of habit in voting should overcome those deficiencies.

### **Prior Turnout Research**

Perhaps the most influential theoretical framework for understanding voter turnout originated with Downs (1957). Downs (1957, 48) argued that rational voters seek only to influence the outcome of elections: “[the] vote should be expended as part of a selection process, not as an expression of preference.” He formalized this framework by proposing that a person votes if

$$pB > C,$$

where  $B$  represents the policy benefits of having one’s preferred candidate win,  $p$  is the probability that one’s vote will be pivotal in the election, and  $C$  represents the costs of voting. Of course, the likelihood of being the pivotal/decisive voter in any given election is infinitesimally small, so no one will vote unless the costs are almost nonexistent. Because people do vote in elections, the model is implausible as specified here.

Other scholars sought to rescue the basic rational choice theory of turnout by revising assumptions about  $p$  and  $C$ . Some assumed that the costs of voting are minute, so that almost any non-zero probability of casting the deciding vote results in positive levels of turnout (see Niemi 1976; Aldrich 1993) or that the probability of being the pivotal voter is grossly inflated in the minds of potential voters (see Riker and Ordeshook 1968; Palfrey and Rosenthal 1985). But the most influential update to rational choice

turnout models came when Riker and Ordeshook (1968) added a term for the “psychic benefits” of voting, the  $D$  term, such that a person votes if

$$pB + D > C,$$

where the  $D$  term includes feelings of fulfilling one’s civic duty and other forms of satisfaction derived from voting. And because  $p$  is still essentially 0, the theoretical framework simply predicts that people vote when the psychological benefits ( $D$ ) outweigh the costs ( $C$ ).

For many empirical turnout scholars—including Green and Shapiro (1994) in their widely read critique of rational choice theories of turnout—this theoretical framework offers few, if any, novel insights that hold up in empirical research. Nevertheless, conceptualizing turnout in terms of its costs and psychological benefits is useful for organizing the long list of predictors described in later turnout research.

### *Costs and Benefits*

While the body of empirical findings on turnout is too vast to review here, I highlight several primary findings related to the costs and benefits of voting. Many are incorporated in the theory of habitual voting proposed below. The most obvious costs of voting involve the time and transportation required to get to the polls. People without cars and who live further from their polling place are less likely to vote (Haspel and Knotts 2005; Gimpel and Schuknecht 2003). Similarly, individuals who recently moved residences are far less likely to vote (Squire, Wolfinger, and Glass 1987; Plutzer 2002), and changing polling places decreases the likelihood of voting by about 2% (Brady and McNulty 2011). Additionally, those with less free time during the day vote at a lower rate (Brady, Verba, and Schlozman 1995).

Strict registration requirements in a state are associated with lower turnout rates, presumably because registration requirements make voting more effortful. States that close registration 30 days before an election have much lower turnout rates (Rosenstone and Wolfinger 1978), and states with Election Day Registration (i.e., no closing date) have noticeably higher rates. Election Day Registration is thought to boost turnout in a state by about 5 percent (Highton 2009, 509; Highton and Wolfinger 1998; Knack 2001; Brians and Gofman 2001). Strict registration laws likely depress turnout so much because they make voting a costly two-stage process for non-registered citizens in which the first stage—registering—is effortful and far less gratifying (Timpone 1998; Glass, Squire, and Wolfinger 1984, 52).

Education, political interest, political efficacy, and strong preferences for a candidate can also be considered “costs” of voting. Each takes time and effort to acquire, and is associated with a higher likelihood of voting. While the mechanisms underlying the effects of education on turnout are largely unknown, many propose that education stimulates interest in politics (see the following section), familiarity with voting procedures and races (Rosenstone and Hansen 1995, 14) and strengthens the association between voting and feelings of civic duty (Wolfinger and Rosenstone 1980). Being more interested in politics also dramatically increases the likelihood of voting (see Verba, Schlozman, and Brady 1995; Brady et al. 1995), as does having more political efficacy (greater confidence in the ability to understand and influence the political system) and having a strong preference for one candidate over another.

Regarding the benefits of voting, people might vote to fill a sense of “civic duty,” altruism, or group solidarity. Citizens who think voting is a “civic duty” are more likely

to go to the polls (Blais 2000), as are individuals displaying more altruism in survey responses (Jankowski 2002) and behaviorally in lab settings (Fowler 2006b). And partisans are thought to experience positive emotions from having expressed solidarity with their party (Fiorina 1976). In particular, strong partisans can “bask in the reflected glory” of their candidate’s victory if the candidate wins (see Cialdini et al. 1976). This may explain why strength of party identification is strongly associated with turnout.

Many scholars also argue that voting has social benefits. Rosenstone and Hansen (1993, 137) contend that individuals who have more extensive social ties as a result of gaining life experience and political interest have greater “susceptibility to the social rewards” of political participation. And Rolfe (2012) demonstrates persuasively that the characteristics and structure of individuals’ social networks powerfully influence their voting behavior, further highlighting the social nature of voting. Rolfe argues that individuals in larger social networks, particularly with politically active members, are more likely to be mobilized. Prior research also demonstrates clearly that receiving social pressure to go to the polls boosts turnout. An enormous body of research beginning with Gerber, Green, and Larimer (2008) utilizes randomized field experiments to demonstrate that telling people in mailings, phone calls, and canvassing messages that their turnout is being watched (thereby applying social pressure) significantly boosts turnout.

#### *Prior Research Deficiencies*

Despite the extensive literature on turnout, scholars have yet to sufficiently answer the question, “Why do people vote?” I argue in this section that our limited understanding of turnout results from four deficiencies in existing research. The

deficiencies are primarily due to model misspecification and undertheorizing about the relationships among factors that shape turnout. I describe the consequences of the misspecification issues and underdeveloped theories, and means of addressing them.

First, turnout researchers have produced relatively weak causal inferences about the effects of many voting costs and benefits. Weak inferences about the causes of turnout are often due to researchers failing to control for key confounds in their empirical models, or making specification decisions about control variables that are weakly grounded in theory.<sup>3</sup> Of course, this issue is not unique to voter turnout research. Social science researchers commonly seek to estimate causal effects and fail to do so effectively because of omitted or improperly included controls. But the problem is far more widespread and has more serious consequences in voter turnout research because of the strong associations among the factors included in most models.

Like many political behaviors, voting is shaped by myriad factors that tend to be related to one another. Many of the most powerful individual level psychological predictors of turnout—interest in politics, political efficacy, education, strength of candidate preferences, feelings of voting as a civic duty, political knowledge, perceived closeness of an election—are associated with one another. Individuals who are more interested in politics tend also to feel more politically efficacious, be more educated, have more political knowledge, have stronger preferences about specific candidates, and see

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<sup>3</sup> This problem applies primarily to observational studies, especially those using cross-sectional survey data. Experiments in turnout research typically allow researchers to make stronger causal inferences and avoid bias from ignoring potential confounds. But while randomized experiments are ubiquitous in voter mobilization studies—which I review in Chapter 5—those studies tend to address why, how, and when people vote only indirectly. They are most concerned with techniques for boosting turnout at the margins, such as effective messaging in campaign phone scripts. When voting determinants researchers use experimental designs, they tend to be natural experiments or ones lacking random assignment (e.g., Krasno and Green 2008; Lassen 2005).

voting more as a civic duty. Excluding any of these constructs (or measuring them poorly) in an empirical model of turnout might bias the estimates of the remaining variables' effects. Specifically, the coefficient and standard error estimates for the remaining variables in the model that are correlated both with the excluded variable and turnout will be biased. Excluding key variables can therefore substantially alter the conclusions about many other variables in turnout models.

Examining the relationship between education and turnout reveals the serious consequences of the problem. Early turnout scholars found that education is one of the most important predictors of voting, if not the single most important predictor (see Wolfinger and Rosenstone 1980). However, the observed effects of education on turnout are highly sensitive to (and often disappear upon) including certain controls, like political interest. Brady et al. (1995, 283) find that whether education has an observed effect on turnout in their data depends on whether or not they include a measure of political interest in their model. The authors contend that education does not have a direct causal influence on turnout, and that education influences voting almost exclusively through political interest.

Denny and Doyle (2008) examine the consequences of including a measure of general interest in politics in models of turnout using data from the 1997 British general election. The authors demonstrate (see especially Table 2) that excluding political interest results in the observed effects of education and childhood comprehensive ability growing considerably, the effects of childhood aggressive personality to become statistically significant at conventional levels, and other significant changes. Denny and Doyle conclude that many reciprocal and complex relationships exist among these

factors,<sup>4</sup> and that turnout scholars learn more by studying factors like personality and cognitive style—that may reasonably be considered to be exogenous—than factors like education and political interest—that are endogenous to many other constructs.

Additionally, Milligan, Moretti, and Oreopoulos (2003) find that more years of education is associated with higher turnout rates, but that the effect disappears when examining only registered voters.

If the observed effects of education disappear almost entirely after including a standard “interest in politics” measure—as Brady et al. (1995), Denny and Doyle (2008), and others find—then turnout scholars should be highly concerned about bias resulting from simple specification decisions. The education-turnout connection has been one of the key empirical findings in the literature. Perhaps unsurprisingly, several recent studies using experimental and matching designs find that education has little or no causal effect on turnout (Berinsky and Lenz 2011; Tenn 2007; Kam and Palmer 2008).<sup>5</sup>

To avoid bias stemming from improperly including unneeded control variables in the model, researchers can rely more on randomized experiments, which provide stronger causal inferences. Of course, using experiments does not guarantee ironclad causal inferences. Researchers using experiments may still rely on underdeveloped theories, a problem I discuss in the next subsection. And the random assignment in an experiment

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<sup>4</sup> Specifically, the authors contend that having a more aggressive personality and more education make a person more interested in politics, which subsequently make her more likely to vote.

<sup>5</sup> Existing research is mixed on whether education causally influences turnout. Berinsky and Lenz (2011) exploit a natural experiment using the Vietnam draft, and Tenn (2007) and Kam and Palmer (2008) use matching methods and find that education is likely a proxy for family and personality characteristics that are the actual causal influences on turnout. These studies suggest that acquiring more education only minimally affects the likelihood of voting. Additionally, having more education may simply be a proxy for greater social involvement in groups that tend to have higher turnout, and have no causal effect on turnout (Rolfe 2012). However, Sondheimer and Green (2010) examine the results of three policy experiments that exogenously induced educational attainment and find evidence for a causal effect of education on turnout.

may not be fully random, especially in research on determinants of voting. In recent experiments in the voting determinants literature (e.g., Lassen 2005; Sondheimer and Green 2010; Berinsky and Lenz 2011), the measure of interest is frequently manipulated by government policies, administrative decisions, or non-profits, rather than the researcher. In Lassen's (2005) study on political information, Copenhagen public officials decided which four of the fifteen districts carried out a pilot project (i.e., assigned to treatment) that provided their residents information about an upcoming referendum. Observers might be concerned that the criteria for selecting the four districts in the study was not random—or whether the propensity score matching method used to control for differences between the groups is sufficient to rule out threats to internal validity.

To avoid misspecification bias, turnout researchers can also develop improved measures of the theoretically important constructs. In particular, they can develop measures based on narrower conceptualizations of constructs like political interest. More narrowly constructed measures are less likely to capture effects of others, like education, on turnout. For example, political scientists might abandon the general “interest in politics” variable so frequently used. Political interest could instead be conceptualized as interest in the specific candidates and issues in an upcoming election, or the extent to which a person follows specific political issues in the news (like health care reform). When modeling some forms of political participation, perhaps like the decision to write a letter to a member of Congress or talk to neighbors about politics, a variable based on a narrower conceptualization would be more distinct from the dependent variable.

Researchers can also exclude variables from the model not expected to have a proximate causal effect. The effect of variables like education is relatively distant and mediated by other variables considered more theoretically important by the researcher. Or a variable may be very proximate to the dependent variable, but not useful for the model. Political interest, for example, might mediate the effects of other theoretically important variables on turnout (as Denny and Doyle 2008 find with some childhood personality traits and cognitive abilities). In those situations, the model should be specified with the multiple causal paths and estimated appropriately. Structural equation modeling (SEM) methods are one way of doing this. The researcher might also exclude political interest if she does not expect that it has a causal effect at all.<sup>6</sup>

The most important way for turnout scholars to avoid bias from misspecification is to develop improved theories—especially ones explicating the mediators, moderators, and reciprocal relationships among the many predictors of turnout—and use those theories to carefully inform their model specification decisions. Of course, this is true for many areas of political analysis. But the consequences of bias stemming from model misspecification are particularly large in turnout research, as I argue in the discussions about education and political interest above. I discuss the issue of improved theories in detail next.

The second primary deficiency in turnout research is underdeveloped theories. The current answer to the question, “Why do people vote?” in the existing literature is, “A long list of seemingly disparate factors.” Scholars have identified about 20 to 25

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<sup>6</sup> For example, political interest would not cause turnout if some characteristics (perhaps personality characteristics) cause an individual both to vote at a higher rate and to be more interested in politics. And if those characteristics are not included in the model, but political interest is included, then political interest may appear to cause turnout by virtue of specifying the model with only interest.

factors—like education, political interest, efficacy, sense of civic duty, strict registration laws—that are typically found to be associated with turnout. But they are mostly unsure how these factors relate to each other and to turnout, and the conditions under which they matter, especially over the life cycle. In other words, the literature may be reasonably described as a collection of empirical findings—albeit an impressively large collection—with weak theoretical linkages. Turnout scholars have few mid-level or comprehensive theories with strong empirical support. Fowler, Baker, and Dawes (2008, 234) describe the current approach to studying turnout as the “everything-but-the-kitchen-sink approach.”

For example, turnout scholars relying on cross-sectional survey data typically enter a number of variables into their models, and assume these variables have simple direct effects on turnout. But the actual effects of most factors on turnout are not simple and direct. For example, Rosenstone and Hansen (1995) consider an impressively broad set of factors (far broader than most turnout studies) and uncover many unique findings about the role of elite mobilization in stimulating turnout among the socially connected and resource rich. The study is far-reaching in its scope and impact. However, Rosenstone and Hansen might use additional interaction terms and tests for sensitivity to alternative specifications that include moderators and mediators.

Of course, many formal theories of turnout exist—many that grew out of Downs’ rational calculus of voting. The 1960s, 1970s, and 1980s, saw the development of theories related to the psychic utility of voting (the *D* term) including the minimax regret decision theory (see Ferejohn and Fiorina 1974). But such rational decision-making theories have usually been found empirically wanting; their main contribution is the

virtual tautology that people vote when the benefits outweigh the costs (Green and Shapiro 1994). Scholars in the 2000s developed many more formal theories of turnout such as the group-based ethical voter models (see Feddersen and Sandroni 2002; Feddersen 2004) that appear plausible but have very rarely been empirically tested. Fowler's (2006a) formal behavioral model on the role of habit is a more promising recent model of turnout with propositions that appears to conform to empirical evidence. The propositions have not been empirically tested in detail though.<sup>7</sup>

Empirically minded turnout scholars have generated a handful of mid-level theories including the civic volunteerism model of participation with its focus on resources, especially civic skills (Verba et al. 1995; Brady et al. 1995). The civic volunteerism model is intended to explain political participation more generally, rather than just turnout. Resources-based frameworks like this are also commonly used to explain turnout, and helpful to reference in illustrating the broader point about theoretical clarity.<sup>8</sup> In the model presented by Brady et al. (1995), political participation (including turnout) is a direct function of resources, including civic skills, time, and income. Adults acquire civic skills through nonpolitical institutions like jobs, community organizations, and church experiences; when they are younger, individuals acquire many civic skills through education and schools, including student government (273). As in other resource-based explanations, the authors propose that education strongly influences political participation, and in distinct ways from political interest.

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<sup>7</sup> Fowler's (2006a) model also only seeks to account for over-time persistence in voting and its basic implications for turnout. It does not address the large body of research in psychology on habitual behaviors, or specific ways that social and political determinants of voting interact with turnout.

<sup>8</sup> Most of the deficiencies highlighted in this section on turnout largely apply to theories of participation too.

The Brady et al. (1995) resource model is more comprehensive than most models of participation. It specifies numerous structural relationships and proposed mediators to explain the causal effects of resources. But as with many models of turnout and participation, the underlying mechanisms must be more thoroughly tested. For example, the precise manner by which civic skills cause an individual to vote, donate money, or spend time on political activities are not empirically tested. Similarly, the proposed mechanisms by which education influence participation are not examined, as with virtually all studies of education and participation. A large body of research now demonstrates using experimental, matching, and observational designs that education has little if any causal effect on turnout (see above for further discussion). Although the evidence is still mixed, if the revisionist claims about education are correct then the resource model's key propositions are disconfirmed. The processes underlying a key finding by Brady et al. (1995)—that political interest dominates the effects of most predictors, including civic skills, on voting and spending time on political acts—is unclear too. Despite extensive scholarly attention to understanding political interest (see Prior 2010), political scientists have yet to explain the causal effects of political interest on turnout.

Some reasonably developed mid-level theories of turnout exist. The best example is Rolfe's (2012) recent social theory of turnout, which integrates prior explanations for turnout on social connectedness, education, and resources. Its key propositions appear consistent with empirical evidence—including the evidence that education does not have a causal effect on turnout, but that its association with turnout is a consequence of

social connections among similarly educated individuals. Rolfe's theory will be important to further test in future research.

Empirical findings about turnout should accumulate more effectively if scholars develop more mid-level and comprehensive theories. A common framework for understanding why people vote, even when some propositions require modification, provides a useful starting point for future research and disagreement. In the area of Supreme Court decision-making, scholars have generated numerous such mid-level theories, relying both on formal theory and empirical analysis to generate many thoroughly tested propositions.<sup>9</sup> Some of the empirical findings are debated of course, but the more comprehensive theories appear to have produced reasonable accumulation of knowledge on important topics.

The third deficiency in turnout research—also related to specification and developing improved theories—is that it neglects potential psychological mediators and moderators. Harder and Krosnick's (1998) coda following their review of turnout research calls for psychologists to study turnout more. The authors reason that, "All of the factors that influence turnout presumably do so via psychologically mediated processes" (542). Voting is clearly an optimal domain for study by political psychologists. In other domains of political science research—including studies of lobbyists' influence on congressional voting, or the relationships between democratic governments and capital markets—psychological processes matter less.<sup>10</sup> In turnout

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<sup>9</sup> Examples include the dynamic models of justices' interactive decision-making and opinion-writing (e.g., Maltzman, Spriggs, and Wahlbeck 2000) and research on the ideological basis of justices' decision-making (the attitudinal model; e.g., Segal and Spaeth 2002).

<sup>10</sup> Psychological factors may of course be proximate causes of political outcomes in those domains too. For example, members of Congress (MCs) and their staffs trust certain lobbyists and organized interests more, and decide whether to rely on lobbyist-provided information to

research, psychological processes are often the most significant (and not just the most proximate) causes of turnout for ordinary citizens. These include: feelings of civic duty, strength of candidate preferences, sense of political efficacy, acceptance of social norms for voting, political interest, and perceived closeness of the election. Many other factors—like education,<sup>11</sup> age, race, voter registration requirements, and campaign contact—do not appear to have a direct causal effect on turnout.

The final deficiency in prior turnout research is that a large piece of the turnout puzzle is missing. Scholars have a far-from-complete understanding of why people vote. Even the most extensive individual level models of turnout estimated with survey data typically have R-squared values between .05 and .25 (or equivalently large values for other goodness-of-fit statistics). Consider Rosenstone and Hansen's (1995, Appendix D-1) model for turnout using American National Election Survey data on presidential elections from 1956-1988. It is considered one of the most comprehensive turnout models, and sometimes utilized as a baseline for comparing new turnout models (e.g.,

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different degrees when casting votes. MCs' trust in lobbyists—a psychological construct—should moderate the effects of lobbyists' information-giving on MCs' roll call voting.

But although the psychological variable in this example—lobbyist trust—is a proximate influence on voting, its variation appears relatively inconsequential for understanding variation in roll call voting. Whether a lobbyist provides information in the first place surely matters more. (But see La Pira (2008) for experimental evidence that congressional staff members' perceptions of bias in lobbying messages influences their judgments about the information contained in those messages.) And organized interests and lobbyists are known to give resources and information to MCs who are already more likely to vote for them.

<sup>11</sup> For example, the reasons for the observed association between educational attainment and turnout in most studies are unclear. Aside from the issue of education's causal effect on turnout, we do not understand *why* educational attainment would influence voting (for a discussion see Sondheimer and Green 2010; Berinsky and Lenz 2011). Education might have a causal influence on turnout, but that influence could be mediated by psychological constructs. Education may strengthen individuals' acceptance of voting as a civic duty, stimulate interest in politics, or boost feelings of efficacy—which could make voting more likely. Or acquiring education may strengthen cognitive skills that make it easier for individuals to register to vote, select candidates, and find one's polling place. If education has a causal effect, the most proximate cause is surely psychological.

Aldrich et al. 2011). With 30 variables, the model has a likelihood ratio index (or McFadden's R-squared) of just .23. Given the large number of variables, and that the McFadden's R-squared increases for every new variable, the *adjusted* McFadden's R-squared would be much lower than .23.<sup>12</sup> The Verba et al. (1995) turnout model has an R-squared value of .23.<sup>13</sup> Although R-squared values should be interpreted cautiously—and pseudo R-squared values should not be immediately compared to those from OLS regression models—it is clear we can do much better, especially given the practical and theoretical importance of understanding turnout. And it is clear that scholars need more comprehensive and fully developed theories of turnout that explicitly incorporate over-time psychological mediators and moderators.

#### *Over-Time Persistence in Voting*

Political scientists became more interested in the role of habit in voting after discovering empirical evidence for significant over-time persistence in voting. Most turnout research relies heavily on static, time-invariant explanations. Scholars typically assume that the decision to vote is made separately in each election. They rarely assume that an individual's future likelihood of voting is influenced by her past voting behavior.

But since the 2000s, many studies have found strong evidence of over-time persistence in voting (Gerber et al. 2003; Davenport et al. 2010; Meredith 2009; Denny and Doyle 2009; Melton 2011). These studies demonstrate that voting is self-reinforcing.

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<sup>12</sup> See Long and Freese (2001, 38). McFadden's R-squared values are the pseudo R-squared values reported by Stata with its probit, logit, and logistic commands. Higher values indicate models with a better fit to the data.

<sup>13</sup> The dependent variable in the authors' model is actually a sum of two survey items: self-reported turnout in the 1988 election and responses about how often respondents voted in presidential elections over the lifetime. Because the variable measures a broader construct than just turnout, I suspect the R-squared value is higher than for a turnout-only model. Additionally, models of self-reported turnout typically have inflated R-squared values relative to models of validated turnout.

Voting once makes one significantly more likely to vote again, all else being equal. Most convincingly, Gerber et al. (2003) use an instrumental variables approach with the random assignment to turnout-enhancing treatments as the instrument to estimate the causal effect of past voting on future voting. Individuals who received the mobilization treatment were more likely to vote and, the authors find, were then significantly more likely to vote in the next election relative to the control. Gerber et al. (2003) conclude that voting once makes a person about 50 percent more likely to vote in the next election, *ceteris paribus*. Davenport et al. (2010) also leverage randomization in voter mobilization experiments to highlight significant over-time persistence in voting. The empirical research on persistence in voting suggests that voting in the prior election is the single largest predictor of future turnout.

While some call the evidence of over-time dependence in voting decisions “habitual voting,” scholars have a poor understanding of what voting habits are and how they are formed and sustained over time. Over-time turnout dependence is labeled “habitual voting” because the phenomenon fits with common conceptions of habits in daily life. Some individuals appear to vote in every election just as they brush teeth everyday. But political scientists have no shared definition of “habit” or “habitual voting.” And they do not know why voting habits develop, and for whom they develop. Understanding the concept of habit, why habits form and are sustained over time, and an empirical measure of them for voting are important to determine if over-time persistence in voting is actually the result of habit and not other factors. Moreover, political scientists have no theory of turnout that explicitly incorporates the role of habit.<sup>14</sup> It will

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<sup>14</sup> Fowler’s (2006a) formal model of habitual voting exists, but is insufficient to explain any of the mechanisms underlying habit formation.

be important to clarify the theoretical relationships between habit, the variables responsible for the formation of voting habits, and other determinants of voting typically emphasized by political scientists, like political efficacy, campaign contact, and interest in the candidates. These are my goals in the next chapter.

## Chapter 2

### A Theory of Habitual Voting

According to Collins, Kumar, and Bendor (2009, 457-8), the assumption that voting is self-perpetuating is “a necessary feature of any model of turnout.” The authors contend that formal models cannot generate accurate predictions about aggregate turnout without explicitly considering over-time persistence in voting. That conclusion may also apply to less formalized models of turnout. Most existing empirical investigations of turnout ignore the self-reinforcing features of voting, in part because of their common reliance on cross-sectional survey data. If Collins et al. (2009) are correct, empirical turnout scholars have much to gain by identifying the mechanisms responsible for over-time persistence in voting and incorporating them into future studies of turnout.

To begin evaluating whether empirical turnout studies must consider the self-reinforcing features of voting, consider the following scenario. Imagine that voting is self-reinforcing because individuals experience psychological changes as a result of voting more frequently: they develop a stronger sense of identity as a voter and the actions in voting become easier and more effortless to carry out. Consider too that frequently voting individuals experience small but noticeable increases in efficacy and contentment from their now-familiar trip to the polls. Individuals may skip an election or two, particularly after moving residences, but soon get back to voting in general elections and the occasional primary. Consider that, unlike the first few elections they were eligible to vote in, individuals now feel quite uncomfortable if they stay home on Election Day. In that case they would need to convince themselves that they were “just too busy.” Let us further assume that when voting is something individuals typically do

(and do with hardly any effort), their propensity to vote is less influenced by the pieces of campaign mail, political ads on TV, stories in the news, and feelings about the candidates.

To determine whether Collins et al. (2009) are right in calling for turnout scholars to account for the self-reinforcing features of voting, replace the word “self-reinforcing” with “habitual”—and assume that the scenario above reflects experiences of the average voting-eligible citizen. The scenario implies that habitual voters have different psychological associations with voting than individuals with weak voting habits. The psychological determinants of turnout are different. Existing turnout studies that treat habitual and non-habitual voters similarly will therefore make biased inferences about why citizens vote.

If the scenario is accurate, then habitual voters are also less influenced by the usual determinants of turnout than non-habitual voters. Aldrich et al. (2011) clarified this important source of heterogeneity and suggested that factors like interest in politics and candidate preferences shape turnout more for non-habitual voters. In that case, failing to account for the interactive effect of habit and other determinants on turnout would yield biased inferences. Turnout scholars should thus not only control for individuals’ strength of voting habits in empirical analyses of turnout, but also model the moderating and mediating influences of voting habits. The moderation effects identified by Aldrich et al. (2011) are just a starting point in clarifying the over-time causal effects on voting.

In this chapter, I first describe the scholarly trends in research on habit leading up to the most recent studies in social psychology. I then draw on psychology research to clarify the specific processes underlying the influence of past voting on future voting. I

provide a conceptual definition of habit and summarize the components of the habit formation process. In the second half of the chapter, I apply the understanding of habits in psychology to voting. I offer a new theoretical framework for understanding turnout focused on the habit formation process that incorporates the voting determinants previously emphasized by political scientists.

### **Psychology Research on Habit**

Psychologists have long acknowledged the powerful role of habit in shaping behavior. Almost 125 years ago, William James (1890) wrote an entire chapter of his well-known book *Principles of Psychology* on habit. James argued that much of humans' and animals' lives are guided by habit: "When we look at living creatures from an outward point of view, one of the first things that strike us is that they are bundles of habits" (104). Habits are guided by unconscious, automatic planning, he claimed. James (112-26) developed many prescient propositions about the human brain, its plasticity, and the role of habit: he surmised that repeated behavior alters structures of the brain, making the subsequent "tendency to act" more automatic, thus establishing habits.<sup>1</sup>

Behaviorist scholars throughout much of the 20<sup>th</sup> century emphasized that behavior is largely the result of habit. In this perspective, habits are the learned associations resulting from pairing a stimulus and response over time (Hull 1943). In the theory of interpersonal behavior (Triandis 1977), behavior is largely a function of habit and intention. Typically measuring habit with an indicator of frequency of prior behavior, research on the theory of interpersonal behavior often found that habit is the

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<sup>1</sup> James' propositions about brain plasticity (a term he used well before it became popular in the 20<sup>th</sup> and 21<sup>st</sup> centuries) and changes resulting from repeated behavior appear to accurately foreshadow research on how habits are encoded in procedural memory as a result of learning (see Eichenbaum and Cohen 2004, 435).

best predictor of future behavior. Despite evidence for a strong past-behavior-future-behavior association, many studies on the theory of interpersonal behavior nevertheless focused on intentions and predictors of intentions.

Later, in the 1980s and 1990s, social psychologists devoted considerable attention to understanding automaticity and its relationship with behavior. The early automaticity research did not generally seek to explain habit formation; but it directly informed the psychology work on habitual behaviors that developed soon after. Bargh and colleagues defined the characteristics of “automatic” behaviors, and demonstrated how automatically activated attitudes and stereotypes influence behavior (Bargh 1994; Bargh, Chen, and Burrows 1996).

Psychologists in the 1990s advanced the understanding of habit significantly. Attention shifted away from the issue of how strong past behavior predicted future behavior, and onto the mechanisms responsible for it. In particular, psychologists sought to clarify the processes underlying the formation of habitual behaviors. Drawing on a range of theories and evidence in the discipline, Ouellette and Wood (1998) describe a habit as a specific form of automaticity: context-linked automaticity (see also Verplanken and Aarts 1999; Verplanken, Aarts, and van Kippenberg 1997; Aarts and Dijksterhuis 2000). In other words, repeating a behavior over time in a stable context (i.e., a consistent time, place, or setting) causes the behavior to become gradually associated in memory with the context. Over time, the behavior is carried out more automatically upon encountering the context. Ouellette and Wood’s (1998) experiments and meta-analysis of prior studies on behavioral predictors provide early evidence that behaviors are guided less by conscious intentions as they become habitual.

From a slightly different perspective on habit developed at the same time, researchers explained habit as a form of goal-directed automaticity. For Verplanken and Aarts (1999), habitual behaviors are still automatic behaviors cued by specific situational contexts. But they additionally emphasize the importance of goals in triggering habitual behaviors. Aarts and Dijksterhuis (2000) provide early evidence for ideas about goals prompting automatic behavior after the behavior becomes habitual.<sup>2</sup> And Bargh (1994) defined “automatic” behaviors as ones performed more efficiently and guided less by conscious intentions, control, and awareness.

Much of the current understanding of habit formation in social psychology derives from these early accounts, particularly the slightly more narrow Ouellette and Wood account. But recently, the role of “psychological rewards” has become more prominent in the literature. Habits form initially because they are functional in attaining goals: people repeat behaviors in order to experience psychological rewards (see the subsection below for details). Additionally, researchers now routinely incorporate identity-related explanations into the understanding of habit (Verplanken and Orbell 2003). Particularly for behaviors with fewer opportunities for repetition, like blood donation, identity is about as important as automaticity in explaining habits (Masser, White, Hyde, and Terry 2008).

In the next subsection I explain the habit formation process in detail, the research supporting these explanations, and how it applies to voting. But it is useful to first situate the early habit research in a broader scholarly context. Ouellette and Wood (1998)—in conjunction with researchers on habits as automatic goal-directed behavior at the time

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<sup>2</sup> Aarts and Dijksterhuis (2000) find that when people are asked to think about (the goal of) traveling across town, bike riding thoughts become quickly accessible for people who are habitual bike riders but not for non-habitual bike riders.

(e.g., Aarts and Dijksterhuis 2000; Verplanken and Aarts 1999)—made several essential contributions. The contributions are useful for voter turnout researchers to consider, given the lack of systematic study of habitual voting.

First, the early habit research continued the shift in social psychology from explaining behavior largely by referencing conscious intentions to more strongly emphasizing automaticity. Prior studies on behavioral predictors were dominated by the theory of reasoned action (Fishbein and Ajzen 1975) and theory of planned behavior (Ajzen 1985), which emphasized the role of attitudes and behavioral intentions in predicting behaviors. Research on theories of reasoned action and planned behavior equated “past behavior” and “habit,” and tended to treat habit simply as a variable to control for, rather than a construct of interest in its own right. Habit was seen as an “empty construct,” according to Verplanken and Aarts (1999). Ouellette and Wood (1998, 54) write: “Early in their careers, most psychology graduate students learn that frequency of past behavior, a standard indicator of habit strength (Triandis, 1977, 1980), is the best predictor of future behavior. This truism is typically paired, however, with the caveat that the past-behavior-future-behavior relationship is not especially meaningful.”

Wood and colleagues demonstrated how understanding habit is meaningful. They clarified why frequency of past behavior predicts future behavior so well, and the *conditions under which* it does (Wood and Neal 2007; Wood, Tam, and Witt 2005; Wood, Kashy, and Quinn 2002): conscious intentions predict future behavior less and less as the behavior is repeated over time, especially in a stable setting. And habitual behaviors are carried out more automatically, as scholars emphasize. And habits can be broken simply by disrupting the performance context. These conclusions directly called

into question previous psychology findings about the weighty role of intentions in predicting behavior.

Research on habit then surged in the 2000s—in the areas of social psychology, consumer behavior, health behavior, blood donation, travel choices, and numerous others. Many applied the understanding of habit to explain practical interventions that establish and disrupt habits, with titles like “Interventions to Create and Break Habits” (Verplanken and Wood 2006), and “The Habitual Consumer” (Wood and Neal 2009). Research had implications for ways to increase recycling (Holland, Aarts, and Langendam 2006), decrease binge-drinking (Gardner et al. 2012; Orbell and Verplanken 2010), promote flossing (Orbell and Verplanken 2010), and help people lose weight (Lally, Wardle, and Gardner 2011).

In many ways, the late-1990s epistemological stance on habit in social psychology is the one currently adopted by political scientists. Political scientists often acknowledge that prior voting frequency is the strongest predictor of future voting. And some have hypothesized about the mechanisms underlying the strong causal effect of prior voting (e.g., Gerber et al. 2003, 548-9). But turnout scholars have not drawn extensively on psychology research to understand the role of habit in voting, developed theories of habitual voting, and have not tested them. Turnout scholars still tend to explain voting with conscious intention-like variables, including the strength of candidate preferences, interest in politics, political knowledge, and perceived closeness of the election, to name a few. Scholars typically describe turnout as “the decision to vote.” This phrase presumes that individuals develop a conscious behavioral intention to vote (a “decision”). It ignores that for individuals who repeatedly vote in the same place,

intentions matter much less for whether they vote or not. All indications are that political scientists consider “habit” to be an empty and meaningless construct, much as psychologists in the 1970s, 1980s, and early 1990s did.

This is not to say that understanding habit will be the same boon to political science as it was to social psychology. It surely will not. But undertaking a systematic study of the mechanisms underlying the largest predictor of turnout (habit) can only help. This is my goal in the dissertation. As I emphasize elsewhere, I will not equate voting and flossing, or voting and riding a bicycle. For activities with fewer opportunities for repetition like voting, intentions always influence behavior to some degree. And behavior can reach only a moderate level of automaticity relative to flossing or bike riding. But enormous variation exists among voters in their level of voting automaticity and centrality of voting to their identities. Like other activities with fewer opportunities for behavioral repetition, the role of identity is particularly important in maintaining behavior over time, as I will argue. Understanding the major components of habit strength—automaticity and identity, contextual stability, psychological rewards, and behavioral repetition—offer additional explanatory value and theoretical clarity in the study of turnout.

#### *Conceptual Definition of Habit*

A broad conceptual definition of a habit is “a psychological disposition to repeat past behavior” (Neal, Wood, Labrecque, and Lally 2011, 492). This definition indicates that habits are not themselves frequently performed behaviors. Although a habit is formed gradually as a behavior is repeated over time in a specific context (a process I

discuss below), the repetition is not the habit.<sup>3</sup> Instead, the psychological disposition is the habit. Before explaining the habit formation process, the relevant literature, and how it applies to voting, I address two inaccurate colloquial meanings of “habit.” Both meanings are at odds with the habit formation process described in research.

The term “habit” is sometimes equated with “addiction,” or the related term, “physiological dependence.” However, “addiction” typically describes repeated behavior, including drug use, that produces an extreme short-term reward and harmful long-term effects (Webster’s dictionary definition). Harmful effects might include physical dependence, whereby one’s body reacts negatively without the rewards. Behaviors other than drug use, like gambling or shopping, can also become “addictive.” However, habits are a broader class of behaviors than addictions. Most addictions are probably habits, but the vast majority of habits are not addictions. Numerous daily, weekly, and annual activities can easily become habitual without physiological dependence or harmful long-term effects.<sup>4</sup> In fact, about 45% of people’s behaviors in an average day are considered habitual (Wood, Quinn, and Kashy 2002). Habits also differ from addictions because the psychological rewards are far more extreme for addictions.<sup>5</sup>

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<sup>3</sup> In Chapter 3 I discuss the conceptual differences between behavioral frequency and habit, and problems associated with measuring habit using indicators of behavioral frequency (see also Verplanken 2006; Gardner 2012; Verplanken and Orbell 2003).

<sup>4</sup> Presumably all habits are associated with some low-intensity psychological dependence. Habitual blood donors, cyclists, and recyclers should imagine some discomfort at the idea of stopping the activities, and seek to continue them even in the face of some negative consequences. But the habitual recycler or cyclist would likely give up recycling or cycling more quickly than an individual addicted to a drug like nicotine or heroin would quit “cold turkey.”

<sup>5</sup> Blood donation, cycling, and recycling—common activities that can become habitual—almost never generate the euphoria necessary to become addictions. Nevertheless, the experience of psychological rewards, however small, are still an integral part of understanding habit formation, including voting habit formation. I discuss these below.

The term “habit” is also sometimes equated with “routine,” especially “mindless routine.” These terms are closer to the understanding of habit in psychology research. But “mindless routine” implies a set of behaviors entirely disconnected from an individual’s conscious intentions. Researchers agree that behaviors are less influenced by conscious intentions as the behavior becomes more habitual. But significant variation exists among types of behaviors. As the earliest findings on habit demonstrate (Ouellette and Wood 1998) conscious intentions shape future behavior more for behaviors that can only be performed annually or biannually, like voting and blood donation. Differences in effects of intentions still exist between habitual and non-habitual voters. But the difference between the average voter and average TV-watcher is greater. In other words, individuals with the weakest voting habits should still be influenced by conscious intentions more than individuals with the strongest TV-watching habits.

#### *Explaining the Habit Formation Process*

The habit formation process has five components in my view of the research: behavioral repetition, automaticity in behavioral performance, centrality of the action to one’s identity, stable performance context, and the experience of psychological rewards. How are these components related? How exactly are habits formed? A behavior becomes habitual as it is repeated over time in a specific context, gradually strengthening the association in memory between the behavior and context (Wood and Neal 2007, 2009; Neal, Wood, Labrecque, and Lally 2011). As behaviors become habitual, they are carried out more automatically, especially when the performance context is encountered (Wood and Neal 2007; Verplanken 2006; Neal, Wood, Labrecque, and Lally 2011) and they become more central to one’s identity (Verplanken and Orbell 2003). To

summarize, a habit is a psychological disposition with two defining features: (1) context-specific automaticity<sup>6</sup> and (2) a sense of personal identity associated with the action in memory.<sup>7</sup>

Researchers generally agree that habits initially begin to form because the behavior produces psychological rewards; habits are functional in pursuing goals (Wood and Neal 2007, 843-53; Lally et al. 2010; Neal, Wood, Wu, and Kurlander 2011). There is also reasonable consensus that psychological rewards perpetuate actions most in the early stages of habit formation (Wood and Neal 2007, 843-53). Rewards are less powerful in causing behavior as the behavior becomes habitual. Over time, after repeated experiences of reward and action, the psychological rewards also come to be experienced less intensely (Wood and Neal 2007).

Social psychology and neuroscience researchers assign different degrees of importance to the role of psychological rewards in establishing habits. Neuroscientists studying animal learning and habit (e.g., Dickinson 1985; Balleine and Dickinson 1998) are highly concerned with the process by habits initially take hold. Variation in the experience of rewards is thought to be highly consequential for the behavior. For many neuroscience and animal learning researchers, habit formation is conceived of as a form

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<sup>6</sup> Recall that “automatic” behaviors are performed more efficiently and with less conscious awareness, control, and intentionality (Bargh 1994).

<sup>7</sup> See the identity section below for a description of the debate among habit researchers about the role of identity in habit formation. In short, there is no strong consensus, and empirical evidence is mixed. This is one of the more contested components of the research on habit. But the contestation may be the result of heterogeneity among types of behaviors in the extent to which identity-relevance is a part of habits. Some scholars may study behaviors with more identity-relevance. I will argue that daily and weekly behaviors do not become as relevant to the self-concept as they become habitual, relative to annual and biannual activities. As Sniehotta and Pesseau (2012) state, “One would be hard-pressed to suggest that habitual eating of potato chips is part of one’s self-identity.” But the identity component should be important for understanding habit formation among behaviors with less opportunity for repetition like voting and blood donation.

of stimulus-response learning, the central idea for which originates with Thorndike's (1911) "law of effect." The "law of effect" states that "responses that produce a satisfying effect in a particular situation become more likely to occur again in that situation, and responses that produce a discomforting effect become less likely to occur again in that situation" (Gray 2011, 109).

For habit researchers in social psychology, experiencing psychological rewards is less important in explaining behavioral outcomes in general. For the social psychologists with the rigid-context-cueing perspective of habit (e.g., Wood, Neal, Quinn), the process by which habits are first promoted is often taken as given and generally ignored in favor of explaining behavior that occurs *after* habits are formed. However, understanding why an individual initially begins to repeat a behavior is important—especially when designing policy interventions to create habits. The social psychology and animal learning perspectives also differ in the extent to which changes in rewards are associated with changes in behavior once the behavior becomes habitual.<sup>8</sup>

More theorizing and empirical analysis on the role of psychological rewards in the habit formation process should be conducted. The animal learning researchers have well developed models and generated an impressive set of findings on the topic. But these should applied to humans and voting carefully, since the research is most often conducted with rats. It is likely that the rewards used with rats (more related to food, social interactions with other rats, sleep) are not always analogous to the complex psychological

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<sup>8</sup> Wood and Neal (2007, 2009) contend that once a person's habit is strong, features of the context primarily cue the habitual response; rewards play a minimal role for the person. However, Dickinson (1985) and Balleine and Dickinson (1998) demonstrate empirically that goal-directed behavior is possible once habits are established, and outline conditions under which rewards can be devalued and lead to behavior change even when habits are established. In general, behavior is more sensitive to the experience of rewards in the animal learning perspective.

rewards related to voting specified in this chapter. Humans also presumably have more self-control and ignore short-term reward risks when faced with long-term rewards.

Conversely, we should also not infer too much about psychological rewards from the social psychology research on habit. The role of rewards is not heavily emphasized in theoretical perspectives in this research, and rewards are never measured directly. And since the theorizing is based more on daily and weekly habits, the understanding might not apply as readily to voting. As I emphasize elsewhere, the variation in habit formation processes by type of behavior is likely sizeable; so the specific weight assigned to each component and mechanism of habit formation is likely to vary. Lally et al. (2010) demonstrate empirically that habits vary (somewhat) across types of behaviors; automaticity grows more slowly for some behaviors (like doing situps in the morning, or eating fruit with lunch) than others (walking for 10 minutes after breakfast). The five components of the habit formation process are summarized in Table 1.

**Table 1. Components of the Habit Formation Process**

	<b>Component</b>	<b>Role</b>
1.	Psychological rewards	Incentivize behavior
2.	Behavioral repetition	Establish habit
3.	Stable context	Establish habit
4.	Automaticity	Definitional feature of habit
5.	Identity relevance	Definitional feature of habit

Consider the first time voting on Election Day. A person initially will not know exactly where to go, what to bring, what to expect upon arriving, how much time to

allow, and what specific steps will be required. The actions will not be performed efficiently or smoothly; they will need to be guided by more conscious control, awareness, and intentions. Upon voting, the first-time voter will experience certain rewards<sup>9</sup>: feeling good and more politically efficacious, and having less anxiety associated with Election Day acts. As she votes more often, especially at the same precinct location (i.e., physical context), the voting-related acts will become more automatic (performed efficiently without thinking much, and with less guidance by conscious control, awareness, and intentions). She will also begin to think of herself more as a person who votes, and that aspect of her identity will become more relevant than it had been. If she moves to a different state or residence (i.e., if she experiences a disruption in her voting context), then voting will not be as automatic and (to a lesser extent) voting may feel less relevant to her identity.

In this example, the person's voting "habit" is a combination of (1) context-specific automaticity (i.e., ability to get to the polls, cast a ballot, and fit the acts into one's routine efficiently and with little conscious, controlled guidance and intentions) and the (2) personal identity as a voter. Researchers could measure her voting habit strength using questions or instruments designed to assess the strength of these two constructs for her. See Chapter 3 for a further discussion of the measurement process.

Many types of behaviors can acquire the psychological dispositions indicative of habits. Donating blood, going to church, recycling, smoking cigarettes, riding a bicycle, and attending sporting events might also become habits as they are repeated in a stable

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<sup>9</sup> The specific rewards I describe here (on mood, efficacy, etc.) are not derived from a single list in social psychology or political science. I surmise that these are the important rewards incentivizing behavior, based on individual studies in both disciplines. I discuss these in the following subsection.

context. Gardner, de Bruijn, and Lally (2011) systematically review studies on nutrition, physical activity, and travel choices that utilize the Verplanken and Orbell (2003) habit index. The index measures the level of automaticity (and identity relevance with one item) for the actions. Gardner et al. (2011) find that intentions predict future behavior much more for individuals with weak nutrition and physical exercise habits, in all but one study reviewed. This supports the notion that intentions diminish in importance as behaviors become habitual. The studies Gardner et al. (2011) reviews also report correlations between habit strength and future behavior, which were found to be medium-to-strong (average  $r=.44$ ), confirming that a habit strength index measuring automaticity and identity tends to be associated with future behavior, as expected. In the next section, I discuss the specific implications of each component of habit formation for voting.

## **Application to Voting**

### *Behavioral Repetition*

While different researchers assign importance to slightly different aspects of the habit formation process, they agree on one necessary condition for an action to become habitual: it must be repeated over time. Behavioral repetition in a stable context causes the action to become habitual. The implications for voting are relatively straightforward. The more times one votes (in the same context), the stronger one's voting habit will become. This also means that a person cannot be a habitual voter without ever having voted, or after having voted only once. Some individuals might require more trips to the polls to develop a strong habit, and a few have only moderately strong habits even after voting many times.

## *Psychological Rewards*

For a person to vote, voting must serve a purpose. It must generate psychological rewards. Psychological rewards are thought to be responsible for perpetuating all actions in the early stages of habit formation (Wood and Neal 2007, 843-53). Their importance in causing behavior is thought to diminish as the behavior becomes habitual.

Every action has its own unique set of psychological rewards, or “satisfying effects,” the term Thorndike (1911) uses. I have so far not discussed the specific rewards experienced as a result of voting. This is in part because social psychology research on what types of rewards exist, and how we should conceptualize and measure them, is relatively rare. The research may provide additional theoretical clarification soon. But for now I review the existing research and offer specific propositions about the psychological rewards that would most likely be experienced as a result of going to the polls. The list is not meant to be exhaustive.

For donating blood, the rewards may include a greater sense of altruism and efficacy. For riding a roller coaster, rewards may include excitement combined with contentment following the ride. For voting, I propose that the primary psychological rewards of voting are (1) a more *positive mood*, (2) more *energetic mood*, (3) stronger sense of *internal political efficacy*, and (4) *decreased anxiety* associated with going to the polls and (5) *increased pride* associated with going to the polls.

*Mood.* Regarding mood, I propose that voting—regardless of the candidate one votes for—significantly improves both of these. The improvement should be greatest for first, second, and third time voters.<sup>10</sup> Essentially, successfully casting a ballot should

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<sup>10</sup> Specifically, I propose that the changes are greatest for individuals in the early stages of habit formation. Habit formation results primarily from repeated behavior, but also repeating the

make one's current emotional state more positive. In the "circumplex" model of emotions, all emotional states can be represented on two primary dimensions, arousal and positive-negative valence (see Posner, Russell, and Peterson 2005). Contentment is simply the most positively valenced emotion in the circumplex model.<sup>11</sup> Also, the word "mood" typically describes diffuse emotions that are not specifically linked to a particular activity; and I propose that the emotional changes are caused by the specific act of voting. However, I use "mood" because most people are poor at attributing emotions to causes, and so will experience the voting-related emotional changes as diffuse rather than specific task-linked changes.

Although almost no research exists on the effects of voting on emotional states, studies suggest that there may be quite a large underlying relationship. Blais and Gelineau (2007) use data on voting in Canadian elections in 1997 to demonstrate that voting for the candidate who wins the election increases feelings of satisfaction with democracy. Some research also suggests that the act of voting—regardless of which candidate or party one votes for—is associated with self-reported happiness and life satisfaction. Happiness and life satisfaction reports may be considered rough proxies for mood and contentment levels. But methodological issues have not allowed researchers to determine the direction of causation. Weitz-Shapiro & Winters (2008) find a positive and

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behavior in a stable setting (i.e., at the same precinct polling location). So, reference to "first, second, and third time voters" is somewhat misleading—but only somewhat. Even if one moves polling locations, many of the emotional associations are still intact; and, I argue throughout that the context/setting for voting is far less critical for habit formation than for other activities.

<sup>11</sup> One might argue that voting could shift an individual's emotional state to both a more positive *and* aroused point (thus shifting the individual's mood on both dimensions of the emotion circumplex). Some voters may become excited after casting a ballot, especially when a particular race is close or the voter has strong candidate preferences. However, I also expect voting to reduce anxiety associated with going to the polls (discussed below). Although anxiety is more of an "avoidance" emotion (potentially on a third dimension of the circumplex), it is also a state of heightened arousal. I therefore have no expectations about levels of arousal following voting.

significant relationship between voting and life satisfaction in Costa Rica, without confirming the direction of causation. Dolan, Metcalfe, and Powdthavee (2008) find that people who voted in the previous election are more likely to be satisfied with their life, but not that voting causes life satisfaction. Clearly, the relationship requires further empirical investigation.

The act of voting, regardless of the candidates one votes for, should improve mood and contentment because voting is consistent with virtually all social norms. Parents, friends, co-workers, and others in society virtually all approve of voting. To the extent that there is variation in parental, peer, or regional norms of voting, they should influence turnout largely by changing the reward structure (by increasing the level of positive feelings one would experience as a result of voting). So, if an individual's friends and family are less concerned with political participation (as is the case in friend circles and families with the lowest education levels), then she may feel less pressure to vote. This is almost equivalent to the main argument in Rolfe's (2012) social theory of voting. She argues that voting is shaped primarily by an individual's social connections—including the number and structure of connections and characteristics of those in one's social network, like how educated they are. But one must measure the psychological rewards to directly hypothesize about them. Rolfe did not measure the psychological experiences associated with voting, and how they are different in different social networks.

However, the theoretical framework I have begun to propose is far more *self-centered*. Voting is of course highly social. And social norms are likely very important for motivating one to vote for the very first time—before any positive emotional rewards

are experienced (see the section on costs and benefits in Chapter 2 for a brief discussion of evidence for social motives for voting, including altruism and social pressure). Social norms should allow one to feel good after voting.

So that my claims do not appear too inflexible, I should emphasize that *both* social and self-related constructs will be important to understanding turnout. Turnout is probably best explained with reference to an indirect effect suggested two paragraphs above. But the prior research on turnout has almost exclusively emphasized the social factors, at great risk to understanding turnout. Although social norms surely determine whether one experiences emotional changes and increased positive mood from voting, the strongest predictors of emotional and mood-related changes appear to be personal. Voting is a deeply personal experience. Most people do not have extensive interactions with family members and peers about their voting experiences on Election Day by which their self-conception is improved only because of direct social approval—or direct avoidance of social disapproval. If direct social approval or disapproval were required for voting rewards, no one would vote.

Instead, I propose that most of the positive emotions experienced from voting are because individuals like to view themselves in positive ways. Specifically, one's identity as a "voting person" is a primary way of incentivizing voting early in one's voting career. Of course, the *direct* causes of future turnout in my theoretical understanding (including voting habit strength) are surely far more important for understanding turnout than the causes of voting-induced emotional changes (an indirect effect with a relatively long causal chain). But considering the importance of one's personal identity in many domains, this variable is important to study. Additionally, variations in identity are far

greater than variations in social norms. Most people already concede that voting is important, and report feeling guilty if they do not vote. So it is difficult to believe that social networks and social norms within those networks could be far more consequential for voting outcomes than personal identity.

*Efficacy.* The second proposed psychological reward of voting relates to the sense of self-efficacy. Turnout scholars have long known that internal and external political efficacy powerfully influence turnout (Campbell et al. 1954; Easton and Dennis 1967). Internal efficacy is the belief that one can understand politics and therefore participate competently in it. External efficacy is the belief that that one's political participation will be effective (i.e., that government will respond to one's participation). In general, research by Bandura (1977) shows that successfully completing a task boosts one's feelings of efficaciousness in that domain, which should make the task more easy and likely to be completed in the future.

Feeling a loss of control is essentially the opposite of efficaciousness, and is known to reduce participation in that domain. Losing control and efficacy also produces worse mental health outcomes and life satisfaction (Langer and Rodin 1976). Langer and Rodin (1976) demonstrate that nursing home residents living in decision-free environments who were given more freedom to make decisions—including control over caring for a plant, and staff communications that emphasized their personal responsibility—had significantly improved alertness and life satisfaction over time. More behavioral control also caused residents to participate in nursing home activities—especially active and social activities—at a higher rate, the authors find.

Voting and political efficacy should interact in a similar manner over time. Successfully participating in politics by casting a ballot should strengthen one's sense of political efficacy. Specifically, voting should increase one's internal political efficacy more than one's external political efficacy. Internal political efficacy is self-referential, and going to the polls or not going to the polls is carried out by oneself. I would not expect it to influence one's beliefs about the broader political system, which external efficacy references. More specifically, Valentino, Gregorowicz, and Groenendyk (2009) use American National Elections Studies panel data to demonstrate that voting is associated with increased levels of internal political efficacy, especially if one's favored candidate wins the election, which makes the person more likely to vote in the future. The evidence from Valentino et al. (2009) alone is a powerful reason to expect reciprocal relationships between turnout and efficacy.

*Anxiety and pride associated with voting acts.* The third reward of voting is related to the emotions associated with going to the polls. Voting should also decrease anxiety associated with going to the polls. Gerber et al. (2003) propose this as a potential mechanism by which voting is habit-forming, even though they do not test the proposition. Research from a wide variety of perspectives in social psychology also suggests that carrying out an action decreases anxiety about performing the action again. Wood, Quinn, and Kashy (2002) confirm empirically that habitual behaviors are associated with more positive affect and fewer feelings of stress, overload, and lack of control. Wood et al. do not, however, confirm that repeated behavior in a stable context *causes* changes in feelings of stress, overload, and lack of control. It may be that people who have less anxiety with those tasks select them as tasks to repeat.

But other researchers, including Vallacher and Wegner (1987, 1989), generally find that more experience with an action leads to less anxiety associated with it. Additionally, scholars continually find that anger motivates political action, including turnout (see Valentino et al. 2009). It is therefore reasonable to expect that people gain a sense of contentment or improved mood after voting with higher-than-average daily anger levels. Researchers have elsewhere suggested that “release” feelings are experienced after carrying out an angry action.

### *Automaticity*

As an action is repeated in a consistent setting over time, it begins to be carried out more automatically. This basic relationship between repetitious behavior in a stable setting and automaticity should hold for voting too. Voting the first time is less automatic and requires more effort: finding one’s polling place, making travel decisions, knowing what to bring (if registering on Election Day), and adjusting one’s daily routine to incorporate the voting trip.

Readers who habitually vote may have difficulty imagining that this effort exists, since it is relatively unnoticeable for them. Automaticity is a difficult concept to think about, since by definition, it involves not thinking. I can conceptualize how personality or emotional states influence behavior far more easily, since it is easy to imagine myself feeling that way. To this point, Neal et al. (2011) demonstrate convincingly that as an action becomes more habitual, individuals are actually *more likely* to think the action is strongly influenced by their conscious goals and intentions—and less likely to think the action is caused by habit.

In reality, what we think, want, and intend to do matters less and less as an action becomes automated. So even though most inexperienced voters claim in surveys that they strongly intend to vote, many do not. In my data for Minnesota, about 88 percent of people reported being extremely or very likely to vote. Only some of them voted of course. This is understandable given that first-time voters have not formed any identity as a voter yet, there is effort in voting, voting is associated with more anxiety (probably outside conscious awareness), because they have not yet had positive, rewarding experiences with voting yet, and there are no formal repercussions for not voting. In short, it is easy to why, when the process of habit formation has not yet started, small impediments to voting that crop up on Election Day will be more likely to stop a non-habitual voter from voting. It is perhaps unsurprising that the most common response for why young people do not vote in presidential elections is “too busy, conflicting schedule.”

Psychologists, beginning largely with Bargh (1994; see also Wood and Neal 2007, 2009), have defined “automatic” actions as actions performed efficiently and with less conscious awareness, control, and intentionality. Thus, as voting becomes more automatic, individuals carry out the planning and travel acts more efficiently, do not think consciously about them as much, vote with less need to control those tasks, and are less influenced by their intentions. Intentions certainly matter for initiating voting among habitual voters still, but they are more likely to vote regardless of intentions. Specifically, habitual voters are more likely to vote even if they do not care much about the candidates and issues in that particular election.

This view stands in stark contrast to common political science perspectives on voting. Political scientists typically assume that voting is a “decision,” and one made relatively deliberately, consciously, and with intention and some effort. They imply that there is a large degree of information processing, especially about the costs and benefits of voting (see Aldrich et al. 2011). In reality, only some of what people do in the average day is done deliberately, consciously, or with much intention and effort.<sup>12</sup> While non-habitual voters certainly think more about voting—how, whether, and when to go—habitual voters do it far more automatically. And habitual voters are likely to vote regardless of whether they are particularly motivated by the political issues or candidates in a particular election, a key proposition worth re-iterating.

In ten years, it is likely that a far greater share of political science research will emphasize the automatic components of political behaviors. After the “cognitive revolution” in psychology in the 1950s and 1960s, psychologists turned away from stimulus-response and habit-based understandings of behavior and heavily emphasized the role of cognition—especially information processing—in theories of social and political attitudes and behaviors. The cognitive revolution eventually made its way into political science, after some delay, and political science theories subsequently began emphasizing cognition. But Bargh and others ushered in an era of automaticity in psychology that may soon lead political scientists to describe many attitudes and behaviors in terms of their automatic components.

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<sup>12</sup> Researchers estimate that about 45% of the activities we perform each day are habitual (Wood et al. 2002). Wood et al. define an activity as “habitual” if it is carried out frequently in a consistent, specific context, thereby reaching a high level of automaticity.

## *Identity*

When an action becomes habitual, it not only becomes automatic, but performing it also becomes more central to one's identity (see Verplanken and Orbell 2003). Voting more and more thus leads one to perceive oneself more strongly as a voter, and shifts the "voter" aspect of one's identity to a more central location in the broader self-concept. Bem (1972) and subsequent self-perception research demonstrates that individuals perceive themselves based on how they act. Even if a person acts in a way that contradicts her previously held values, she is likely to shift her self-concept to view herself as "a person who acts that way." The implication is that voting causes a person to think, "I voted, therefore I must be a political person who votes!"

Consequently, one is far more likely to go to the polls again and again after beginning to vote frequently. Individuals generally go to great lengths to protect central aspects of their self-concept. They seek out and process information selectively to protect the self (Sanitioso and Wlodarski 2004) and alter behavior as when self-handicapping (Feick and Rhodewalt 1997). And habitual voters are likely to protect their self-concept by making sure to go to the polls. Such self-protection is why the identity-altering aspect of voting makes voting so powerfully habit-inducing. Self-protection is more powerful even than self-advancement (see Baumeister et al. 2001). Prior to voting ever, an individual may vote to advance/improve her self-perception. But once she begins to vote, she will go to far greater lengths to ensure she keeps voting. Baumeister et al. (2001) note that avoiding negativity is far more important than gaining positivity for individuals' self-perceptions. Consequently, habitual voters experience greater psychological discomfort than non-habitual voters if they stay home on Election Day—

since it will lead them to necessarily question their identity as a voter and perhaps experience self-esteem loss.<sup>13</sup>

Note that this proposed mechanism underlying habitual voting is very different than the predictions of social identity theory. I do not argue that people vote to express solidarity with any social group. The group called “voters” is so large, vague, and heterogeneous that one could hardly vote to express solidarity with them. The mechanism I propose here is self-centered.

Habit researchers have not reached a consensus on whether a sense of personal identity is a necessary feature of habits. Orbell and Verplanken (2003) include four items that measure the identity component of habit strength—and in the dissertation I create items that are intended to measure the same component but be appropriate for voting. I construct two original two items, one of which I consider key in the measurement (“Voting is an important part of who I am as a person”). Since then hundreds of studies have used the Verplanken and Orbell index,<sup>14</sup> and have relied on the full set of items—which includes the identity items. But scholars have recently questioned whether these items are needed. If they do not measure the concept of habit, they should not be included in a habit index.

Given the strong correlation between identity factors and automaticity factors in self-reported habit items in existing research (correlation of .7 reported in Gardner, de Bruijn, and Lally 2012) I suspect that identity is an important if not necessary feature of

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<sup>13</sup> Also for that reason, habitual national election voters (those who vote frequently in presidential and midterm elections) may not feel great discomfort if they do not vote in municipal elections. Voting in municipal elections is generally considered a qualitatively different form of voting and political participation.

<sup>14</sup> For a systematic review of the nutrition and health-related studies see Gardner, de Bruijn, and Lally (2011); also see Lally et al. (2010).

habits. I have yet to see any empirical evidence demonstrating that the identity measures are entirely different from habit. Nevertheless, the rhetoric is often heated. ,,,, few scholars claiming that identity measures are entirely unrelated are unsubstantiated ( ) in the types of behaviors most likely to generate the identity association after repeated performance in a stable setting. For a recent discussion of the identity component, see Gardner, de Bruijn, and Lally (2012).

### *Stable Context*

For an action to become habitual, the setting (or context) must be relatively consistent over time as the action is performed frequently. The setting includes the place, time, people in the setting. The context might even be conceptualized as a goal, as Orbell and Verplanken (2010) argue. It includes almost anything that might evoke the automatic response in memory.

Although a stable setting is an important feature of habit formation, and has several implications for voting, I devote less time to this aspect of habitual voting. Aldrich et al. (2011) emphasize voting location extensively in their discussion of habitual voting; and it is clear that disrupting the physical context of voting by changing residences decreases the likelihood of voting. However, to understand how voting habits develop and grow over time, context is far less important. The performance context is virtually always stable in elections. In fact, the context stability is naturally built into voting behavior. For most voting that actually occurs in the U.S., it takes place on the first Tuesday of November. There are campaigns and news stories and advertisements that all highlight the day and features of the context too.

There are two features of the context that might vary. First, people are increasing voting early in the U.S.—prior to Election Day. For early voters the temporal flexibility may make habit formation more difficult. But there is some evidence to suggest that the people who take advantage of the traditional forms of early voting are more likely to be habitual voters. I address this extensively in Chapter 6.

Second, the only other feature of the voting context that varies significantly is the location. When people move residences, they might move precinct voting locations. This might potentially disrupt the habit to an extent. However, while the context is important for establishing the automatic component of habitual voting, I believe strongly that context is far less important (perhaps even inconsequential) for establishing the *identity component* of habitual voting. Once a person begins to vote frequently—thus experiencing positive psychological rewards and seeing oneself as a person who votes—one’s self-conception is unlikely to change dramatically even after moving to a different precinct. I test several of these claims in Chapter 4. Nevertheless, moving residences clearly has a negative marginal effect on turnout. But since this effect is already well established by prior research and less relevant to my theory of habitual voting, it is not theoretically as interesting unless I establish that there is significant heterogeneity in the effects of context disruptions that previous research ignores. I do in fact find evidence of this in Chapter 4.

#### *Voting Habit Formation Overview*

To summarize, I propose that voting generates specific psychological “rewards,” including improved mood, political efficacy, and decreased anxiety and increased pride associated with going to the polls. These rewards, in turn, increase the likelihood of

voting in the future. After voting repeatedly several times, two important psychological changes occur in individuals: (1) Election Day activities (e.g., traveling to the polls and planning-related tasks) become more automatic and effortless, and (2) voting becomes a more central component of individuals' identities (e.g., they see themselves as political people who vote). These two changes are the definition of a strengthening habit.

Habitual voters continue to vote over time largely because of these dual factors: automatic Election Day actions and, more crucially, the centrality of voting to their identities. The basic characteristics of habitual and non-habitual voters are displayed in Table 2.

**Table 2. Characteristics of Habitual and Non-Habitual Voters**

	<b>Voting Automaticity</b>	<b>Voting Identity</b>	<b>Rewards</b>
Habitual Voters	High	Central	Less strongly influences future turnout
Non-Habitual Voters	Low	Not central	More strongly influences future turnout

Is voting really like brushing teeth? The psychology literature developed with daily and weekly tasks in mind (i.e., brushing teeth, driving to work), and the bulk of empirical testing of these psychological propositions were carried out with data on such tasks. However, numerous studies have also been conducted on annual and biannual tasks like voting, giving blood, and vacation planning. In fact, there is now a large research literature on the predictors of blood donation. This literature is only sometimes referenced by social psychologists. But reviews of blood donation research demonstrate that both past behavior (sometimes described as “habit” in that literature) and identity

variables powerfully influence future blood donation behavior (Masser et al. 2008; Biddle et al. 1985; Armitage and Connor 2001). Masser et al. (2008, 215) conclude that “changes in motivation and the *development of self-identity* as a blood donor are crucial for understanding the processes whereby *first-time donors become repeat donors*” (italics mine).

Although the characteristics of habitual actions are not evidenced as strongly for annual and biannual activities as for daily and weekly ones, they are nevertheless evidenced for them, as shown by results in the dissertation and studies of less frequently preformed activities. In other words, there is variation in the strength of habits for annual and biannual tasks like voting, as there is with daily and weekly ones. But the mean habit strength values are higher for daily tasks. The primary difference between daily habits and voting habits is that intentions and identity should influence the latter far more.

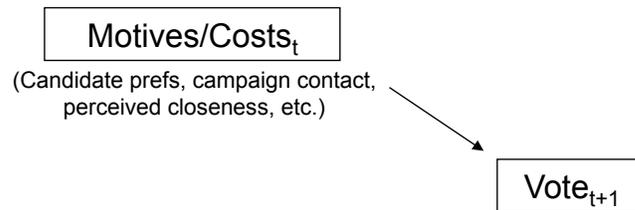
### **Incorporating Habit in a Theory of Turnout**

How are the various psychological factors responsible for voting habit formation related to each other, and related to the determinants of turnout typically emphasized by political scientists? In this section, I explain the a broader theory of turnout that incorporates prior voting frequency, voting habit strength, the psychological rewards of voting, election-specific factors emphasized in existing studies, and future turnout.

Figure 2 displays the typical political science model of turnout. In this model, election-specific motives and costs directly influence turnout (Vote). “Motives” include the strength of candidate preferences, feelings of civic duty, political interest, campaign contact, strength of party identification, and perceived closeness of the election. Motives can be conceptualized as the benefits of voting discussed in the section above on prior

research. I use the term “motives” rather than “benefits” to differentiate them from the psychological rewards of voting that initiate habit formation. “Costs” include free time, distance to the polling place, having a car, and voter registration. Although factors like education, church attendance, and “civic skills” might be conceptualized as costs or motives, I do not believe that they have a direct proximate causal influence on turnout in the same way that the other motives and costs do. So I avoid discussing them in this section.

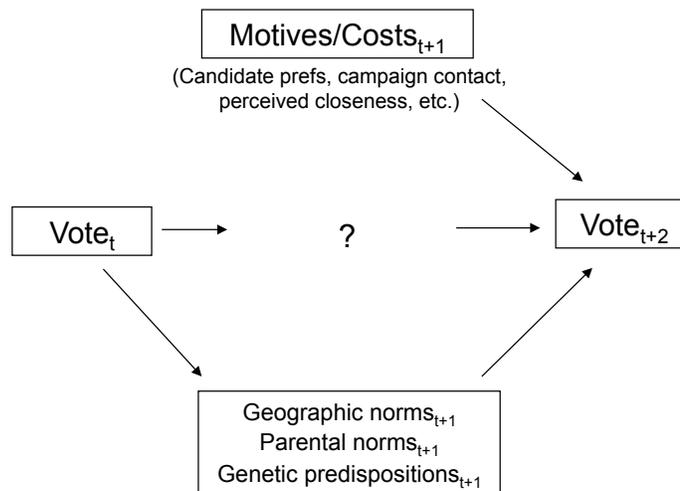
## Figure 2. Prior Theories of Turnout #1



The direct causal relationships specified in Figure 2 ignore the dynamic features of voting habit formation. Figure 3 displays the structural relationships in more recent research on turnout. A number of recent studies have demonstrated that individuals who vote in the past election are about 50% more likely to vote in the next election, all else being equal (Gerber et al. 2003). Past voting ( $\text{Vote}_{t-1}$ ) strongly influences future voting. Voting apparently makes you want to vote again and again, regardless of the election-specific and static motives and costs. This could be evidence of “habitual voting,” and certainly fits with common conceptions of habit. But political scientists have a poor understanding of how and why people develop the habit of voting over time, and what the psychological mechanisms responsible for persistence in voting over time (thus, the question mark in Figure 3). Aldrich et al. (2011) have offered an explanation of habitual

voting, but for the most part the phenomenon is not understood. Additionally, more recent studies of turnout have included measures of regional and parental norms and genetic predispositions (the bottom-most box in the figure; Fowler, Baker, and Dawes 2008).

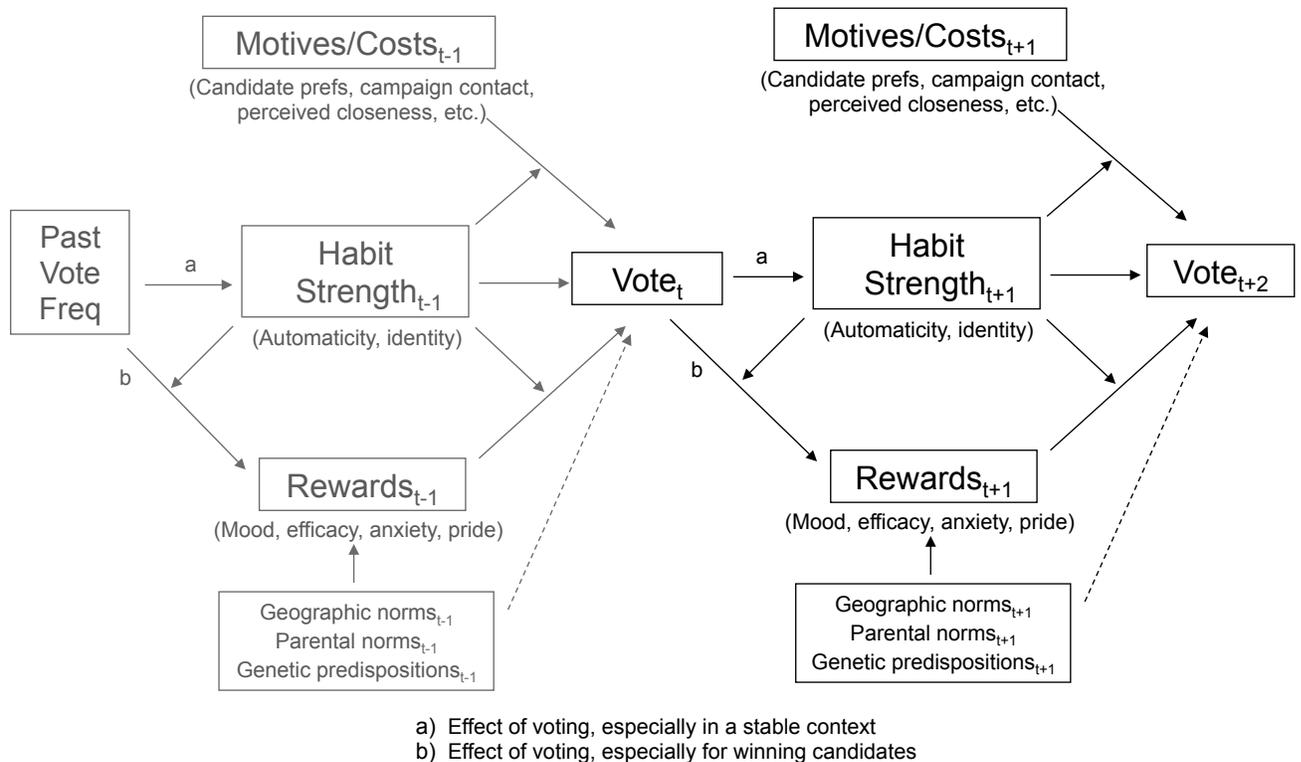
**Figure 3. Prior Theories of Turnout #2**



My alternative theoretical framework for understanding voter turnout is summarized in Figure 4. Prior voting does not have a direct unmediated influence on future voting. Instead, prior voting, especially in a stable context, makes voting more automatic and strengthens one's identity as a voter. (The defining features of voting habit strength are included in parentheses: automaticity in voting acts and centrality of voting to one's identity.) The arrow from  $vote_{t-1}$  to rewards indicates that voting generates psychological rewards (improved mood, greater political efficacy, and decreased anxiety associated with voting) that make future voting more likely to happen. The rewards should be experienced more strongly if one's favored candidate wins. The process in Figure 4 should be conceptualized as a loop: after each iteration  $vote_t$  becomes  $vote_{t-1}$ ,

and the process begins again. In this way voting strengthens the habit of voting *indirectly through psychological rewards* and also *directly by influencing automaticity and voting identity*.

**Figure 4. A Dynamic Theory of Habitual Voting**



In this theoretical framework, there are two conditional effects of voting habit strength. The moderating arrow pointing down and to the left from habit strength indicates that the habit of voting moderates the effect of past voting on rewards. Essentially, when habit strength is low (the first, second, and third times voting), the psychological rewards should be experienced intensely after voting. But when habit strength is high, the rewards should be experienced less intensely as a result of voting. Finally, the moderating arrow pointing up and to the right from habit strength indicates that the habit of voting moderates the effect of motives and costs on future voting.

Conceptually, this means that for individuals with weak habits, motives and costs strongly influence voting, as political science theories often emphasize. But for habitual voters, motives and costs have a much weaker influence—since habitual voters vote more automatically and are less influenced by intention.

Overall, this perspective differs dramatically from prior political science perspectives on turnout, which generally ignore the role of automaticity, identity, emotions, and psychological incentives for voting. In fact, prior studies of turnout rarely analyze any constructs similar to these (perhaps besides emotions). Rather, turnout scholars usually assume that voting is the result of conscious, deliberate processing. And the theory is dynamic. Voting habits form slowly over time, and the predictors of turnout therefore have noticeably different effects for individuals at early and late stages in the habit formation process.

Of course, the theory overlooks some influences on turnout—including variables related to brain systems, physiology, and educational attainment. I exclude education because scholars largely do not know how it influences turnout (for several propositions see Sondheimer and Green 2010), or whether its effect is causal. I suspect that there is some causal effect of education but that the causal chain is so long and complex that using education-related variables will be not particularly helpful to explain turnout. And I have not yet detailed the many ways that the numerous costs and benefits of voting interact to shape final behavioral outcomes. I have also simplified the notation by leaving off arrows connecting  $\text{motives}_{t-1}$  and  $\text{motives}_t$ , and  $\text{rewards}_{t-1}$  and  $\text{rewards}_t$ , and analogous connections. Nevertheless, theory as outlined is more comprehensive in its scope than prior turnout theories, and simultaneously flexible enough to allow substantial

changes without becoming irrelevant. For findings about turnout to accumulate more effectively, pursuing a more comprehensive theoretical framework will be important.

Finally, the theory of habitual voting has many specific and nonintuitive predictions about turnout that can be tested empirically. One challenge in testing the theory is that many of the concepts—including those related to habit and psychological rewards—have not yet been defined or measured in prior research. And the empirical tests will require new data sources and a careful focus on measurement. Given Aldrich's (1993) warning that voter turnout is highly sensitive to small changes in costs and benefits, and that empirical models of turnout are uniquely sensitive to the measurement of costs and benefits, a focus on measurement is all the more important. For that reason, I devote the following chapter to conceptualizing and measuring voting habit strength and its components, automaticity of Election Day activities and centrality of identity as a voter. Without strong measures of these concepts, the theory must be left as it stands now. After developing strong measures of the psychological variables, I test 13 propositions generated by the model in Chapter 4.

### **Chapter 3**

#### **Measuring the Strength of Voting Habits**

Although psychologists have long understood that habit powerfully influences social behaviors, they did not develop direct empirical measures of habit until relatively recently. Until the 1990s, researchers in psychology almost exclusively relied on measures based on prior frequency of behavior (Verplanken 2006). For example, individuals attending sporting events frequently might be thought to have stronger sporting event attendance habits. Such a measurement strategy was inappropriate largely because habit and prior behavioral frequency are distinct constructs. Habit is a psychological construct, as emphasized in numerous studies on habit (Verplanken and Aarts 1999; Verplanken and Orbell 2003).

The broadest conceptual definition of a habit is “a psychological disposition to repeat past behavior” (Neal et al. 2011, 492). Operationalizing this concept without further specification is virtually impossible though. To operationalize habit, researchers must know the specific features of the disposition that make future action likely to be repeated. Prior scholarship indicates that habit is the automaticity of a behavior linked to a specific context, such as a place or time (Ouellette and Wood 1998; Verplanken and Aarts 1999; Aarts and Dijksterhuis 2000). For many researchers, habit is also the relevance of the behavior to one’s identity (Verplanken and Orbell 2003).

Existing measures of voting habits in political science are at odds with the understanding of habit in psychology. Like psychologists in much of the 20<sup>th</sup> century, political scientists typically measure the strength of habits using indicators of behavioral frequency, such as the number of prior elections a person voted in. Frequency-based

measures are rough, indirect proxies for the underlying psychological construct. Moreover, few turnout scholars have systematically defined the concept of habitual voting, or empirically examined the psychological mechanisms responsible for habitual voting (but see Aldrich et al. 2011; Valentino et al. 2009).

In the previous chapter, I draw on a broad range of research in psychology to define the concept of voting habit as: (1) automaticity of Election Day actions and (2) relevance of voting for the self-concept. I also present a broad theoretical framework for understanding voter turnout based on this definition. The theory addresses the important deficiencies in existing turnout research outlined in Chapter 1, primarily by specifying the over-time causal relationships among the many determinants of turnout. I treat prior turnout, habit strength, and future turnout as distinct concepts.

Testing the theory's propositions requires a valid and reliable empirical measure of the strength of voting habits. In this chapter, I proceed by first considering alternative measurement strategies. I consider direct and indirect measures of the psychological features of voting habits. Indirect measures represent constructs that are similar to, but different than, the intended one. The two most common indirect measures of habit are prior voting frequency and multiplicative frequency-context indicators (see Neal et al. 2011). Direct measures might use survey self-reports or variants of the Implicit Association Test (IAT) to assess the psychological features of habits. I argue that indirect, frequency-based measures do not validly represent the strength of voting habits.

My measurement strategy relies on survey self-reports. I adapt the self-report habit index (SRHI) developed by Verplanken and Orbell (2003), intended to measure the strength of habits for daily and weekly activities. I term the measure the Voting Habit

Strength Index (VHSI) to differentiate it from previous, non-voting-related measures.

The voting habit index differs from the SHRI in four important ways. First, I use question wording specific to voting and voting-related acts, like traveling to the polls and arranging one's schedule to include a trip to the polls.

The second difference is that I include more items to assess the identity component of habit. Verplanken and Orbell's SHRI includes only one item that explicitly assesses the importance of behaviors for individuals' identities. I alter the wording for the SHRI identity item and construct three new ones. As I emphasize in Chapter 2, the identity component should be a more central aspect of habits for behaviors like voting, blood donation, and attending sporting events, which are performed relatively infrequently. A single item is insufficient to validly and reliably measure the importance of voting to one's self-concept. Additionally, the dissertation requires a separate identity subscale to test any hypotheses about the distinct effects of identity and automaticity on turnout.

The third important difference is that the VHSI assesses automaticity specific to the voting context. In all conceptualizations of habit in psychology, a behavior becomes automatic as it becomes associated with a specific performance context (e.g., time and place). As recent habit scholars have pointed out, Verplanken and Orbell's SHRI includes items that ask about behaviors in isolation from the context (Sniehotta and Pressau 2012). Events like watching TV can occur anytime and anyplace, so a question that assesses habitual TV-watching by referring only to "watching TV" may not be

valid.<sup>1</sup> The questions I use to measure voting automaticity in the VHSI refer to the context with terms like “on Election Day.”

The fourth point of departure from the SHRI: the voting habit strength items avoid explicit references to behavioral frequency. Both indices are intended to measure the psychological construct of habit rather than behavioral frequency. But the Verplanken and Orbell index includes two items that explicitly reference frequency: “Behavior X is something I do frequently” and “Behavior X is something I have been doing for a long time.” These are inappropriate given that habit and behavioral frequency are distinct concepts (see Gardner, Abraham, Lally, and de Bruijn 2012).

In this chapter, after clarifying the concepts and outlining my measurement strategy, I use data from an original panel survey of registered voters in Minnesota to assess the validity of the VHSI. The survey data are supplemented with information on respondents’ turnout over a six-year period from the state voter file. The empirical analysis has four steps: I use a confirmatory factor analysis (CFA) to test propositions about the two-factor structure of voting habit strength. Additionally, I further validate the VHSI by testing propositions about how voting habit strength relates to prior election voting. The relationship between habit and prior voting frequency should be asymptotic, as Lally et al. (2011) find for several non-voting activities.

In the third stage of the empirical analysis, I conduct a response time analysis for the automaticity and identity items. Response time data allow me to assess the validity of the separate identity and automaticity subscales. Faster times for questions related to the

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<sup>1</sup> The SHRI items might instead refer to behaviors in a specific context, like TV-watching at home before going to sleep (see Snihotta and Pesseau 2012). If a person sometimes watches TV, but always does so in a different place and at a different time, her habit should be weaker than if she watched at the same place and time.

automaticity of Election Day actions should indicate actions that are more automatically carried out in real-life settings. Finally, I assess the predictive validity of the VHSI. The results indicate that the VHSI predicts future turnout independent of most other major factors known to influence turnout, including political interest, campaign contact, and political efficacy. I conclude by discussing the implications for empirical studies of habitual voting, psychology theories of habit formation, and political science theories of turnout.

### **Alternative Measurement Strategies**

The language turnout scholars use to describe voting habits varies greatly and reflects significant uncertainty about the concept. Prior studies sometimes use the term “habitual voter” interchangeably with “frequent voter,” “always voter,” and “high propensity voter.” Researchers at other times discuss “seldom voters,” “intermittent voters,” “presidential election voters,” and “disaffected voters.” Existing empirical measures of habitual voting also reference individuals’ frequency of prior election voting. For example, Malhotra et al. (2009) identify habitual voters as individuals who voted in each of the past 4 elections in their analysis (3% of the authors’ sample). Aldrich et al. (2011), in one of the most theoretically driven investigations of habitual voting, identify habitual voters as individuals who vote in multiple elections at the same location (distinct from those voting in multiple elections in different locations). Almost universally, existing turnout studies rely on proxies for habitual voting somehow based on past voting frequency. As the most common measurement strategy, I consider it first.

### *Frequency-Based Measures*

One of the greatest risks associated with measuring habit with indicators of prior voting frequency is that the measure can represent effects of myriad unobserved influences on past turnout that are unrelated to habit. This problem is discussed extensively by prior scholars and plagues all analyses of the effects of lagged dependent variables (see Denny and Doyle 2009; Gerber et al. 2003). When estimating the effects of past turnout on future turnout, researchers must control for all unobserved factors that influence both past and future turnout. Otherwise, the magnitude of the estimated prior voting effect—representing “habit”—will be biased upwards, since it captures effects of anything that shaped turnout in prior elections. And the standard error will be biased downwards. Scholars must control for the myriad time-invariant factors, such as educational attainment, political efficacy, personality traits, parents’ partisanship and political behavior, church attendance, home ownership, region of residence, and numerous other stable individual traits. Typically scholars treat any residual variance in future voting explained by past voting as “habit” (see Gerber et al. 2003; Denny and Doyle 2009; Meredith 2009).

Controlling for unobserved heterogeneity to provide an unbiased causal estimate of past voting on future voting has proven enormously difficult. To date, Gerber et al. (2003) provide the soundest empirical estimates. The authors use a two-stage least squares approach, with treatment conditions from an earlier randomized field experiment as the instrumental variables. Individuals who were randomly assigned to receive voter mobilization mailings or canvassing visits (who voted at higher rate) were also significantly more likely to vote in the subsequent election. However, the empirical

strategy used by Gerber et al. (2003) cannot be easily imitated in future turnout research. Scholars are rarely fortunate enough to study individuals previously involved in randomized experiments that boost turnout for a portion of the sample. Directly measuring the psychological properties of voting habit strength with survey self-reports would be far more convenient.

Measuring habit strength with prior voting frequency is also conceptually problematic. This measurement strategy assumes that the more often an individual voted in the past, the stronger her voting habit is. This strategy would be reasonable—statistical problems notwithstanding—if (1) voting frequently were a necessary *and sufficient* condition for voting habits to develop and (2) if the frequency of prior voting were linearly related to the strength of voting habits. If these two conditions were met, then variation in habit strength would be captured entirely by variation in prior voting frequency, and a direct measure of habit strength would be unnecessary to estimate some of the effects in the dissertation’s theory of habitual voting. Essentially, if voting often were sufficient to turn someone into a habitual voter—and strengthened the voting habit in proportion to the number of trips to the polls—then no direct measure of the psychological properties of voting habit strength would be needed.

While voting frequently is necessary to become a habitual voter, it is not sufficient for doing so. As discussed in Chapter 2, voting repetitively in a stable context is largely responsible. Both repetition and context are involved. People who change their voting context by moving residences will not form the habit as quickly, even if they vote often. In particular, people who move regularly should not develop much psychological automaticity in voting, or have it become as central to their identities. Of course, most of

the contextual features of voting are relatively stable. People can vote in presidential and midterm general elections on the first Tuesday of November. But there is some variation in physical context, largely in the location of one's polling place. Some people move to residences in different precincts. For this reason, a measure of prior voting frequency would not validly represent the concept of voting habit strength.

Additionally, in Chapter 2, I propose that voting for the winner<sup>2</sup> helps to establish the habit of voting more quickly, drawing in part on findings by Valentino et al. (2009). Most of the psychological rewards experienced as a result of voting occur on Election Day: during and immediately after leaving the polls, people experience mood changes, feel more efficacious, and reconceptualize specific acts as less anxious and more pride-generating. These rewards, experienced most intensely for non-habitual voters, produce associations in procedural memory that facilitate future voting. The rewards should make voting more automatic and central to their identity (the two psychological attributes of habitual voters). But some of the sense of political efficacy and emotional experiences come after one realizes one voted for the winner—after learning the election outcomes. Valentino et al. (2009) demonstrate that voting for the winner increases internal efficacy over time. The efficacy and mood reward should make voting more likely in the future, and speed up the habit formation process. So the relationship between frequency of voting and habit strength should be different for individuals who vote for winning and losing candidates. And frequency of prior voting cannot serve as a proxy for habit strength since it ignores this dynamic.

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<sup>2</sup> Although people generally vote for many candidates in many races each election, I define “winner” as the candidate who wins the race for the highest office. The winners of lower level races should be far less influential on voters' feelings of efficaciousness and mood.

Prior voting frequency should not be linearly related to voting habit strength. Lally et al. (2010) demonstrate that for activities like exercising and eating fruit, the automaticity in performing them increases asymptotically over time—not linearly. The authors find that those acts quickly become more automatic after the first handful of repetitions, and then automaticity levels plateau as individuals subsequently repeat the behavior.<sup>3</sup> In that case, a measure of frequency of prior voting would be inappropriate to measure habit, since it assumes habit strength increases linearly as one votes more often.<sup>4</sup> Also, as discussed above, some individuals develop the habit more quickly depending on the physical voting context and the candidate they vote for. And some individuals are simply more prone to experience psychological rewards from voting and quickly become a habitual voter due to genetic predispositions, personality, and political and participatory norms of their families, close others, and community. As proposed in Chapter 2, these genetic/personality/social-contextual factors should shape how strongly the psychological rewards of voting are experienced. For example, if one’s parents value voting immensely, one should be more likely to experience positive affective changes after voting. In sum, there are many factors that determine whether the habit becomes strong after one time going to the polls, eight times, or never.

Until now I have focused primarily on conceptualization problems in the measurement process—why measuring voting habit strength using an indicator of prior voting frequency is inappropriate. But moving from the conceptualization to operationalization stage of the process highlights additional problems. One might

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<sup>3</sup> The average number of repetitions until plateau varies depending on the activity, according to Lally et al. But the basic asymptotic relationship held for each of the activities they examine.

<sup>4</sup> One might transform a prior frequency variable to make it conform to expectations about the asymptotic relationship with future likelihood of voting. But the transformation decision would be mostly arbitrary.

operationalize prior voting frequency by using: self-reported information about voting history (e.g., from a survey), administrative information from state voter files, or self-reports in panel data acquired after each election. Each of these three data sources introduces further measurement error to a measure of prior voting frequency.

Survey self-reports are problematic because people may either incorrectly remember turnout behavior from the past accidentally, or purposefully misreport voting.<sup>5</sup> Both situations produce significant measurement error. The first is likely since people have difficulty remembering events many years in the past. When measuring voting habits, one would need turnout information for many prior elections, which requires respondents to recall events as many as 8 years before the survey. And, a significant percentage of respondents in surveys purposefully misreport socially beneficial behaviors like voting. Misreporting in surveys occurs when respondents tell the interviewer that they voted when they, in fact, did not. This phenomenon would not be particularly problematic if over-reporting were randomly distributed in the population. However, people who are more likely to be habitual voters are also more likely to report voting when they did not—generating systematic measurement error that biases coefficient estimates for “habit” (as measured by prior voting frequency) in models of turnout. Specifically, individuals with more education, strong partisans, individuals contacted by a campaign, and more frequent church goers are more likely to over-report voting (Bernstein, Chadha, and Montjoy 2001).<sup>6</sup> These groups are also more likely to be

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<sup>5</sup> See Granberg and Holmberg (1991) for a detailed discussion of the reasons why surveys overestimate voting.

<sup>6</sup> The authors’ results are startling at times. Among nonvoters, 37% with a college degree or advanced degree are estimated to misreport voting, 30% of weekly church-goers misreport, and an estimated 49% of white non-voters in the Deep South misreport (Bernstein et al. 2001, Table 3). The authors’ estimates, based on comparisons of validated and self-reported voting from the

frequent voters. In fact, anyone likely to experience greater psychological rewards from voting should be more prone to over-report voting. Bernstein et al. (2001) echo this proposition by hypothesizing that groups under the most pressure to vote are the ones most likely to falsely report voting. Therefore, using a measure of prior voting frequency would introduce enormous problems when assessing the relationships between psychological rewards and habit strength.

Operationalizing prior voting frequency using administrative information from state voter files also produces significant measurement error. As with other measures of prior frequency, researchers must make decisions about which types of elections and how many prior elections to include. Given that prior measures of “habit” have not been guided by much theory, these decisions are usually arbitrary. And there are many decisions: researchers might use any combination of general and primary elections for presidential, midterm, local races. Constructing frequency-based measures of habit using state voter files is further difficult because researchers must know which prior elections a person was eligible to vote in (over 18 and lived in the state) to avoid counting recently eligible voters as infrequent voters.<sup>7</sup> There are also widespread inaccuracies in state administrative records. For example, public voting records are widely known to contain numerous inaccuracies due to people moving and changing names, without official records being updated (see Lupia, Krosnick, and Berent 2011). And Lupia et al.

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ANES, are for white respondents; for black and Hispanic respondents, misreporting among the educated is even greater (also see Abramson and Clagett 1991).

<sup>7</sup> Researchers might avoid problems of prior eligibility by calculating the percent of elections a person voted in using their registration date as the “start date” for eligibility. However, voters frequently fill out new registration requests (and therefore may have a new registration date) when they move or change names. Their voting history prior to the change may not be reflected in their current record used by the researcher.

demonstrate that the inaccuracies in voting records vary enormously across states, making comparisons cross-state measures of prior voting frequency difficult.

In sum, measuring habit strength with indicators of prior voting frequency is problematic for numerous conceptual and methodological reasons. Conceptually, prior voting frequency and habit strength are distinct constructs—and habit strength is a psychological construct. Prior voting frequency does not itself causally influence turnout. Instead, voting habit strength should mediate the effects of past voting on future voting, as detailed in the previous chapter. Moreover, frequently voting should not be a sufficient condition for developing a strong voting habit either—and the two constructs should not be linearly related. Methodologically, measuring habit with indicators of prior voting frequency is likely to produce significant error.

#### *Response-Frequency Measure*

The first measure of habit in psychology not based explicitly on behavioral frequency was the “response-frequency” measure (Verplanken, Aarts, van Knippenberg, and van Knippenberg 1994; Verplanken, Aarts, and van Knippenberg 1997). This measure relies on a task in which individuals select as quickly as possible a model of travel (i.e., bicycle) that they associate with specific travel situations. The response-frequency measure is inconvenient to administer, and can be used only for behaviors that have similar alternatives like travel choices—but has been shown to validly reflect the extent of automaticity linked to a specific context (Danner et al. 2008). Psychology researchers have largely abandoned this measurement strategy due to its inconvenience and its narrow application. Nevertheless, it started the important precedent of measuring habit directly.

### *Frequency x Context Measures*

A third strategy for measuring the strength of voting habits comes from Aldrich et al. (2011). The authors improve upon the usual measure of prior voting frequency by combining it with a measure of context stability in voting. The more an individual votes *and does not move residences*, the greater the strength of voting habit in the study. The authors' measure of habit is a simple product of prior voting frequency and stability of voting context. Neal et al. (2011) adopt a similar strategy for running and stadium attendance—using the product of prior frequency and context stability during performance. While Aldrich et al. draw effectively on psychology research to explain and define voting habits, their measure suffers from virtually all of the problems discussed above. In fact, the only advantage it has over a simple measure of prior voting frequency is its incorporation of physical voting context. This too is problematic since people residences move for many reasons—many of which are correlated with turnout, including age, education, and income. Researchers must control for such unobserved factors, as well as all factors that are correlated with prior turnout. Finally, a measure of habit that includes indicators of voting context is further problematic because context is not the underlying construct of interest (in the same way that prior voting frequency is not the same construct as habit). Voting context may be a *cause* of voting habit strength, but is an entirely separate construct.

### *Direct Measures of Voting Habit Strength*

Alternatively, researchers might measure the strength of voting habits directly. By relying on indicators of prior voting frequency, previous researchers have measured a *cause* of voting habits, rather than voting habits themselves. Aside from the numerous

difficulties with frequency-based measures described above, it is necessary to measure the strength of voting habits directly to test most hypotheses in the dissertation's theory of habitual voting.

The two features of voting habit strength are: automaticity in voting-related acts and centrality of voting to one's identity. Although individuals who have strong voting habits also experience the psychological rewards of voting differently than individuals with weak habits, experiences with rewards are not themselves features of habits.

Psychological rewards cause individuals to develop stronger voting habits over time, and are also caused by voting—voting frequently, especially for the winner, and in a stable context—but do not uniquely identify voting habits or habitual voters. For example, an individual may have a high degree of political efficacy (one of the rewards of voting) regardless of her voting habit. Having more political efficacy does not mean one is necessarily a habitual voter (even though habitual voters on average have greater political efficacy). By contrast, having high automaticity in voting and high centrality of voting to one's self-concept *does mean* that one is a habitual voter. Those features define voting habits.

Researchers can use either explicit or implicit measures to measure automaticity of voting-related acts and the centrality of voting to one's identity. "Implicit" measures are intended to assess the strength of automatic associations between constructs in memory—associations outside awareness, intentionality, and control (see Fazio and Olson 1995). Implicit measures are commonly constructed using the Implicit Association Test (IAT), developed largely by Greenwald and Banaji (1995) and motivated by Fazio et al. (1995). Automatic associations measured by the IAT are often thought to be pre-

conscious, in that they originate prior to conscious awareness. “Explicit” measures are intended to assess conscious, controlled associations in memory. Survey self-reports—verbal or written responses to survey questions—are commonly used to construct explicit measures.

Explicit measures, especially survey self-reports, are most effective in measuring the centrality of voting to one’s identity. The importance of voting to one’s identity is itself explicit and conscious. Self-reported information should therefore be capable of yielding a valid measure. One might argue that implicit measures have stronger validity since they might reflect pre-conscious associations between voting and identity, which are the “true” underlying associations. Perhaps survey respondents claim that voting is important to them when it is not, in actuality. However, scholars have repeatedly critiqued the validity of implicit measurement tools. Significant debate exists, for example, about whether attitudes measured by instruments like the IAT are actually pre-conscious (see Fazio and Olson 2003) and whether IAT results simply reflect cultural norms or other associations entirely (see Arkes and Tetlock 2004). And since the construct I seek to measure is the degree of *conscious* connection between voting and identity as a voter, explicit measures are entirely appropriate. To the extent that there is any discrepancy between implicit and explicit measures of this aspect of habit strength, I expect the discrepancy to be small.

I also believe that explicit, self-reported information is most appropriate to measure automaticity in voting-related tasks—the second dimension of voting habit strength. At first blush, it may seem appropriate to measure this subconstruct using tools like the IAT, since the IAT and related measurement tools are intended to gauge

automatic associations. And automaticity in voting is certainly not conscious and controlled. But, as noted above, I am concerned about the validity—particularly the content validity—of the resulting measures. The IAT measures something that influences numerous attitudinal measures across studies. But since psychologists cannot be certain that the “something” is automaticity, I err on the side of caution by relying on explicit, survey self-reports. Furthermore, it is most straightforward to utilize the same type of measurement strategy for both habit subconstructs—by measuring both with either explicit or implicit measures. And since political scientists and voter turnout scholars are less familiar with instruments like the IAT than psychologists, any index relying on implicit measures is less likely to be accepted and tested in a diverse set of samples. Nevertheless, I rely on some (implicit) response-time measures to validate my voting habit strength measure below. This strategy should appease both supporters of implicit and explicit measures.

Verplanken and Orbell (2003) develop a unique habit strength index using self-reports about daily and weekly activities—which motivates my measurement strategy. Their index has been widely (and even somewhat “habitually”) used by psychologists, most of whom have now abandoned frequency-based measures of habits.<sup>8</sup> While researchers have questioned the effectiveness of a few survey items used in the self-report index, it is still widely accepted as a valid and reliable tool to measure habits. It has been used effectively in numerous samples and settings as both a dependent and independent variable. Researchers have also used Verplanken and Orbell’s measure in

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<sup>8</sup> Wood, Neal, and colleagues in social psychology still measure habits with a product of behavioral frequency and behavioral context. However, as I argue above, this is conceptually and statistically inappropriate. I also demonstrate empirically below that this is an inappropriate measurement strategy.

many real-life settings to help people make and break habits, including exercising and eating healthily (Lally et al. 2011), texting while driving (Bayer and Campbell 2012), smoking (Orbell and Verplanken 2010), binge drinking (Gardner, de Bruijn, and Lally 2012), and flossing (Orbell and Verplanken 2010).

I adopt Verplanken and Orbell's strategy by constructing a self-report index of voting habit strength. The authors construct their index with daily and weekly activities in mind. But the wording of most items is inappropriate for annual or biannual activities like voting. I therefore construct a new set of items that mimic the intent of the Verplanken and Orbell index, but differs from it in the four ways noted at the start of the chapter. Several items were initially examined in a 2009 survey of a random sample of Minneapolis and St. Paul residents, with the final items included in the pre-election and post-election surveys of Minnesota registered voters conducted in 2010. In the next section, I discuss the construction of the voting habit strength index.

### **The Voting Habit Strength Index**

#### *Data*

The data for my analyses in Chapters 3, 4, and 5 come from two original surveys of Minnesota registered voters in conjunction with administrative information from state voter files. Specifically, I merge four data sources: pre-election survey responses, voter file information obtained prior to the election, post-election survey responses, and voter file information from after the election. These sources describe respondents' turnout over a six-year period, and psychological associations and experiences with voting during October and November 2010.

Respondents were recruited by telephone and then emailed the secure link to each of the online surveys.<sup>9</sup> I used the Minnesota voter file to construct the recruitment list, generating a random sample of Minnesota households with at least one registered voter. Recruitment calls were made in the last three weeks of October 2010. One registered voter in each household was randomly selected to be surveyed.<sup>10</sup> Besides registered voters who did not list a phone number and therefore could not be contacted,<sup>11</sup> I made no sample exclusions during recruitment. Callers were trained to invite those contacted to participate in an online survey about “social and political issues.” For the pre-election survey, individuals who agreed to participate were emailed the secure link to the online survey five days prior to the election. Emails were sent to 1,994 addresses with the pre-election survey link.<sup>12</sup> Individuals who did not complete the survey by the day before the election were emailed a reminder. 744 respondents completed the survey in entirety.<sup>13</sup> For the post-election survey, all respondents who started the first survey were emailed the secure link to this survey two weeks after the election. 625 respondents took the survey, 84% of those who completed the first survey.

I take numerous steps to ensure that the records across the four data sources are merged appropriately all respondents. First, the calling list is constructed from the voter

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<sup>9</sup> The University of Minnesota College of Liberal Arts Survey Services formatted the survey for web use, emailed the secure link to the survey, administered randomization in block design for the experiment described in Chapter 5, and recorded survey responses.

<sup>10</sup> If the selected person was not available, a second person from the household was randomly selected and invited to take the survey.

<sup>11</sup> Approximately 85% of registered voters provided a valid phone number.

<sup>12</sup> Approximately 300 addresses generated automatic replies indicating a nonexistent email address (the exact number may be slightly different due to the notification email being sent from an address not associated with the study and therefore not recorded). Some email addresses might also have been recorded incorrectly during the initial recruitment call, with no automatic replies sent.

<sup>13</sup> 786 completed all questions up to the habit items.

file obtained prior to the election. Because all respondents are drawn from this pre-election voter file, I can easily merge information from the version of the file obtained after the election (the basic identifying information rarely changes from before to after the election). This process leads to very few mismatches between the survey samples and voter files. The state is also reasonably effective at maintaining the same voter identification number for registrants who change names or addresses—allowing for few mismatches between the two voter files.

Nevertheless, state voter files are widely known to contain inaccuracies. As an initial way of preventing inaccuracies, callers were told to verbally instruct all potential respondents during the recruitment call to complete both the pre-election and post-election surveys themselves. The secure URL to the survey was sent by email to individuals who agreed to participate; and it is possible that two individuals from the same household or family might share an email account.<sup>14</sup> To prevent inaccuracies, I ask respondents for their first name and date of birth in both the pre-election and post-election surveys. Since the voter files also include names and date of birth, I can confirm that the correct individuals' information is merged across all four data sources—the pre-election survey, initial voter file, post-election survey, and final voter file. I discovered 12 mismatches.<sup>15</sup> An additional 22 respondents (3% of the remaining pre-election

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<sup>14</sup> Observation of callers revealed that potential respondents were usually given this instruction, but some callers were not. Nevertheless, this first step in obtaining data consistency appeared to be reasonably effective.

<sup>15</sup> Specifically, I discovered 5 mismatches between the post-election and pre-election surveys (i.e., people who took the pre-election survey, but whose spouse or acquaintance took the post-election survey). I exclude these respondents from this chapter's analysis. 7 pre-election survey respondents could not be matched to the initial voter file, presumably because someone other than the intended respondent completed the survey. Their voter file information was set to missing.

Because the voter file also lists individuals' addresses, date of birth, and other personal information—and because the mismatched person who took the survey was usually the spouse—

sample) could not be matched to one of the public voter files, presumably because of name changes or moves.

Overall, I consider this a highly successful rate of matching (for a discussion of difficulties and rates of matching survey respondents to public voting records see Lupia et al. 2011). Of course, even after the extensive effort to correctly match all respondents, there could still be a handful of mismatches. Such inconsistencies should primarily produce random measurement error<sup>16</sup> and only reduce the likelihood of finding significant associations, making for harder tests of the hypotheses.

For most of this chapter's analysis, I exclude several groups for theoretical reasons. I exclude the 34 respondents who already voted by absentee ballot at the time of the pre-election survey (administered 1 to 5 days before the election). Since most of my hypotheses relate to psychological changes as a result of voting, I must begin with a sample of individuals who had not yet voted. Absentee voters would have no baseline measures of the variables central to the theory. For that reason, I also exclude the 1 respondent who took the survey after polls opened, since I cannot verify that the respondent had not yet voted.<sup>17</sup> Finally, I exclude people from this chapter's voting analysis who reported in the pre-election survey that they moved to a different state—8

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in several cases I was able to merge the proper person's voter file data despite the mismatch. In a few cases, the individual appeared to change their last name (all female respondents) between the time I obtained the initial voter file and the pre-election survey, and was not caught by the state records updating system. I was able to use addresses and other personal information from the voter file to confirm that name-changers were indeed name-changers and not other individuals. In the end, only 7 pre-election respondents could not be matched to the initial voter file, as mentioned.

<sup>16</sup> Systematic measurement error might result if the excluded respondents are residence-movers, since movers vote at a lower rate. However, such error is inevitable in studies relying on administrative voter files. Movers are routinely more difficult to reach in phone, internet, and mail surveys.

<sup>17</sup> The survey closed the morning of the election, but this respondent started the survey after polls opened and before the survey closed.

respondents. These individuals did not have voter file information, and thus no validated voting history. Likewise, I exclude the 3 respondents in the post-election survey who reported that they voted in a state other than Minnesota for the 2010 election; validated voting information is unavailable for those respondents.

The pre-election and post-election survey samples closely resemble the Minnesota population of registered voters. Age, race, and ethnicity information for Minnesota registrants are available from the U.S. Census Bureau for comparisons. Table 1 displays the distributions by age, race, and ethnicity categories. The survey samples are both slightly younger than the population of Minnesota registered voters on average. Although the 65-to-74 age category proportions are similar, the 75-and-older percentage is smaller in my survey samples. The difference likely results from administering an internet-based survey. Respondents must have had an email address to complete the survey, and older individuals are less likely to have email access. Nevertheless, the other age categories are similar in proportion.<sup>18</sup> Having a similar age distribution is important for generalizing to the broader Minnesota and U.S. populations, given the strong association between age and habit strength. The survey and Minnesota distributions by race and ethnicity are also similar.<sup>19</sup>

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<sup>18</sup> The age distribution among Minnesota registrants is also extremely similar to the distribution among all U.S. registered voters according to U.S. Census Bureau data.

<sup>19</sup> The race categories in my survey are mutually exclusive. They are not in the Census data. Thus, the Hispanic percentage should be larger in the Census data. Nevertheless, the share of Hispanic respondents in the survey samples is still somewhat smaller than expected.

**Table 1. Distribution of Survey Respondents and Minnesota Registrants by Age, Race, and Ethnicity**

	Pre- Election R's	Post- Election R's	Minnesota Registrants <sup>a</sup>
<b>Age</b>			
18-24	8.4%	7.0%	8.4%
25-44	37.9%	36.3%	31.9%
45-64	41.5%	43.4%	39.4%
65-74	8.8%	10.1%	11.2%
75+	2.9%	3.1%	9.0%
<b>Race and Ethnicity<sup>b</sup></b>			
White Non-Hispanic	95.4%	96.4%	94.3%
Black	1.4%	1.1%	2.2%
Hispanic	0.4%	0.5%	1.6%
Asian	2.3%	2.0%	0.8%

<sup>a</sup> Data are from the U.S. Census Bureau, "Voting and Registration in the Election of November 2010 - Detailed Tables"

<sup>b</sup> The Census Hispanic category includes individuals reporting any race in combination with Hispanic ethnicity. The remaining three categories are mutually exclusive. For the pre-election and post-election samples, race/ethnicity categories are mutually exclusive. I exclude respondents not reporting race and reporting "other"--so category definitions more closely resemble Census definitions.

### *Item Descriptions*

12 survey items are used to construct the Voting Habit Strength Index. 10 of the 12 items are generated using pairs of branching questions in which respondents indicate their strength of agreement for a statement (i.e., "Voting is an important part of who I am as a person").<sup>20</sup> I combine responses for each pair of questions to create an 8-point scale indicating the strength of agreement. Items 11 and 12 are constructed using single, non-branching questions.

Four items are intended to measure the centrality of voting to one's identity.

While Verplanken and Orbell's SRHI includes only one explicit identity-related item, I

<sup>20</sup> For each statement, respondents are asked if they agree, disagree, or neither agree nor disagree. Based on the first response, a follow-up question asks how strongly the respondent agrees or disagrees ("slightly," "somewhat," or "strongly") or which way she leans ("towards agreeing" or "towards disagreeing").

use four since identity should be a more important indicator of habit strength for events that occur infrequently like voting. The four identity statements are (with the order they appear in the survey in parentheses):

1. Voting is an important part of who I am as a person. (12)
2. I feel uncomfortable if I do not vote in a presidential election. (3)
3. I feel uncomfortable if I do not vote in an election for governor. (4)
4. I consider myself a frequent voter. (7)

Item 1 is the most straightforward assessment of the importance of voting to respondents' self-concepts. Items 2 and 3 are similar to an SRHI item that asks respondents whether not doing an activity makes them "feel weird." When an activity is important to one's self-concept, one should feel not like one's usual self if the activity is not performed, upon encountering the usual behavioral context. The word "uncomfortable" effectively describes the feeling.<sup>21</sup> I do not include a version of this item for municipal elections.<sup>22</sup>

Item 4 is intended to measure the perception of oneself as a frequent voter. It avoids most of the methodological problems associated with using frequency-based measures discussed above, since it focuses on the self-concept ("I consider myself...") and not just the raw frequency of voting.<sup>23</sup>

To measure the automaticity of voting-related acts, I ask questions intended to assess the extent to which traveling to the polls, planning the voting trip, and making time

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<sup>21</sup> In pre-testing with a version that used the word "weird" instead, respondents often asked for clarification about the meaning of "weird."

<sup>22</sup> The VHSI is intended to measure the strength of voting habits for federal and state elections. Individuals who habitually vote in elections for national and state offices should also habitually vote in local elections. As initial evidence, Gerber et al. (2003) demonstrate the powerful influence of voting in the 1998 midterm election on voting in a 1999 municipal election. But the psychological properties of habits in federal and state voting may be distinct from the properties related to municipal voting, to the extent that individuals' consider local elections less "political" or markedly different in other ways. Nevertheless, future studies might adapt the index to incorporate voting in local elections.

<sup>23</sup> One prior critique of Verplanken and Orbell's index is that it includes two items that measure frequency of prior behavior. The items are: "behavior X is something I do frequently" and "behavior X is something I have been doing for a long time."

in one's schedule for voting are performed efficiently and with a lack of controllability, awareness, and intent. Six of the items follow the same branching format as the identity items. Those items are used to create 8-point scales reflecting respondents' strength of agreement with the base statement. Reverse-coded items and the order of presentation in the survey are noted in parentheses:

5. On Election Day, voting is difficult to fit into my daily routine. (5, reverse)
6. Traveling to my polling place on Election Day is something I can do automatically, without thinking at all. (6)
7. I sometimes forget to vote, even when I am planning on it. (8, reverse)
8. I vote without having to consciously plan it into my day. (9)
9. I would vote even if I were not very interested in the candidates and issues in the election. (10)
10. Even when I am planning on voting, I will skip it if I am too busy that day. (11, reverse)

The two remaining automaticity questions are asked without the branching format:

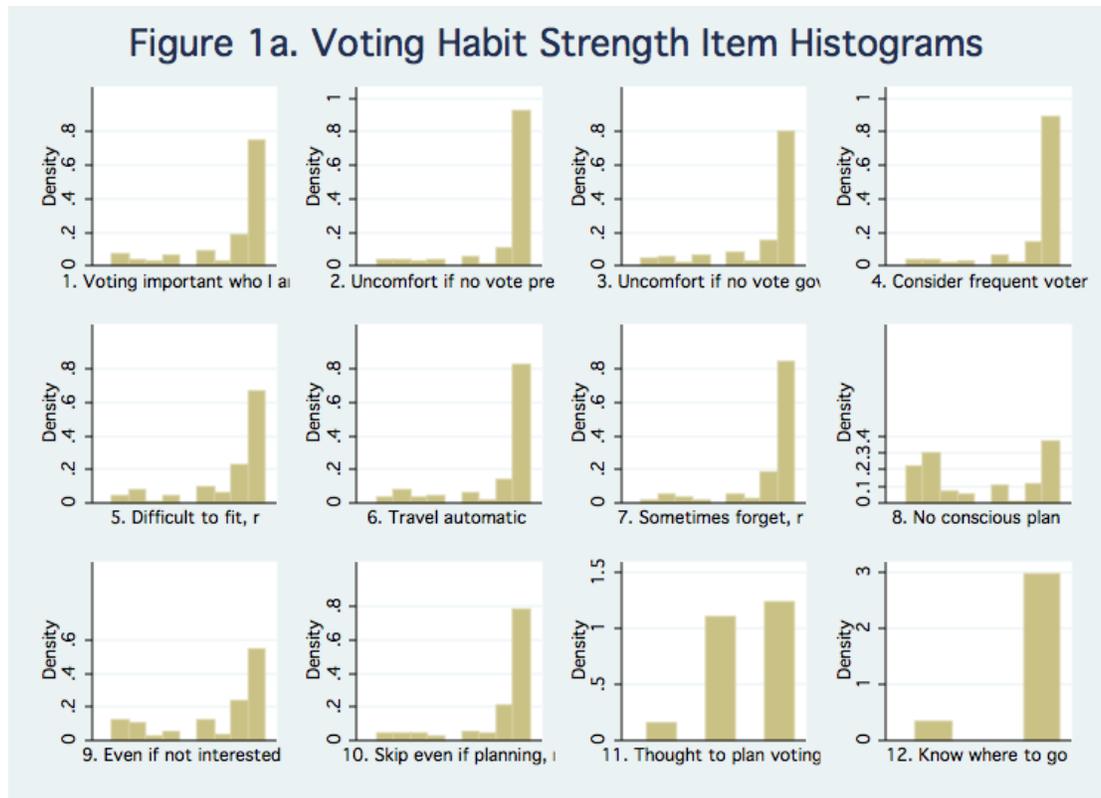
11. It takes some people a lot of thought to plan how and when they will get to the polls on Election Day. It takes other people only a little thought, and some do it without thinking at all. How about you? When you vote does it take... (1)
12. Off the top of your head, do you know exactly where to go to vote on Election Day, or would you need to ask someone or look up the address? (2)

Most of these items are designed to measure multiple aspects of automaticity (efficiency and lack of control, awareness, and intentionality). While most items are similar to SHRI items—and are intended to reflect the same underlying construct—they differ in two ways. As with the identity items, I avoid words that explicitly reference behavioral frequency, unlike the SHRI. I also select words that reflect the specific behavioral setting. Although voting almost always takes place at the same time (second Tuesday of November for federal general elections) and place (one's precinct polling location), I include words like “on Election Day” to emphasize this.

Table 2 displays the descriptive statistics for the items. As with subsequent analyses, I exclude individuals with missing values for any of the habit items. One notable feature of the twelve voting habit items is their significant skewness. Figure 1a illustrates the skewness more clearly with histograms of the individual items (see the next section for formal tests of univariate and multivariate normality).

**Table 2. Descriptive Statistics for Voting Habit Strength Index Items (N=729)**

	Mean	S.D.	Skewness	Kurtosis	Min.	Max.
1. Voting important part of who I am	6.7	2.1	-1.6	4.3	1	8
2. Feel uncomfortable if don't vote - pres.	7.2	1.8	-2.2	6.6	1	8
3. Feel uncomfortable if don't vote - gov.	6.9	2.0	-1.7	4.8	1	8
4. Consider myself frequent voter	7.1	1.8	-2.2	6.8	1	8
5. Difficult to fit voting into daily routine (rev)	6.6	2.1	-1.5	4.1	1	8
6. Travel to polls automatically without thinking	6.8	2.1	-1.6	4.3	1	8
7. Sometimes forget to vote when planning (rev)	7.1	1.7	-2.1	6.4	1	8
8. Vote without consciously planning day	4.4	2.8	0.1	1.3	1	8
9. Vote even if not interested in candidates/issues	6.0	2.5	-1.0	2.4	1	8
10. Skip voting if busy, even if planning (rev)	7.0	1.9	-1.9	5.6	1	8
11. No thought at all to plan how & when to vote	2.4	0.6	-0.6	2.4	1	3
12. Know exactly where to go to vote	0.9	0.3	-2.5	7.5	0	1



*Validating the VHSI: Confirmatory Factor Analysis*

I propose that the voting habit strength items reflect two distinct but related subconstructs. I first conduct a confirmatory factor analysis (CFA) to assess whether the proposed two-factor model fits the data better than a single-factor model. All models are estimated using asymptotic distribution free (ADF) estimation<sup>24</sup> in Stata 12. ADF estimation is more appropriate than maximum likelihood (ML) estimation given the severe non-normality of the data. ADF estimation has fewer distributional assumptions and is more appropriate with these data than ML estimation, which assumes a multivariate normal distribution. As illustrated by the histograms in Figure 1a, the twelve habit items are highly skewed. Separate Shapiro-Wilk tests indicate that each item is

<sup>24</sup> The ADF method is also known as generalized method of moments (GMM), a version of weighted least squares (WLS).

non-normally distributed ( $p < .001$  for each item besides item 11;  $p < .05$  for item 11<sup>25</sup>). A test for multivariate normality demonstrates that the voting habit items are not multivariate normal ( $p < .001$ ).<sup>26</sup> The skewness is largely understandable if voting is central to many individuals' identities, and the items do not sufficiently discriminate among those for whom voting is highly and very highly relevant to the self-concept.

The consequences of using ML estimation for this extent of non-normality are substantial. Monte Carlo simulations of empirical cases of skewness and kurtosis that approximate the voting habit items (approximate univariate skewness of -2 and univariate kurtosis of 4) demonstrate that any model fit indices and parameter estimates with maximum likelihood estimation will be significantly biased. In particular, Muthén and Kaplan (1985) perform simulations with several data situations, one closely resembling my habit items (see "Case 4": univariate skewness -2.03, univariate kurtosis 2.90). As in many other simulation studies, Muthén and Kaplan find that modest levels of skewness and kurtosis have negligible effects on parameter estimates and fit indices for structural equation models. But for Case 4 (and presumably these severely non-normal data), the chi-square statistic rejection frequency was 167% higher than for the multivariate normal case, and the standard error estimates were biased (also see Curran, West, and Finch 1996; Gao, Mokhtarian, and Johnston 2008). This suggests that for these data, ML estimation may produce considerably upwardly biased fit statistics and increase the likelihood of improperly rejecting model results.

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<sup>25</sup> Item 11 ("no thought to plan") is also not continuous, a separate SEM assumption violation. However, given that this item is measured on 3-point scale with a clearly continuous underlying dimension for respondents (degree of "thought to plan"), I treat it as continuous. Item 12 is also not continuous. Since these two items cannot be easily combined with others, and the remaining 10 items are measured on continuous 8-point scales, the violations should not noticeably influence the model fit statistics or estimation of other habit strength parameters.

<sup>26</sup> I use the Doornik-Hansen omnibus test, the default option for the *mvttest* command in Stata.

Since there are no obvious transformations to generate a multivariate normal set of voting habit items—a process that carries risks itself—there are two primary options. I can use (1) ADF estimation or (2) ML estimation with robust standard errors and robust, corrective chi-square statistics, such as the Satorra-Bentler scaled chi-square statistic. I opt to use the ADF method.<sup>27</sup> Given the substantial risks associated with ML estimation with extreme non-normality discussed above, the large number of cases in these data, and the relatively simple models being estimated, ADF estimation is optimal.

One of the more comprehensive simulation studies (Curran, West, and Finch 1996) concludes that the ADF test statistics are unbiased using sample sizes of 500 and more, especially for non-normal data and simpler models, despite a small loss of power in detecting misspecification. My data include 729 cases (after excluding cases with missing values for the habit items). I also estimate one-factor and two-factor models with 12 items and relatively simple covariance structures.<sup>28</sup> In other words, the common advice that test statistics with the ADF method are most appropriate with large samples of at least 1,000 to 1,500 cases does not appear to hold under these conditions. With severe non-normality, less complex models, and a sample size of over 700, ADF estimation should perform much better than ML estimation.<sup>29</sup> In future iterations, I will present the results using ML estimation with Satorra-Bentler chi-square statistics as a robustness

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<sup>27</sup> For all model specifications using these data, all coefficient estimates for factor loadings are significant, regardless of estimation method. So I focus the remaining discussion on bias in fit statistics.

<sup>28</sup> Models with reasonably high numbers of indicators per factor carry an even lower risk of bias (see Boomsma and Hoogland 2001). I use 8 items for the automaticity factor and 4 for identity.

<sup>29</sup> For further reviews of Monte Carlo simulation studies of simple models under severe non-normality, see Muthén and Kaplan (1992) who extend their earlier (1985) simulation findings for to incorporate these conditions.

check. I presume that the substantive conclusions are not noticeably altered by the decision to use ADF, but additional checks will be useful.

Table 3 displays the confirmatory factor analysis results for the proposed two-factor model proposed. The model has an acceptable fit to the data. All factors loadings are significant ( $p < .01$ ), suggesting that the automaticity and identity items reflect the underlying two subconstructs. The standardized loadings range from .20 to .73.<sup>30</sup> The root mean square error of approximation (RMSEA) is .059. RMSEA values below .07 or .08 typically indicate an acceptable fit to the data (Hu and Bentler 1999). Additionally, the 90% confidence interval for the upper and lower bounds of the RMSEA estimate includes .05 and the upper bound is below .07.<sup>31</sup> As an additional check of model fit, I present coefficient of determination (CD) estimates in Table 3. CD values range from 0 to 1, with higher values reflecting a better fit to the data. The CD value for the model—.88—indicates an acceptable fit, and that about 88% of the predicted variance is explained by fitted variance.<sup>32</sup> Overall, the results indicate that, by conventional standards, the two-factor model appears to fit the data reasonably well.

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<sup>30</sup> The mean of the standardized loadings is .493. ADF estimation may be somewhat deflating the magnitude of the loadings. Using ML estimation, the loadings range from .40 to .77, with a mean of .55. Although ML estimation will yield biased standard error estimates and fit statistics like the RMSEA and chi-square statistic—due to the substantial non-normality in the data—the magnitude of the coefficients should not be influenced by the non-normality.

<sup>31</sup> The 90% confidence interval for the upper and lower bounds of the RMSEA estimate essentially provides a 95% confidence interval around the test statistic (Stata “SEM” documentation). Although there is only about a 6% probability that the RMSEA estimate is less than .05, we can be confident it is below .10.

<sup>32</sup> CD resembles an R-squared statistic, in that it expresses the estimate of the overall explanatory power of the instrument, and assesses the difference between the fitted and predicted variance.

**Table 3. Loadings for Two-Factor CFA Model<sup>a</sup> for Voting Habit Strength (N=729)**

Identity Items		
1. Voting important part of who I am	0.66	--
2. Feel uncomfortable if don't vote - president	0.20	--
3. Feel uncomfortable if don't vote - governor	0.47	--
4. Consider myself frequent voter	0.73	--
Automaticity Items		
5. Difficult to fit voting into daily routine (rev)	--	0.55
6. Travel to polls automatically without thinking	--	0.64
7. Sometimes forget to vote when planning (rev)	--	0.36
8. Vote without consciously planning day	--	0.61
9. Vote even if not interested in candidates/issues	--	0.44
10. Skip voting if busy, even if planning (rev)	--	0.47
11. No thought at all to plan how & when to vote	--	0.49
12. Know exactly where to go to vote	--	0.28
Correlation between factors	0.706	
$\chi^2$ (df=52)	182.43	(p<.001)
RMSEA	0.059	
90% CI, lower bound	0.050	
90% CI, upper bound	0.068	
CD	0.888	

Notes: Entries are standardized loadings. Estimated using asymptotic distribution free (ADF) method. All loadings are significant (p<.01). CFA=confirmatory factor analysis; RMSEA=root mean square error of approximation; CD=coefficient of determination.

<sup>a</sup> Corresponds to the two-factor model with additional error covariances in Table 3. Two pairs of observed variances are allowed to vary: (a) "uncomfortable if don't vote for president" [2] and "uncomfortable if don't vote for governor" [3] and (b) "travel to polls automatically" [6] and "know where to go" [12].

Two pairs of errors are allowed to covary in the model. The two “feel uncomfortable if I do not vote” questions, corresponding to items [2] and [3], are worded virtually identically besides the words “governor” and “president.” Responses to these questions should be driven by related considerations due to their shared format. Additionally, items [6] and [12] both explicitly ask about the process of traveling to one’s polling location, and so responses to those questions should also be motivated by similar

concerns. The modification indices for the model without allowing these error covariances indicated that they would improve the fit. Nevertheless, the model fit is similar regardless of the choice of error covariance structure (see discussion of Table 4 below).

Previous habit scholars, while theorizing that both automaticity and identity components of habit strength exist, have only recently tested a two-factor model. Gardner, de Bruijn, and Lally (2012) construct additional identity items to examine the separate roles of identity and automaticity in habit using data on binge-drinking, and find that identity and automaticity items conform to two distinct but strongly related factors.<sup>33</sup> Verplanken and Orbell (2003) combine all automaticity and identity items in a single additive index after exploratory factor analysis revealed support for a one-factor solution. Verplanken and Orbell's EFA results could not support a two-factor solution based on identity and automaticity since they use only one explicit identity item. The greater number of identity items in the current study allows me to test a two-factor measurement model. Moreover, the identity dimension of habit strength should be more important in the domain of voting than daily and weekly activities, as emphasized in the previous chapter.

The results in Table 4 compare fit statistics for the one-factor and two-factor models. Although the models are nested, I cannot rely on formal chi-square difference tests to compare them because the ADF method does not produce log likelihood values. I instead present a range of descriptive fit indices. The indices demonstrate that the two-factor model presented above (with errors covarying between the two specified pairs of

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<sup>33</sup> Unlike the current study, Gardner et al. (2012) draw the conclusion that only the automaticity factor represents habit—rather than automaticity and identity, despite their correlation.

items) fits the data modestly better. Based on the RMSEA and SRMR, the two-factor model with additional error covariances performs best. The CD results indicate that the two-factor models fit better than both one-factor solutions.<sup>34</sup>

**Table 4. Model Fit Indices for One- and Two-Factor CFA Models for Voting Habit Strength**

	$\chi^2$	df		RMSEA	SRMR	CD
<b>One-Factor Model</b>						
Standard error covariances	231.47	55	p<.001	0.066	0.777	0.805
Additional error covariances <sup>a</sup>	194.93	53	p<.001	0.061	0.593	0.798
<b>Two-Factor Model</b>						
Standard error covariances	222.30	54	p<.001	0.065	0.685	0.902
Additional error covariances <sup>a</sup>	182.43	52	p<.001	0.059	0.526	0.888

Notes: CFA=confirmatory factor analysis; RMSEA=root mean square error of approximation; SRMR=standardized root mean square residual; CD=coefficient of determination.

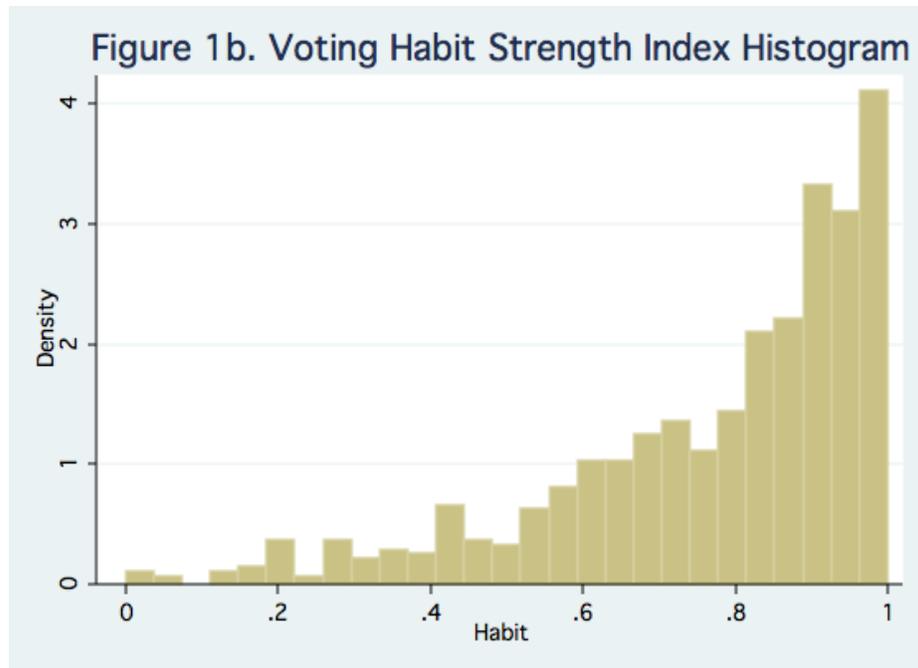
<sup>a</sup> Two pairs of observed variances are allowed to vary: (a) "uncomfortable if don't vote for president" [2] and "uncomfortable if don't vote for governor" [3] and (b) "travel to polls automatically" [6] and "know where to go" [12].

While the data support a two-factor solution, the two-factor model yields only a modestly improved fit over the one-factor model. RMSEA values for the primary two- and one-factor specifications are below .07 and .08, the conventional cutoff for good fit. Additionally, the two factors—representing automaticity in voting tasks and identity as a voter—are highly correlated (.71). While the correlation is not perfect, it suggests that treating voting habit strength as a unidimensional construct is appropriate and will yield similar results in most empirical analyses. The one-factor measurement strategy is also more convenient in most analyses. Testing many propositions in the dissertation requires

<sup>34</sup> While the comparative fit index (CFI) is sometimes examined in SEM analyses, ADF estimation is known to underestimate the CFI when: correlations between the items are low, and when there is extreme non-normality in the data (high kurtosis in particular). Both of these conditions hold for these data. In particular, correlations for some voting habit items (especially items across the two proposed factors) are as low as .20.

estimating the effects of voting habit strength on turnout. Turnout is measured using a binary indicator and SEM analysis does not support estimates of the effects of latent two-factor variables on binary indicators. Due to the highly correlated factors and practical methodological reasons discussed below, I rely on a unidimensional voting habit strength scale for much of the dissertation's remaining analysis. In the few situations in which I propose automaticity and identity have separate effects on behavior, I use the separate subscales.

To construct the habit strength index, I generate a factor score for the latent habit variable using the estimates for the one-factor model with additional covariances (i.e., the model corresponding to row 2 in Table 4). The resulting measure has less error than a simple additive sum of the 12 habit items. The habit strength factor score is nevertheless highly correlated with the index from a simple additive sum of the items ( $r=.976$ ). I rescale the habit score to range from 0 to 1, and use it for much of the chapter's remaining analysis. The mean habit score is .77, suggesting that the distribution is heavily skewed towards upper end of the habit index. Figure 1b presents a histogram of the final voting habit strength index.



*Further Content Validity Analysis: Frequency vs. Habit Strength*

The CFA results present strong initial evidence that the strength of voting habits is validly represented with indicators of automaticity and identity as a voter. But additional steps are necessary to confirm that the index measures the intended construct—that the index has high content validity. Perhaps it simply measures frequency of prior voting, in which case the VHSI would be largely unnecessary. Or the items may actually measure intentions to vote, albeit with a more extensive instrument than most surveys.

The VHSI is clearly capable of distinguishing habitual voters from frequent voters. It is distinct from measures of frequency of prior turnout. The correlation between habit strength and prior voting frequency in my sample is .486<sup>35</sup>—which is

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<sup>35</sup> Although there are several ways to measure prior voting frequency, this particular measure is a count of the times an individual voted in all primary and general elections from 2004 to 2009 (12

moderate, but far from perfect. There are some people who vote often but have not yet developed a strong habit, and some who have not yet voted much but already developed a strong habit. Table 5 displays the percentage distribution of respondents in my sample by category of prior voting frequency and habit strength.<sup>36</sup> I select the category cutoffs using quartile values of the two variables.<sup>37</sup> Most respondents (about two-thirds) fall into

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elections in total). I use validated turnout information from state administrative records and self-reported turnout information for the 2004, 2006, and 2008 general elections. (Self-reported turnout information is only available for those three elections.)

There are difficulties associated with both validated and self-reported turnout information. In particular, for respondents who moved from a different state, validated voting information is unavailable for the time they lived elsewhere. In the few analyses in the dissertation that require a prior voting frequency measure in which I can safely exclude the state-to-state movers without harming the generalizations, I do. I limit the sample to individuals who lived in the state for 10+ years in those situations, and rely on a raw count of validated prior voting from state records. The consequence of excluding state-to-state movers is that the association between prior frequency and habit will be slightly artificially inflated. (It will appear higher than it actually is because moving residences across state lines interrupts the habit formation process, and a sample full of residentially stable people develop voting habits after fewer repetitions.) But self-reported voting information can be incorporated to help generalize to the whole population. The correlation reported above ( $r=.486$ ) includes all respondents in order to describe the underlying association between frequency and habit strength more accurately. My inclusion of some self-reported voting is an attempt to reduce the enormous measurement error associated with using validated voting information for state-to-state movers (since those people do not have as much validated voting information available).

The consequences of these measurement decisions will become more apparent in the Chapter 4 models. Nevertheless, the simple correlation between habit and prior voting frequency can range from .423 to .517, depending on the measure of prior voting and group it is estimated for. The myriad complications associated with measuring prior voting frequency are another reasons that the VHSI measure of habit strength is preferable to a frequency-based one.

Additionally, I avoid using “percent eligible” voting measures in the dissertation whenever possible, since most of my hypotheses about habit relate to raw frequency of voting, not percent of eligible elections voted in. The difference between raw frequency and “percent eligible” measures is enormous. The differences arise primarily among young adults and state-to-state movers. For example, a 20 year-old who voted in only 1 election would have a “percent eligible voting” score of 100%, but a raw frequency measure of only 1. Since the 20 year-old has not had significant time to develop the habit of voting, her habit score is likely low. Measuring the association between “percent eligible” and habit strength would be quite inappropriate to test hypotheses about the effects of voting frequently for her, for other young people, and for state-to-state movers.

<sup>36</sup> I display this distribution only for respondents who lived in Minnesota for 10 or more years, and use raw validated voting information only.

<sup>37</sup> The categories do not have equal numbers of respondents for two reasons. First, the prior frequency variable is not perfectly continuous. For some prior voting values, there were many people (e.g., lots who had voted 3 times), who had to all go in one category or another. And the

the highest habit and highest frequency groups, or the lowest habit and lowest frequency groups. Including respondents in the darker shaded cells—moderate-high habit scores and moderate-low frequency, and moderate-low habit and moderate-high frequency—76% of respondents have habit scores that would be predicted solely by voting frequency. But there are many discrepancies. The Kendall tau-b value summarizing the association between frequency and habit strength in Table 5 is .33, indicating a strong or moderately strong association.

**Table 5. Percentage Distribution of Respondents by Voting Habit Strength and Prior Voting Category\***

Voting Freq.	Voting Habit Strength				Total
	Lowest		Highest		
Lowest	10.7	4.6	3.0	3.9	22.2
	6.5	6.9	5.5	5.0	23.9
	3.7	6.8	5.5	5.6	21.6
Highest	0.7	7.2	10.8	13.7	32.4
Total	21.6	25.5	24.8	28.2	100.0

\*For respondents living in the state 10 years or longer (N=695). Frequency of prior voting is a raw validated count of voting in the 12 prior elections, from 2004-2009.

Why are there some discrepancies between habit strength and voting frequency? I further validate the VHSI by demonstrating that the discrepancies result from dynamics proposed in the theory of habitual voting. Specifically, factors like the context in which people vote help explain why habitual voters differ from frequent voters. The VHSI appears to capture the differences in these data. Based on the theory of habitual voting, I would expect people in the top-right cells in Table 5 to have developed stronger habits

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quartile values are calculated for the entire sample, rather than just residents for 10+ years, but are displayed here for the 10+ year residents only. There is some discrepancy between the two samples.

more quickly than people in the top-left cells (the infrequent non-habitual voters) for several reasons. I expect that, relative to the infrequent non-habitual voters, these people consistently vote in a more stable context (the same place without moving residences), vote for the winning candidate at a higher rate, and expect more strongly to experience psychological rewards from voting.<sup>38</sup> More rewarding voting experiences would lead individuals to develop a stronger habit after fewer trips to the polls.

I examine most of these differences extensively in Chapter 4 using panel survey data. I briefly consider the propositions about stable voting contexts here. The “quick habit-formers” (the high-habit-low-frequency individuals in the top-right four cells) are, in fact, more residentially stable: 3.2 residential stability scores<sup>39</sup> vs. 2.6 for the non-voting weak-habit respondents, who are in the four top-left cells. The difference is statistically significant ( $p < .01$ ).<sup>40</sup> If the VHSI were not measuring the strength of voting habits, it is difficult to imagine that physical voting context would so strongly influence VHSI scores. In general, the VHSI is not simply a measure of frequency, and some of the discrepancies between VHSI and voting frequency are explained by differences in physical voting context, as predicted.

The relationship between habit strength and voting frequency should also be asymptotic—such that the strength of a person’s voting habit grows most quickly after

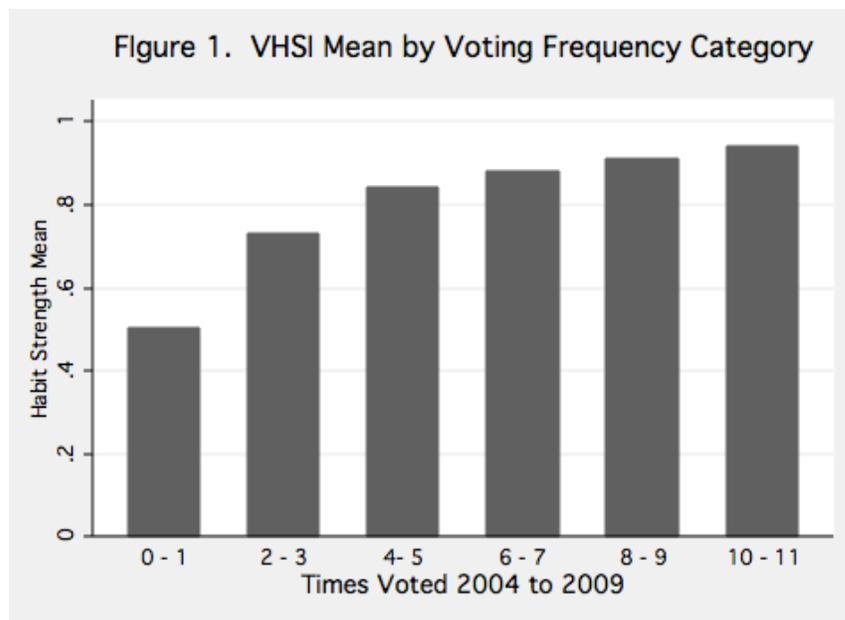
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<sup>38</sup> They may expect to experience more rewards due to factors like personality, genetics, and having parents and friends who value voting more.

<sup>39</sup> Residential stability is assessed by asking respondents how long they have lived at their current residence. This would indicate the time since their last precinct polling place change. Response options include less than 1 year, between 1 and 5 years, 5 and 10 years, and more than 10 years.

<sup>40</sup> In Chapter 4, I estimate similar effects using many controls. The quick habit-formers also vote at the same time of day each election at a much higher rate (28% vs. 16% for the non-voting weak-habit respondents,  $p < .01$ ). In fact, the quick habit-formers have the highest rate of same-time voting of the four quadrants in Table 4. Although I do not find in Chapter 4 that same-time voting increases the likelihood of future voting significantly, perhaps same-time voting has a more indirect influence on habit strength unobservable in these data.

the first handful of times voting, and much more slowly after that. Lally et al. (2011) find strong evidence for the asymptotic relationship using data on daily and weekly behaviors. If the habit formation process for voting resembles that for other activities, then the non-linearity should hold in these data as well. As an initial test, one might examine the cells in Table 5. There should be fewer respondents in the bottom-left cells than the top-right cells. I do not actually find that to be the case. However, there are certainly fewer respondents in the single bottom-left-most cell (.7% of the sample) than the top-right-most cell (3.9% of the sample). And the cell cutoffs are selected essentially arbitrarily—based roughly on quartile values—and not equally spaced. To examine the asymptotic relationship more closely, I present a bar chart displaying mean VHSI values by category of prior voting frequency in the sample. As Figure 1 demonstrates, such a relationship exists.



## **Response Time Validation Analysis**

To further assess the validity of the VHSI, I examine response times to the habit strength questions. Faster responses to survey questions are thought to indicate responses that are more accessible in memory (Bassili 1993; Johnson 2004). Some debate exists about whether response times measure any constructs besides accessibility. Perhaps slow responses reflect a respondent's ambivalence about an issue rather than a lack of accessibility (Bassili 1996). However, many researchers in psychology and political science accept that faster responses reflect greater accessibility of attitudes. The more immediately relevant question for this chapter's analysis is, what does greater accessibility mean for voting habits? Bassili (1993) proposes, and demonstrates empirically, that faster responses to questions about behaviors indicate that the respondent is more likely to carry out the behavior in real-life settings. Bassili (1993) found that the response time to an intent-to-vote question was a better predictor of actual voting than explicit answers about voting intentions. As such, response latencies appear to validly measure readiness to act in real-life settings. In the language of automaticity, latencies should measure greater automaticity of the behavior in question. I therefore use them to examine whether habitual voters—as measured by the VHSI—are more prone to vote automatically. If so, this would be a strong validation of the VHSI. I also use response latencies to further validate the automaticity subconstruct of the VHSI.

Specifically, I examine whether habitual voters, as measured by the VHSI, are more likely to respond quickly to the twelve VHSI questions. I generate a mean response time for all of the habit questions.<sup>41</sup> I also create a variable containing the mean response

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<sup>41</sup> The habit questions are branching. And while all respondents receive the same initial question in each branching set, they might receive different follow-up questions depending on the first

time for just the eight automaticity questions for all respondents. Relying on multiple measures has been shown to reduce random measurement error for numerous psychological constructs, including political preferences (e.g., Ansolabehere, Rodden, and Snyder 2008). Prior to generating the mean for all the question times, I eliminate outliers by setting all times above 30 seconds to missing. I elect to not transform the data, despite some non-normality. There are numerous possible transformations with response time data, which tend to be quite skewed. But I avoid any such transformations because the composite measure is not noticeably skewed (skewness=.25), despite some kurtosis (4.2), and interpreting the model results with transformed dependent variable is less straightforward. And since I seek to validate the VHSI rather than generate point estimates, I am less concerned about the distributional assumption violations.

My survey data come from a web-based survey, which provides relatively accurate measures of response times compared to phone and in-person surveys. Measurement error in response time data is ubiquitous though (see Johnson 2004), including in web-based surveys. Individuals might step away from the computer, or become distracted for many reasons. But most of such error is randomly distributed across respondents. Even where systematic individual differences in response times exist, I am primarily concerned with systematic differences that are correlated with voting habit strength. Otherwise, any such measurement error will only dilute the observed associations between habit and response times, deflating standard errors, and making it harder to detect associations and validate the VHSI.

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response. I therefore only include response times for the initial sets of questions, to avoid any differences in response times resulting from selected answers.

Age is an important individual level determinant of response times to control for. Numerous studies suggest that older individuals respond more slowly to survey questions (see Martin 2004). Since age is highly correlated with habit strength, the analysis must control for it. The time of day one completes the survey is also relevant. Although there is no universal relationship between time of day and mental alertness, one would expect that as the day and night progress, respondents become less mentally alert. I do not know how voting habit strength may be associated with tendencies to take surveys at night or mental alertness. But to avoid underfitting the model, I control for the time the survey is completed. Additionally, mental state and mood should be highly related to response times. I rely on a battery of questions to assess the extent of mental states experienced over the past two weeks, and use stepwise regression to determine which moods/states are most related to response times on the habit questions. They are: calm/peacefulness, happiness, and being worn out. I control for these to avoid the risk that such moods are experienced more by habitual voters and thus bias the results.

Table 6 presents the results of OLS regression models of mean response times for the voting habit strength questions. The coefficient for the habit index (-2.25) indicates that respondents with high habit strength scores respond significantly more quickly to the habit questions. Specifically, moving from the lowest to highest habit strength value is associated with a 2.25-second faster response to the average VHSI question. The magnitude of this effect is enormous, considering that the average response time is 8.7 seconds. The results in the second column show that higher scores on the automaticity subscale—a simple additive index of the eight VHSI automaticity items—are strongly associated with lower response times for those items. The results strongly suggest that

the VHSI automaticity subscale identifies individuals with a higher readiness to travel to the polls and cast a ballot (more automaticity in voting-related tasks). Importantly, automaticity subscale scores are more strongly associated with response times to the automaticity questions than identity subscale scores. The magnitude of the automaticity subscale coefficient (-1.21) is much larger than the identity subscale coefficient (-.89), and is statistically significant at the .05 level.

**Table 6. OLS Estimates for Mean Response Time (RT) for Habit Strength Questions**

	RT All Habit Qs		RT Automat. Qs	
	Coef.	S.E.	Coef.	S.E.
Habit strength <sup>a</sup>	-2.25	(0.43) ***	--	--
Habit - Automaticity	--	--	-1.21	(0.56) **
Habit - Identity	--	--	-0.89	(0.51) *
Age	0.05	(0.01) ***	0.05	(0.01) ***
Hour of day (1-24)	0.04	(0.02) **	0.03	(0.02) *
Calm & peaceful	0.19	(0.14)	0.16	(0.16)
Happy	0.11	(0.14)	0.04	(0.16)
Worn out (reverse)	0.00	(0.11)	0.00	(0.13)
Intercept	6.31	(0.61) ***	7.73	(0.76) ***
N	715		674	
Model F	16.86 (p<.01)		10.9 (p<.01)	
Adj. R <sup>2</sup>	0.118		0.093	

Notes: Responses times above 30 seconds are set to missing.

\* p<.1, \*\*p<.05, \*\*\*p<.01, using two-tailed tests

<sup>a</sup> Habit strength is measured using the VHSI (weighted with factor loadings from measurement model), and ranges from 0 to 1.

Neither the habit-automaticity nor the habit-identity scales are weighted. They are simple additive scales, and rescaled to range from 0 to 1.

### *Action Identification*

In research on action identification theory, Vallacher and Wegner (1987, 1989) find that people who are experienced at an action tend to identify it on a higher, more abstract level; and people inexperienced with an action identify it on a lower, more

concrete level. For example, a person can identify eating as chewing and swallowing (a low level identification) or getting nutrition and reducing hunger (a high level identifications) (see Vallacher 1987, 6). The authors suggest that people for whom an action is habitual identify the action on a higher level. The same should be true of habitual *voters*. The 2010 survey respondents were asked two questions about how they identify voting:

1. In general, when you vote, which of the following do you think about more?  
Travelling to the polls / Influencing the election
2. Which of these do you think about more when you vote? Being civically responsible / Filling out the ballot

The two abstract responses are “influencing the election” and “being civically responsible.” I combine responses to create an index with three possible values representing the level of abstractness in identifying voting. The habit strength scale should be correlated with the abstractness index. Indeed, the correlation between the VHSI and abstractness index is .336.

Additionally, I expect that the VHSI should *not* be associated with two variables: party identification and ideology. Habitual voters should be both Democrats and Republicans, and liberals and conservatives. The correlation between VHSI and party identification is -.001, and the correlation between VHSI and ideology is -.07, which are weak associations, as expected.

### *Predictive Validity*

Does the VHSI predict what it is intended to predict? Among other causal influences, I theorize that the habit strength should strongly influence future turnout. Chapter 4 contains the bulk of the future turnout analyses. However, I now assess the strength of association between the VHSI and validated turnout in the 2010 election.

Voting habit strength should significantly influence turnout when controlling for other influences on turnout—including psychological factors potentially related to habit. I measure validated turnout by locating respondents in state administrative records after the election.<sup>42</sup> The pre-election survey with the VHSI questions is administered several days before the election.

The first column in Table 7 demonstrates that the VHSI strongly influences turnout. In fact, the predictions from the model show that moving from the lowest to the highest value of habit strength is associated with an 80% increase in the likelihood of voting (from 17.1% likelihood of voting to 97.7% likelihood). By any standard, this effect is large. The model with just the VHSI produces a pseudo R-squared value of .21. This is far greater than the R-squared values of most other fully specified turnout models in the literature—including Rosenstone and Hansen’s (1995, Appendix D-1) model using American National Election Survey data on presidential elections with 30 independent variables.

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<sup>42</sup> Virtually all respondents were successfully matched to administrative records because the original recruitment list comes from state records itself. For this reason I can conveniently match respondents using their state voter identification number even when they move within the state or change names.

**Table 7. Probit Estimates for Validated Turnout in 2010 General Election**

	Coef.	S.E.		Coef.	S.E.		Coef.	S.E.	
Voting habit strength	2.95	(0.29)	***	2.01	(0.33)	***	1.19	(0.44)	***
Prior voting frequency	--	--		0.38	(0.06)	***	0.36	(0.07)	***
Political efficacy	--	--		--	--		0.02	(0.02)	
Strength of party identification	--	--		--	--		-0.13	(0.11)	
Interest in politics - general	--	--		--	--		-0.18	(0.18)	
Interest in candidates/issues	--	--		--	--		0.64	(0.16)	***
Perceive election as close	--	--		--	--		0.14	(0.06)	**
Prefer Dem. candidate (Dayton)	--	--		--	--		0.23	(0.22)	
Prefer Repub. candidate (Emmer)	--	--		--	--		0.46	(0.24)	**
Strength of candidate preference	--	--		--	--		0.05	(0.14)	
Angry about candidates/issues	--	--		--	--		0.32	(0.08)	***
Enthusiastic about cand./issues	--	--		--	--		-0.22	(0.11)	**
Intercept	-0.95	(0.21)	***	-1.41	(0.23)	***	-3.34	(0.50)	***
N	705			705			663		
LR $\chi^2$	113.3			162.7			204.8		
Pseudo R <sup>2</sup>	0.210			0.302			0.426		

\* p<.1, \*\*p<.05, \*\*\*p<.01, using two-tailed tests

The results in the second column demonstrate that habit strength, as measured by the VHSI, powerfully influences turnout independent of prior voting frequency. Prior voting frequency is measured as the number of times the person voted in the previous 12 elections. Of course, this model specification is not theoretically sound, since I propose elsewhere that prior voting frequency influences habit strength, which in turn influences future voting. But the results are useful to demonstrate that the VHSI is measuring a construct distinct from prior voting. This effect is particularly large considering that the prior voting frequency coefficient captures all other time-invariant effects on previous voting not captured in the model (which includes numerous other effects).

Additionally, the effects of the VHSI on turnout are distinct from those of political efficacy, emotions about the candidates and issues in the upcoming election, general political interest, interest in the candidates and election issues, perceived closeness of the election, strength of candidate support, and strength of party

identification. As in prior studies (Denny and Doyle 2008), political interest and efficacy are highly related in these data, and there is significant multicollinearity associated with their effects on turnout. The coefficient for efficacy is significant after removing general political interest from the model. Other variables in the model are highly related, leading to multicollinearity. However, I am not concerned with estimating the coefficients with precision in this section—only in demonstrating the independent effects of habit strength on future turnout to establish predictive validity. Overall, the VHSI predicts what it is intended to predict.

## **Discussion**

In this chapter, I systematically define voting habits and rule out alternative strategies for measuring them. Rather than measure them using indicators of prior voting frequency or physical voting context, I directly measure the psychological properties of voting habits. I demonstrate that the concept is reasonably well structured around the two dimensions of automaticity in voting behaviors and identity as a voter. Nevertheless, a unidimensional index of voting habit strength is most convenient and appropriate for empirical research on turnout, and I therefore construct a 12-item index using survey self-reports that incorporates the measurement properties of the items. Taking numerous steps to validate the resulting index, I conclude that it measures the concept validly.

Since I validate the VHSI using a single data set of Minnesota registered voters, future studies might administer the habit questions in samples in other states. Minnesota is routinely one of the states with the highest voting rates in the U.S. Yet the state has many non-habitual voters, and the VHSI appears to classify them appropriately.<sup>43</sup> There

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<sup>43</sup> See Table 1 of Chapter 4 for a comparison of my survey samples with the Minnesota population by age, race, and ethnicity.

is little reason to believe the non-habitual voters in Minnesota are so different from those in other states that the VHSI would not effectively identify them.

As a conceptually clear, valid, and empirically convenient measure of voting habits, the VHSI gives turnout researchers a common—and powerful—tool to study the behavior of “habitual voters.” Political scientists frequently discuss and offer propositions about habitual voters, often without a definition of habitual voting. The discipline still has no widely accepted conceptual definition or empirical measure of the strength of voting habits—and few systematic investigations of the mechanisms, psychological or otherwise, that may cause the strong effect of past turnout on future turnout. Aldrich et al. (2011) undertake the first theoretically driven measurement of habit strength, but rely on a frequency-based measure.<sup>44</sup>

The predictive validity analysis in this chapter suggests that the concept of habit is quite important to understanding turnout. The effect of the index on turnout overwhelms almost all other effects common in existing turnout models, and suggests that researchers can account for a far larger share of variance in turnout by incorporating the VHSI. In Chapter 4, I examine why and how habit strength shapes voting outcomes.

This chapter’s analyses have other important implications for the study of habitual voting, psychology theories of habit formation, and political science theories of turnout. For testing hypotheses about habitual voting in the dissertation, developing a valid and reliable measure of habit strength is critical. In particular, an indicator of voting frequency does not allow me to effectively estimate the over-time causal relationships between turnout and habit strength—including the effects of turnout on habit and habit on

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<sup>44</sup> Aldrich et al. (2011) and Valentino et al. (2009) provide the only empirical tests of propositions about habitual voting processes to date. Both analyses measure habit with indicators of prior voting frequency, or prior frequency in conjunction with residential stability.

turnout. A direct measure of habit is necessary for many empirical tests about over-time voting.

With regard to psychology theories of habit formation, I present one of the first studies to measure the identity component of habit strength. Numerous scholars assume that identity is important in habit formation, and that the SRHI measures, to some extent, the identity component of habit. But this assumption has gone largely untested across hundreds of studies using the SRHI (but see Gardner et al. 2012), in part because the one or two identity items in the SRHI are not sufficient to measure the construct by themselves. Having four identity items allows me to explicitly test hypotheses about the role of identity in habit formation. I determine that the identity items can be validly combined in a separate subscale, but rely on the unidimensional scale for the bulk of the dissertation's analyses.

With regard to political science research on turnout, separate identity and automaticity subscales would allow more useful analyses even than in psychology. Psychologists have long been familiar with measures related to the self-concept. To my knowledge there are no existing empirical analyses of individuals' identities as voters in political science. Public opinion scholars have long conceptualized party identification as being related to the self-concept. It is not unreasonable to think that if one's identity as a partisan strongly influences a constellation of political attitudes (as with party identification), then one's identity as a voter might powerfully shape voting behavior. Political scientists might also adapt the measure for analyses of other forms of political behavior, to measure the strength of political protest habits, political discussion habits, or

campaign donation habits. Such an analysis might be particularly useful if political fundraisers could target individuals with the strongest identities as donors.

## **Chapter 4**

### **Testing the Theory of Habitual Voting**

Bob Schieffer, the moderator of the final presidential debate in 2012, concluded the debate by quoting his mother: “Go vote. It makes you feel big and strong.” I interpret this advice to mean that voting makes one feel more efficacious, and that it incentivizes future turnout. The notion that voting influences one’s psychological disposition, and that one’s psychological disposition also affects voting, is the basis of the dissertation. In Chapter 2, I offer a more comprehensive theory of turnout focusing on the formation of voting habits over time and its many reciprocal causal relationships. Unlike other theories of turnout, most factors in it are treated as endogenous to several others. Voting should generate psychological rewards such as a greater sense of political efficacy and more positive mood. In turn, a greater sense of political efficacy and more positive mental associations between turnout and mood should increase one’s likelihood of voting again. These propositions appreciate that voting is not an activity one decides whether to do or not depending on whims of the moment. It is powerfully shaped by whether and how one voted in the past.

This perspective on the causes of political behaviors differs from prior research. Instead of simply analyzing whether donating money to a campaign is influenced by X, Y, and Z, researchers might also consider how donating money to a campaign influences one’s identity as a campaign donor, automaticity in donating, and emotional state. The effects of those psychological changes on one’s propensity to donate again might then be estimated. In other words, political behaviors besides turnout might be studied as habits. Researchers might examine protest attendance, initiation of political discussions, reading

political news, and running for local office as habits. This research strategy will dead-end, however, if a particular condition is present. If the variable to be treated as endogenous to political behavior and also exogenous to future behavior—such as political efficacy or emotional associations with the behavior—is mostly stable over the time, then it is best simply to consider the variable as exogenous. While this condition might hold for general interest in politics (Prior 2010) or other variables in political science research, it does not hold for emotional states, personal identities, and automaticity linked to behaviors. Almost by definition, emotional states change often. This is what might make them powerful influences on behavior (Hull 1943).

For example, well-known research by Langer and Rodin (1976) found that individuals living in assisted living situations could experience quite significant changes in their sense of efficacy over time; there is significant within-person variation in efficacy over time. Individuals in the study who were given more freedom to make decisions—including control over caring for a plant, and staff communications that emphasized their personal responsibility—had significantly improved alertness and life satisfaction over time. Greater efficacy also caused residents to participate in nursing home activities at a higher rate, the authors find. In many domains of political behavior, there might also be significant within-person variation in efficacy and its reciprocal relationship with behavior.

The goal of this chapter's analysis is to test the propositions in the theory of habitual voting. Specifically, I examine 13 hypotheses derived from the theory of habitual voting. I first test hypotheses related to the causes of habit strength, then factors influencing how individuals experience the psychological rewards of voting, and

conclude by testing hypotheses about all factors directly and indirectly influencing turnout and the future likelihood of turning out.

In doing so, the analysis systematically examines the reciprocal relationships among voting and several key psychological constructs over time for the first time. Among other findings, the results demonstrate that the act of voting by itself significantly lower the anxiety associated with going to the polls, especially for individuals with weak habits. Voting also produces small but significant changes in positive mood associated with voting, and increases internal political efficacy over time. These changes occur most noticeably for individuals with weak voting habits, as the theory dictates (but the results speak to some pride, mood, and efficacy-related effects for habitual voters too). Finally, the results confirm one of the primary hypothesized changes in the habit formation: voting in a stable context strengthens one's identity as a voter and makes voting acts more automatic.

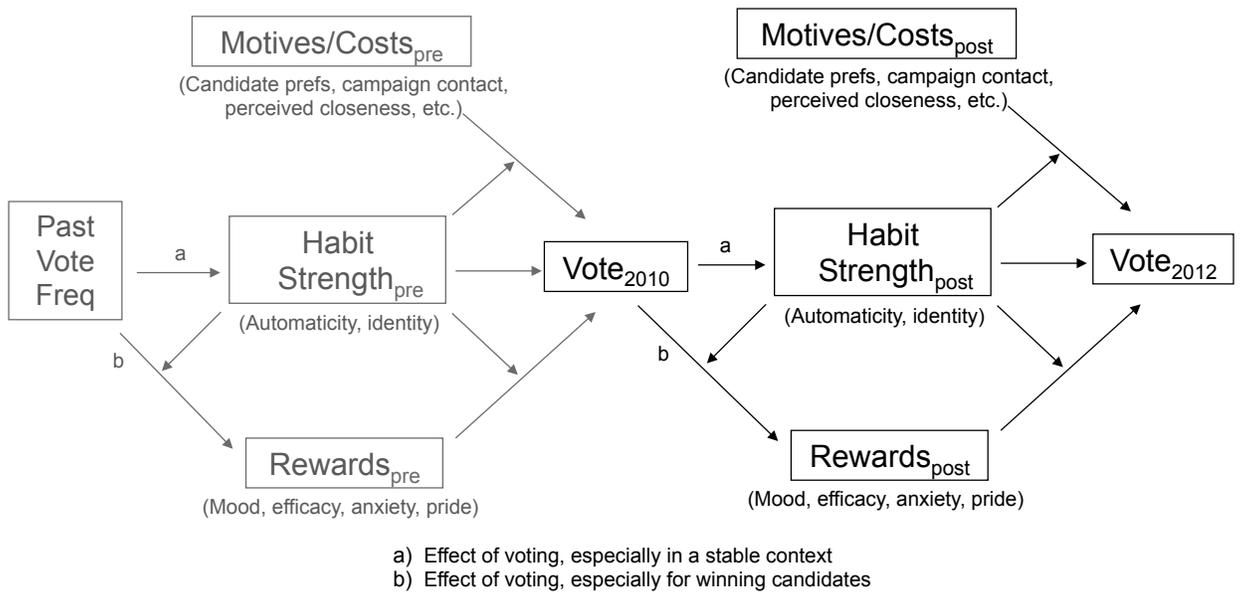
In the analysis, I utilize original panel survey data from respondents interviewed immediately prior to the election and then two weeks after the election. Since the indicators are available from the same respondents immediately before and after the election, I am less hindered by problems of unobserved heterogeneity common in prior turnout studies. In general, I demonstrate that to understand turnout, scholars must examine the re-occurring causal relationships among variables in the theory of habitual voting.

### **Repetitious Voting and Habit Strength**

The data for this chapter's analysis are described in detail in Chapter 3. The primary structural relationships hypothesized based on the theory of habitual voting are

summarized in Chapter 2, Figure 4. To better illustrate how those relationships are estimated using the data in this chapter, I present the model with labels specific to my data in Figure 1. The boxes are arrayed in the hypothesized chronological order of causation: frequency of voting in the period from 2004 to 2009 occurs first (“past vote freq”); the variables measured with the pre-election data occur next; voting or not voting in person in the general election on November 2, 2010 then occurs; and the post-election variables are measured approximately two weeks after the election.<sup>1</sup> The relationships proposed here should repeat continually in the future, and begin when a person first becomes eligible to vote.

**Figure 1. Structural Model of Habitual Voting**



I derive 13 specific hypotheses from the theory outlined in Chapter 2 that I test in this chapter. The first set of hypotheses concerns path *a*, on the relationships between prior voting and habit strength. (I examine the stability of the voting context in the following section). They can be summarized as follows: voting frequently strengthens

<sup>1</sup> The “pre” subscript indicates variables measured using the pre-election data, and the “post” subscript indicates variables measured using the post-election data.

the habit of voting, particularly for individuals with weak voting habits (H1). More specifically:

H1a: Each additional vote increases a person's voting habit strength.

H1b: Each additional vote increases a person's voting habit strength more for individuals with weak habits than strong habits.

Although I propose in H1b that the effects of voting on habit strength are not constant across levels of habit strength, I have no expectations about the precise functional form that summarizes that relationship. I nevertheless suspect that it is generally asymptotic. And given the centrality of these hypotheses for the overall theory, evidence disconfirming them would be particularly damning for it. Evidence supporting these proposed causal relationships would provide particularly strong support for the theory.

I first examine whether voting causes one's voting habit to strengthen (H1a). As noted in Chapter 3, voting frequently is associated with stronger voting habits using bivariate correlations. The correlation in my pre-election sample is .49.<sup>2</sup> And regressing voting habit strength on voting history reveals that repetitious voting is strongly related to habit strength, as measured by the VHSI. Each additional election of voting (including primaries) is associated with a .05 increase in habit strength (with habit strength values ranging from 0 to 1). The coefficient for voting history in this simple single-variable linear model is significant ( $p < .01$ ). Turnout in the November 2010 election is also associated with higher habit strength scores. Specifically, turnout in the election is associated with .27 higher VHSI scores on average ( $p < .01$ ). Controlling for any number of costs and motives of voting (e.g., political interest, enthusiasm and anger for candidates and issues, free time, perceived closeness) as measured in my data set does not

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<sup>2</sup> The correlation is between frequency of prior voting in the past 12 elections (using validated voting, and self-reported voting for the presidential elections) and the VHSI.

lead the coefficient to drop below conventional levels of significance—or even get close to dropping below conventional significance levels. In other words, going to the polls by itself appears to cause voting habits to strengthen from this initial evidence.

But the effect of prior voting might be spurious. Perhaps personality characteristics like extraversion, emotional instability, or impulse control are actually responsible for the creation of voting habits—or factors like political interest, education, or other types of political participation like attending protests. Perhaps politically interested people develop stronger voting identities and automaticity in voting acts regardless of their actual voting patterns. If such non-voting factors are correlated with both turnout and habit strength, then excluding them from the models estimated above would produce biased estimates of prior voting. This is highly unlikely given that the habit questions specifically ask about getting to the polls, which non-voters (regardless of personality characteristics, political interest, education, etc.) would be less familiar with. And results from Chapter 3 demonstrate that the VHSI does in fact appear to measure voting habit strength effectively, and is distinct from political interest, efficacy, education, and other constructs. Nevertheless, to more convincingly demonstrate that frequent voting strengthens habits, numerous controls are needed for the modeling strategy. But even using cross-sectional data with numerous controls would not provide particularly convincing empirical evidence.

Alternatively, I might take advantage of the panel design of the 2010 surveys—and the fact that so little time elapsed between the surveys—to gain some leverage on the methodological problem. I measure the strength of voting habits immediately prior to the election, and then two weeks after the election. Any changes in habit strength should be

primarily the result of voting or not voting. My empirical strategy in this section is to determine whether turnout in the November 2, 2010 election is associated with increasing VHSI scores. By modeling over-time changes in habit strength, I do not need to explicitly control for factors correlated with both likelihood of voting and post-election habit strength—since accounting for baseline, pre-election habit strength accomplishes this. Of course, I should control for anything besides turnout that might influence the likelihood of habit strength changing in the period between the surveys, including factors that might influence the *rate of change* (i.e., personality factors that might facilitate quicker or slower strengthening of voting habits). And I should include controls for other political events that occurred in that time period. This design allows me to more effectively assess the effects of voting on habit strength.

Specifically, I regress change in voting habit strength ( $habit_{post} - habit_{pre}$ ) on November turnout, baseline habit strength ( $habit_{pre}$ ), an interaction between turnout and baseline habit strength ( $habit_{pre}$ ), and several controls. Because the habit variables are skewed—towards high values—there is a ceiling on the effect of turnout on habit change. Individuals with the very highest VHSI scores will not display any change, even if they vote. Thus, I include an interaction between turnout and baseline habit strength. Also, because I hypothesize that habits strengthen most quickly after the first few trips to the polls, individuals with low VHSI scores should see the largest change. The effects of voting on habit strength should plateau at high levels of prior voting frequency, especially given the ceiling effects. To account for such non-linearity in the effects of turnout, I estimate ordered probit models with interactions between turnout and baseline habit.<sup>3</sup> To

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<sup>3</sup> I rely on probit and ordered probit models in the bulk of the dissertation's models that use categorical dependent variables—rather than logit and ordered logit models. Despite the

estimate these models, I construct categorical versions of voting habit strength.<sup>4</sup> I exclude the “know where to go” item when constructing the habit variables for this analysis because it would be almost perfectly correlated with turnout for respondents in the post-election survey who voted in November,<sup>5</sup> and use simple additive scales rather than weighting the items using factor loadings from the measurement models in Chapter 3. The correlation between the usual pre-election VHSI and this version of the VHSI using non-weighted pre-election survey responses is .97. Thus, I am comfortable using these variables as valid measures of habit strength.

Additionally, I include controls for the key psychological rewards of voting: mood, emotions associated with going to the polls, and internal political efficacy.<sup>6</sup> These might influence the rate of habit strength change over time, as suggested above. I also include dummy variables as controls indicating whether respondents received an “actions” or “rewards” imagery message during the survey.<sup>7</sup> Since these messages are designed to influence turnout—and thus also to influence habit strength—and since

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somewhat more straightforward interpretation of coefficients in logit models, probit models are more appropriate because the distributions of most variables in my data have thinner tails.

<sup>4</sup> I create categories based on cutoffs at .1 intervals. Since only 1 respondent was in the lowest value category, I combine the bottom two categories, creating 9 total categories.

<sup>5</sup> The question asking respondents if they know exactly where to go to vote on Election Day was not asked in the post-election survey. To make the pre- and post-election habit variables comparable, I exclude it from the construction of both. When designing the survey, I expected that all respondents who voted would respond “yes” to this question—since it was asked only 2 weeks after the election. The index would thus inappropriately capture some effects of 2010 voting on future voting and future likelihood of voting. While this is true, including the item would have allowed for some sensitivity tests, and for me to confirm this proposition. Nevertheless, the “know where to go” item is routinely one of the two items with the lowest standardized loadings. Excluding it should have little effect on VHSI scores, especially because in the non-weighted VHSI it ranges from 0 to 1, while most other habit variables range from 0 to 7.

<sup>6</sup> I discuss the measurement of these in the following sections.

<sup>7</sup> Respondents were randomly assigned to receive one of three versions of the survey. Two versions contained imagery mobilization messages that I analyze the effects of in Chapter 5.

respondents received them between the time when  $habit_{pre}$  and  $habit_{post}$  were measured, I control for them.

Table 2 displays the model results. The dependent variable ranges from -3 to 6, indicating the extent to which the habit score decreased or increased from late October to mid November (from the pre-election to post-election survey). The score decreased for 14 percent of the sample, stayed the same for 45 percent, and increased for 40 percent of the sample. Figure 1 displays the predicted probabilities of habit strength increasing at each baseline habit level, for voters and non-voters.<sup>8</sup> The results demonstrate clearly that voting once on November 2, 2010 is associated with large and significant increases in voting habit strength. This directly supports H1a. The increase is significantly greater than for non-voters at all but the very highest habit strength level, which supports H1b. In Table 2, the negative sign on the coefficient for the interaction term ( $voted \times habit_{pre}$ ) also reflects this: going to the polls strengthens habits far more for individuals in the early stages of habit formation. Finally, the evidence supports the general proposition

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<sup>8</sup> For ease of illustration and interpretation, the predictions in Figure 1 are generated using a stripped-down model. This model collapses the dependent variable into three categories: -1, habit strength decreases; 0 habit strength does not change; 1, habit strength increases. I also exclude the “rewards” variables from this model, since I would need to select values for each of the variables when generating predictions. And selecting any values—even the median or mean values—carries improper assumptions: the rewards are endogenous to habit strength; so, the rewards will never be stable across values of baseline habit strength. Non-habitual voters tend to have lower values of the rewards.

Thus, my choices are either to set the rewards variables to their mean value for each habit strength level for the predictions, or exclude them from the model. And, because this model fits the data almost as well as the 9-category DV model with all controls, I use this stripped down model to generate the predictions. I cannot conduct formal tests to compare the fit of the two models, since they use different dependent variables and are thus not nested. But a likelihood ratio test comparing the fit of the model with and without rewards variables suggests that the combined effects of these variables is not zero ( $\chi^2=16.14$ ,  $df=9$ ,  $p<.01$ ). This is good news for some hypotheses in the theory of habitual voting. But not as good evidence that a stripped down model is appropriate for predictions. Nevertheless, because the collapsed dependent variable actually produces a slightly better fit, the overall fit of the stripped down model (collapsed dependent variable with no rewards variables) vs. the full model in Table 2 is only somewhat worse. The pseudo  $R^2$  value is .170, versus .185 for the full model.

that increases in habit strength plateau, as with an asymptotic relationship, the more one votes in a stable context.<sup>9</sup> The plateau is part due to individuals with strong habits approaching the highest (ceiling) values.

**Table 2. Ordered Probit Estimates for Change in Voting Habit Strength<sup>a</sup>**

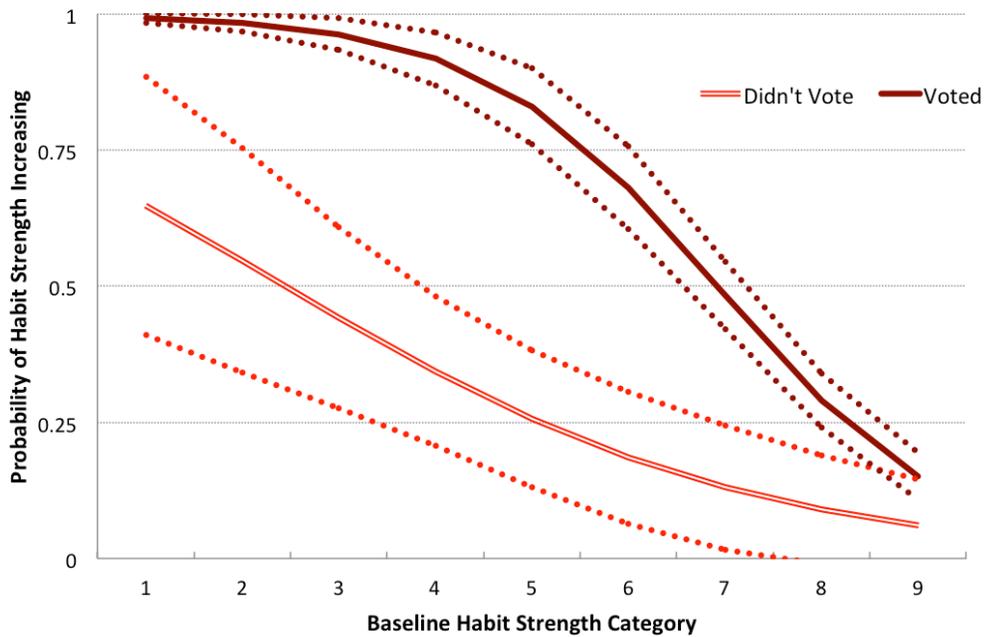
	Coef.	S.E.
Voted Nov. 2, 2010	2.36	(0.38) ***
Habit strength <sub>t-1</sub>	-3.20	(0.64) ***
Voted x Habit strength <sub>t-1</sub>	-1.47	(0.66) **
Mood <sub>t-1</sub>	0.40	(0.37)
Internal political efficacy <sub>t-1</sub>	0.56	(0.26) **
Anxiety linked to polls <sub>t-1</sub>	-0.04	(0.24)
Pride linked to polls <sub>t-1</sub>	0.64	(0.21) ***
"Actions" imagery message	0.44	(0.13) ***
"Rewards" imagery message	0.31	(0.13) **
$\tau_1$	-3.39	(0.52)
$\tau_2$	-2.59	(0.41)
$\tau_3$	-1.44	(0.39)
$\tau_4$	0.18	(0.38)
$\tau_5$	1.45	(0.39)
$\tau_6$	2.38	(0.41)
$\tau_7$	3.30	(0.46)
$\tau_8$	4.45	(0.60)
N	433	
LR $\chi^2$ (df=9)	232.34	
Pseudo R <sup>2</sup>	0.185	

Notes: \*\*\*p<.01, \*\*p<.05, \*p<.1, using two-tailed tests

<sup>a</sup> Change in habit strength is from late Oct. to mid Nov. Habit strength in this model is a raw sum of VHSI items, rather than the weighted sum using factor loadings, and excludes "know where to go" item. I convert pre- and post-election habit strength to categorical variables using .1 cutoffs, to estimate an ordered probit model. All final variables are rescaled to range from 0 to 1.

<sup>9</sup> I examine the effect of the stability of the voting context on habit strength in the following section. But for now this evidence supports the general proposition.

**Figure 2. Predicted Probability of Habit Strength Increasing after the Election, by Turnout and Baseline Habit Strength Category**



Notes: Dotted lines indicate 95% confidence intervals. Turnout is for the 2010 election. Changes in habit strength are assessed from late October to mid November (from pre-election to post-election survey).

More specifically, individuals with the weakest baseline voting habits (the bottom baseline habit category) have a 99 percent predicted probability of their habit strength score increasing after voting. And individuals who voted in the bottom five habit strength categories all have a probability of habit strength increasing above 80 percent. (These five categories represent individuals with VHSI scores between 0 and .5, on a 0-to-1 scale). Non-voters also saw their habit scores increase some over time. But this was most likely due to social desirability when responding to habit questions in the post-election survey. In sum, going to the polls is associated with far greater predicted increases in habit strength. And this effect is concentrated among individuals with weak to moderate voting habits. When habitual voters go to the polls, they experience some

strengthening—but far less than for non-habitual voters. These results directly support hypotheses 1a and 1b, providing strong initial support for the theory. Given the centrality of these relationships to the theory, it might be reasonably questioned without such evidence. The results also further validate that the VHSI measures what it is intended to measure.

### **Voting Context and Habit Strength**

As numerous studies have demonstrated using data on daily and weekly actions, performing an action in a stable context over time helps it become habitual (see Wood, Tam, and Witt 2005; Wood and Neal 2007). Political scientists have documented a similar phenomenon related to voting: moving residences (i.e., disrupting the physical voting context) decreases the likelihood of voting in the future. But studies have to examine the relationship between voting and the voting context in sufficient depth. We do not yet understand which groups of voters are influenced most by residential moving, and precisely why their voting likelihood is reduced. In general, I hypothesize that voting frequently, especially in a stable location, strengthens the habit of voting (H2). I offer several more specific propositions:

H2a: Each additional vote that occurs while living at the same residence strengthens one's voting habit (more than voting after one moves residences).

H2b: Each additional vote that occurs at the same time on Election Day as the last time voting *does not* strengthen one's voting habit (more than voting at a different time of day).

H2c: Voting at the same residence strengthens one's voting habit *more for individuals with weak habits than strong habits*.

H2d: Voting at the same residence increases the automaticity component of habit strength more than the identity component of habit strength.

H2e: Frequency of prior voting is a much stronger predictor of habit strength than physical context stability.

In H2b, I do not propose that the temporal context does not matter for voting—only that the time of day fails to matter. There is little overall variation in the temporal context for voting. Primary elections aside, people usually only have the opportunity to vote on the first Tuesday of November, and only at certain times that day. This makes voting different from donating blood, and other activities that can become habitual<sup>10</sup> but can occur at a variety of times of the year, week, and day. My expectations are not extremely strong with H2b, but this logic suggests that the one piece of temporal context that does not vary—the time of day—should matter little.

Physical voting contexts also do not vary dramatically, since people tend not to change precinct polling location often. However, reasonable variation in the physical context exists as a result of moving residences—either within a city or state, or across state lines. As noted in Chapter 2, movers are far less likely to vote. This stylized fact has been documented in many political science studies. However, prior researchers have not shown how this phenomenon relates to prior voting frequency, psychological experiences resulting from voting, the over-time formation of voting habits, or many other factors known to influence turnout. As such, this stylized fact is of little use to turnout scholars, campaign strategists, or policymakers. We simply know that individuals who move are less likely to vote in the next election.

How exactly does voting in a stable context strengthen voting habits? And what mechanisms are responsible for the turnout-depressing effects of moving? I address these

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<sup>10</sup> For initial evidence supporting the habitual nature of blood donation, including the psychological rewards of blood donation, see Ferguson et al. (2012) and Ferguson and Bibby (2002).

in testing H2c and H2d. Voting consistently at the same time and place should make voting-related acts more automatic (i.e., more efficient, and less guided by conscious control and intentionality), and thus more likely to occur in the future. Specifically, I hypothesize that having a stable voting context strengthens the automaticity in voting more than it strengthens the identity component of habit strength (H2d). Conversely, moving residences and polling locations makes the Election Day actions (including planning the trip to the polls and fitting it into the daily routine) less automatic. Movers are less familiar with how to get to their polling place, must consciously think about changing their daily routine more to incorporate voting, and must devote more thought and effort to planning the trip to the polls. Voting should be far less efficient for them. In general, I believe that moving residences “resets,” or at least stunts, the development of automaticity in voting but does not influence one’s identity as a voter as much.

In H2c, I hypothesize that voting in a stable context strengthens habits more for infrequent voters than frequent voters. (And conversely, disrupting the context should weaken habits more for infrequent voters.) Frequent voters should be able to overcome the turnout-depressing effects of moving disruptions more easily because voting is already more automatic and ingrained into their self-concepts. Finally, in H2e, I hypothesize that voting frequently is a far stronger predictor of voting habit strength (and, indirectly, of future turnout) than the stability of the voting context. Given that the context is already relatively stable for voting (usually the first Tuesday in November), frequency should be the primary driver of habit strengthening.

### *Temporal vs. Physical Context*

I first examine the separate effects of temporal context and physical context on voting habit formation to test H2a and H2b. To measure physical voting context, I ask respondents in the pre-election survey how long they had lived at their current residence.<sup>11,12</sup> Responses are moderately correlated with VHSI scores ( $r=.34$ ) and turnout in 2010 ( $r=.28$ ). In the pre- and post-election surveys, I include one question to measure the temporal voting context: “We’re interested in the time of day that people vote. Do you vote at the same time of day each election you vote in, or do you sometimes vote at different times?” 23 percent of respondents in the pre-election survey responded “yes,” and 28 percent responded “yes” in the post-election survey. By themselves, neither voting time item is strongly related to habit strength or voting in 2010 ( $r<.05$  for all bivariate correlations). Thus, using bivariate correlations, residential stability appears to matter for the strength of voting habits—but temporal stability appears not to matter. This initially supports H2a and H2b.

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<sup>11</sup> Response options for both questions are less than 1 year, between 1 and 5 years, between 5 and 10 years, and more than 10 years.

<sup>12</sup> I also ask respondents how long they have lived in Minnesota. However, I rely on the specific residence question to measure context for three reasons. The effects of length of Minnesota residence are largely captured by responses to the more specific residence question (since a person cannot move states without moving residences); there is very little variation in Minnesota residence (89 percent of the sample has lived in the state for 10 or more years); and Minnesota residence should be correlated with errors in validated voting. Regarding the third reason, it is likely that most measurement error in validated voting is due to out-of-state movers who do not have validated voting information for all elections. Using the Minnesota residence item in any models with validated voting would produce systematic measurement error in the measurement of physical context. Additionally, swapping the specific residence context variable for an additive index of Minnesota residence and specific home residence does not alter any substantive conclusions in this section. The index behaves almost identically to the specific home residence item, including when it is interacted with prior voting frequency.

**Table 3. OLS Estimates for Effects of Stable Voting Location and Time on Voting Habit Strength**

	Stable Location		Stable Time		Stable Location & Time (Pre)	Stable Location & Time (Pre+Post)
Stable context <sup>a</sup>	0.10 (0.02) ***	0.26 (0.04) ***	0.02 (0.01)	0.01 (0.03)	0.26 (0.05) ***	0.28 (0.06) ***
Prior voting frequency <sup>b</sup>	0.29 (0.04) ***	0.66 (0.09) ***	0.34 (0.04) ***	0.34 (0.04) ***	0.57 (0.09) ***	0.56 (0.09) ***
Frequency x context	-- --	-0.50 (0.11) ***	-- --	0.01 (0.09)	-0.47 (0.13) ***	-0.53 (0.15) ***
Intercept	0.20 (0.03) ***	0.11 (0.04) ***	0.24 (0.03) ***	0.24 (0.03) ***	0.14 (0.04) ***	0.16 (0.04) ***
N	660	660	657	657	657	519
Model F	78.09	72.91	72.32	63.18	71.3	52.45
Adj R <sup>2</sup>	0.450	0.466	0.432	0.431	0.462	0.443
Root MSE	0.154	0.152	0.155	0.155	0.151	0.147

Notes: Entries are coefficients with standard errors in parentheses. \*\*\*p<.01, \*\*p<.05, \*p<.10, using two-tailed tests

All models control for rewards of voting (mood, internal efficacy, anxiety and pride linked to voting) and general interest in politics.

All variables use data from before the November election, and are rescaled to range from 0 to 1.

<sup>a</sup> Stable context is defined in models 1 & 2 using location/residence, models 2 & 3 using voting time of day, models 4 & 5 using location & voting time (from pre-election survey), and models 6 & 7 using location & voting time (pre-election+post-election values).  
<sup>b</sup> Number of times voted in prior 12 elections (from 2004-2009)

I also conduct a multivariate analysis of the effects of the voting context on habit strength. This first allows me to provide a better test of H2a (that voting in a stable location strengthens voting habits) and H2b (that voting in a stable temporal setting does not strengthen habits). I can control for possible confounding factors, and for frequency of prior voting, in case the bivariate associations are masking heterogeneity across prior voting levels. I control for the rewards of voting and for political interest—since these factors should be simultaneously associated with habit strength, frequency of prior voting, and (potentially) context stability.

Most importantly, a multivariate analysis allows me to examine the *interactive* effect of prior voting and voting context on habit strength. Although the psychology literature on habits clearly finds that the interactive effect is primarily responsible for strengthening habits, political scientists tend to examine “length of residence” variables in isolation (and their effects on turnout only). H2a and H2b both propose that voting repeatedly in the same context strengthens voting habits—not that simply living in a residence for a long time by itself, or voting at the same time of day by itself, strengthens habits.

In the models in the first and second columns, I measure voting context as “stable location” (length of time in current residence). In the model in column 2, the coefficient for the interaction is significant, indicating that the effect of residential stability on habit strength varies by frequency of prior voting. I examine the substantive interpretation of this effect in detail in the following section. But for now it is worth noting that the interactive effect is significant—and that voting frequently in the same location is

associated with higher habit strength scores on average than just voting frequently. This is strong support for H2a.

In the column 4 model, the coefficient for the interaction is not significant, indicating that the effect of temporal stability (voting at the same time in each election) on habit strength does not vary by prior voting frequency. The interaction is not quite as important for testing H2b, since the temporal stability question specifically asks about voting. But in the column 3 model, the temporal stability coefficient is also not significant. Additionally, both of the “stable location” models fit the data substantially better than the “stable time” models. The former have higher F statistics, higher adjusted  $R^2$  values, and slightly lower root mean squared errors. These results further support the conclusion that voting in the same location over time—but not voting at the same time of day on Election Day—helps promote voting habits, supporting H2b.

There is likely some error in how I measure temporal voting stability in models 3 and 4. Optimally, I would like to determine the exact time that each respondent voted in each election—rather than asking for a summary evaluation for all elections. Such summary evaluations are less accurate because of individuals’ poor memory for events in past years. However, even with some measurement error, I would expect to observe at least a moderate association between habit strength and temporal stability using this variable if an underlying association existed. No coefficients for the temporal stability variables come close to reaching conventional significance levels. I therefore take the lack of association as further evidence that temporal stability matters less for voting habit formation.

In models 5 and 6, I attempt to overcome potential error in the measurement of context stability in order to provide a fairer test of the relative effects of temporal and physical context on habit strength. If the underlying construct is “stability of voting context,” then measuring it simply with temporal stability is not sufficient. It may be associated with habits to some degree, but the variable does not reflect the full domain of content. In model 5, I use an additive index of stability in *both* the time and location of voting. Random measurement error should also be reduced by combining multiple items. This “stable location and time (pre-election)” model fits the data well. But the fit is very similar to “stable location” models (1 and 2). In fact, based on the fit statistics displayed, I conclude that the “stable location” models have a somewhat better fit. Incorporating the time of voting into the context stability variable actually explains *less* variation in habit strength.

In model 6, I measure voting context by combining two temporal stability variables (one from the pre-election and one from the post-election survey) and the location stability variable in a single additive index. Since the election occurred just two weeks before the post-election survey was administered, respondents’ assessments about the time of day of voting should be more accurate—at least for those respondents who went to the polls. This should reduce random measurement error resulting from memory problems. The models appear to perform about as well as those in the first two columns (models 1 and 2). However, the similarity is partly a result of using a smaller sample (only post-election survey respondents). When models 1 and 2 are estimated with the same restricted sample, the root mean squared error is similarly low (.147) and adjusted R-squared is slightly higher (.454).

I conclude again that a stable location—living at the same residence and having the same precinct location for many years—is strongly related to voting habit formation (supporting H2a). Voting at the same time of day each election appears to be unrelated to habit formation (supporting H2b). I suspect that temporal stability is far more consequential for habit formation with daily and weekly activities than with voting, partly since there is far less variation in voting times. One can usually only vote from 7 a.m. to 8 p.m. And of course one can vote only on designated Election Days. Since temporal stability does not appear to explain voting habit strength above and beyond location stability, I use the “stable location” variable as a measure of voting context stability in the remainder of the dissertation. I also prefer to use the most simple, straightforward measurement approach among those producing models with similar fit. And the single “location stability” variable is simplest. In the next section, I explain the interaction between frequency of prior voting and context stability, and its broader implications for the theory of habitual voting.

### *Frequent vs. Infrequent Voting*

How does the stability of one’s voting context interact with the frequency of one’s voting to influence habit strength and future voting? Political science research has tended to focus on the two effects separately, and not as interactions, as noted above. Voting *while living at the same residence* should strengthen the habit—more than voting frequently by itself or living in the same location for many years by itself. In this section, I test H2c, to determine whether and why a stable context strengthens voting habits more for non-habitual voters than for habitual voters.

The model in column 2 of Table 3 contains the estimates of interest for testing these hypotheses. I reprint the model in Table 4 below, this time including the estimates for all the control variables. (I reference the control variable coefficients here and elsewhere in the dissertation.) The model fits the data very well. Frequency of prior voting and residential stability appear to account for a large share of the variation in voting habit strength.<sup>13</sup>

**Table 4. OLS Estimates for the Effects of Voting Context and Voting Frequency on Habit Strength**

	Coef.	S.E.	
Prior voting frequency <sup>a</sup>	0.66	(0.09)	***
Stable context <sup>b</sup>	0.26	(0.04)	***
Frequency x context	-0.50	(0.11)	***
Internal efficacy	0.13	(0.04)	***
Mood	0.16	(0.04)	***
Anxiety linked to polls	-0.05	(0.03)	*
Pride linked to polls	0.19	(0.02)	***
General pol. Interest	0.12	(0.03)	***
Intercept	0.11	(0.04)	***
N	660		
Model F	72.91		
Adj R <sup>2</sup>	0.466		
Root MSE	0.152		

Notes: \*\*\*p<.01, \*\*p<.05, \*p<.01, using two-tailed tests

All variables use data from before the November election (t-1), and are rescaled to range from 0 to 1.

<sup>a</sup> Number of times voted in prior 12 elections (from '04-'09)

<sup>b</sup> Residential stability

More importantly, the coefficient for the interaction term is significant at the .01 level. And the coefficient is negative. This indicates that context strengthens habits less

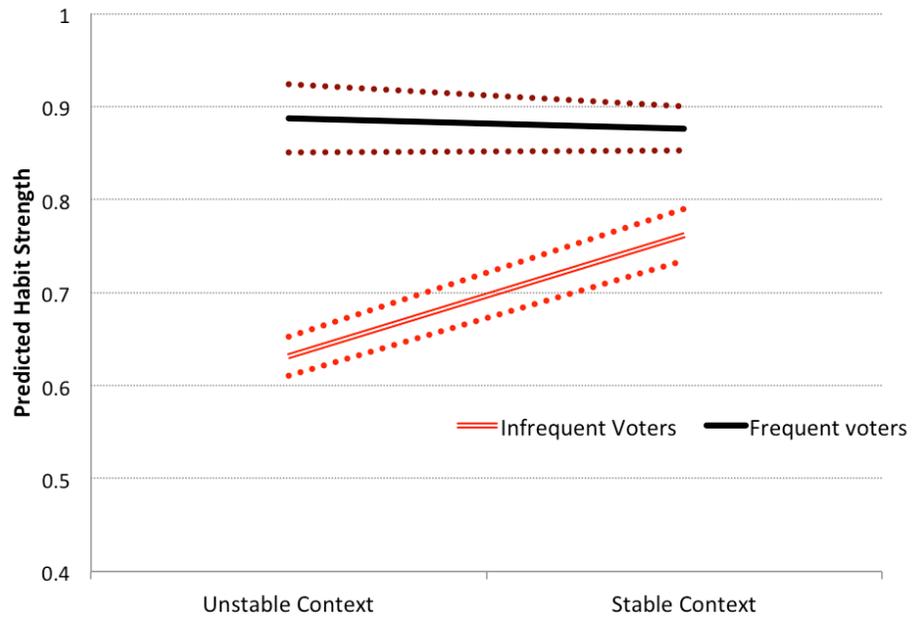
<sup>13</sup> Note that the model does not include baseline (pre-election) habit strength as an independent variable. So the explained variation is all unique to factors entirely separate from the habit construct.

for frequent voters than for infrequent voters. I display the predicted probabilities in Figure 2.<sup>14</sup> Context is associated with habit strength *exclusively for individuals without much voting experience*. For infrequent voters, having a stable voting location is associated with a .13 increase in habit strength. The estimated increase is statistically significant, and the magnitude is reasonably large. However, for frequent voters, context stability has no measurable effect on habit strength. Being in a stable location is actually associated with a slight decrease in habit strength—but the effect is not significant. This evidence strongly supports hypothesis 2c. Frequent voters' habits are apparently immune to the disruption of moving polling locations, presumably because voting is already more automatic and ingrained into their self-concepts. Even frequent voters who move in the previous year have quite high habit scores (a .9 value on average).

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<sup>14</sup> For calculating predicted probabilities, infrequent voting and context stability are defined using 10<sup>th</sup> and 90<sup>th</sup> percentile values. This translates to 2 times voting (in the years from 2004 to 2009) for infrequent voters, 6 times voting for frequent voters, living in the current residence from 1-5 years for unstable context, and living in residence for 10 or more years for stable context.

**Figure 2. Predicted Habit Strength, by Stability of Voting Context and Prior Voting Frequency**



Notes: Dotted lines indicate 95% confidence intervals. Predictions are estimated using 10th and 90th percentile values of context stability and prior frequency.

The predictions also strongly support hypothesis 2e. Frequent voting is far more strongly associated with increases in habit strength than context stability. As noted above, having more residential stability is associated with a .13 increase in habit strength for infrequent voters, and no significant increase for frequent voters. But frequent voting is always associated with large and significant increases in habit strength: a .26 predicted increase for individuals with long-term stable residences and a .11 increase for individuals who moved more recently. If you could do one of two things to strengthen your voting habit—either vote each election, or just live in the same place for many years—you would find that over time, doing the first would make you a habitual voter much more quickly.

The results have other enormous theoretical implications. To date, Aldrich et al. (2011) provide the only theoretically motivated analysis of voting habits that explicitly draws on psychology research on habitual behaviors. Their study is vital in bringing attention to this literature, which already has a reasonable grasp on the dynamics of habit formation for daily and weekly habits. And Aldrich et al. are perhaps the first to suggest an interaction between voting history and context in establishing voting habits. But I have noted in Chapter 3 that the authors' strategy for measuring and assessing the effects of "habit" produces enormous error. They do not measure habits directly, the consequences of which I discussed at length in Chapter 3 and also below. And all of the methodological problems of unobserved heterogeneity that plague estimates of "habit" in past research also plague the authors' estimates. They assess the effects of "habit" using indicators of prior voting frequency and residential stability. The coefficients for past voting, especially in models of future voting, will capture numerous effects besides those of habit. Of course, Aldrich et al. control for some of these. But for the most recent years of their analysis, they also only use turnout information from two prior elections to construct the prior frequency variable. It can hardly be said that individuals who voted in the past two presidential elections validly represent "frequent voters." Turnout in most of the models is also self-reported, generating further error.

Finally, and most importantly for the current chapter, Aldrich et al. do not examine how context stability influences habits *separately* for frequent and infrequent voters. They cannot—because they do not measure habit strength directly. Because the interaction between frequency and context *is* their measure of habit, they assume that only frequent voters living at the same residence for many years are habitual voters.

Frequent voters who sometimes move cannot be habitual voters by this definition. And they cannot examine if some infrequent voters develop the habit faster, some develop it slower, or why.

Fortunately, I measure habit strength directly in the dissertation and examine the differential effects for frequent and infrequent empirically. The results of my habit strength models so far show that the Aldrich et al. measurement of habit does not correctly classify frequent voters who move residences. From Figure 2 it is clear that frequent voters have just as strong habits whether they move or live in the same context year after year. Context strengthens voting habits, but does so almost exclusively for infrequent voters. This finding also helps explain some results of Brady and McNulty (2011). The authors show that when precinct polling locations change (a voting context disruption resulting from a semi-random administrative change in Los Angeles County), turnout is reduced for young voters, who are more likely to be infrequent voters. But the turnout of older voters (who are more likely to be frequent voters) are relatively unaffected. Brady and McNulty find that older voters vote at essentially the same rate as before the context change, largely because a greater share cast absentee ballots. Context changes appear to disrupt turnout primarily for infrequent voters.

I provide one final empirical analysis to highlight the differential effects of context for frequent and infrequent voters. I have so far estimated models of habit strength—since the theory clearly dictates that voting in a stable context strengthens voting habits, which then subsequently influence future voting. So habit strength should mediate the effects of past voting on future voting (in addition to the other indirect effects of past voting on future voting).

I now estimate a turnout model, with appropriate controls, and display the results in Table 5. The model in the first column represents the basic Aldrich et al. specification, using a similar but slightly different set of controls. As suspected, the interactive effect of frequency and context are smaller than they are in the habit strength models in Tables 2 and 4. This is to be expected because voting in a stable context should primarily influence habit strength, which should mediate such effects on turnout. The interaction is nevertheless significant at a .1 standard. But after I include the habit strength variable, the interaction is no longer significant. I suspect that the results would be even more stark if additional controls are included (to account for unobserved influences captured by frequency and context), and if I used a more valid measure of context stability. Nevertheless, the results strongly suggest that the interactive effect of context and frequency on turnout is mediated by habit strength.

**Table 5. Probit Estimates for Turnout in 2010 General Election**

	Coef.	S.E.	Coef.	S.E.
Prior voting frequency <sup>a</sup>	6.32	(1.77) ***	5.44	(1.79) ***
Stable context <sup>b</sup>	1.62	(0.60) ***	1.43	(0.60) **
Frequency x context	-3.94	(2.24) *	-3.53	(2.21)
Habit	--	--	1.10	(0.45) **
Angry about candid/issu	1.14	(0.32) ***	1.21	(0.32) ***
Enthusias. about candid/issu	-0.46	(0.41)	-0.56	(0.42)
Political efficacy	1.07	(0.58) *	0.93	(0.58)
Strength of party ID	-0.26	(0.30)	-0.27	(0.31)
Strength of candidate pref.	-0.02	(0.28)	-0.08	(0.28)
Interest in politics - general	-0.44	(0.51)	-0.51	(0.52)
Interest in candidates/issu	1.50	(0.45) ***	1.42	(0.45) ***
Perceive election as close	0.95	(0.37) **	1.00	(0.38) ***
Dayton (Dem) support	0.19	(0.22)	0.14	(0.22)
Emmer (Repub) support	0.47	(0.24) *	0.42	(0.24) *
Intercept	-2.84	(0.54) ***	-3.12	(0.56) ***
N	660		660	
LR $\chi^2$	193.0		199.07	
Pseudo R <sup>2</sup>	0.399		0.412	

Notes: \*\*\*p<.01, \*\*p<.05, \*p<.01, using two-tailed tests. All variables use data from before the November election (t-1), and are rescaled to range from 0 to 1.

<sup>a</sup> Number of times voted in prior 12 elections (from '04-'09)

<sup>b</sup> Residential stability

This is further evidence that turnout scholars must measure voting habits directly, rather than use a measure based on voting frequency or a frequency-context interaction. The results support the theory of habitual voting, which dictates that voting frequently in the same context influences turnout through habit strength. The VHSI also captures the hypothesized effects far more appropriately than the interactive context-frequency measure. The frequency-context measure fails to capture habitual voters who voted frequently but moved residences. There are no perfect relationships between frequency, context, and habit strength. Some infrequent voters develop the habit faster, due largely to voting in a stable context, and some develop it slower. And, once an individual becomes a frequent voter, the context matters little.

With this in mind, prior turnout scholarship appears to overstate the importance of context stability in voting. Context is a critical piece of the explanation for voting habits. But it primarily helps explain why individuals *develop the habit* of voting—not why they *maintain the habit* over time. And given the enormous unobserved heterogeneity captured inappropriately by indicators of prior voting frequency, direct measures of voting habit strength like the VHSI are all the more critical.

#### *Accounting for Unobserved Heterogeneity*

I have argued that using frequency of prior voting (interacted with context stability or not) as an independent variable in models of turnout is often inappropriate since the coefficient for prior voting captures numerous unobserved influences on turnout. Even including many controls may not always help. For that reason, Gerber et al. (2003) use an instrumental variables approach with the random assignment to vote-enhancing treatments as the instrument to estimate the effects of past voting on future voting. Modeling habit strength, rather than turnout, overcomes some of this problem, since there are fewer unobserved factors that influence both past frequency and habit strength. Habit strength and turnout are different constructs. Nevertheless, as a check on my previous results, I carry out an analysis more similar to the one generating Table 2 and Figure 1 results.

I model change in habit strength from the pre- to post-election survey as a function of voting in the 2010 election and stable voting context here.<sup>15</sup> This modeling strategy is less effective for testing hypotheses about voting context than it is for testing the effects of voting by itself, as I do above. The theory dictates that voting many times

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<sup>15</sup> Specifically, I model post-election habit strength with pre-election habit strength as a predictor using OLS. The coefficients are identical to those if I modeled changed in habit strength (post minus pre).

in the same context slowly strengthens habits. Assessing the effects of voting once in the same context is less effective to test this proposition, and more likely to understate the effects of frequency and context. Nevertheless, if the results are similar using this approach, they would be even further confirmation.

I display the results of such a model in Table 6. The relationships are all almost identical to those in Table 4, just with slightly smaller magnitudes of effects. Predictions yield the identical substantive relationships as those in Figure 2 (but with 2010 voters in this table replacing “frequent voters” in Table 4, and 2010 non-voters replacing “infrequent voters”).<sup>16</sup> Individuals who voted do not have higher predicted habit strength scores whether they have a stable or unstable voting context. For non-voters, being in a stable context is associated with higher habit strength scores. This particular finding about non-voters suggests that the residential stability variable may be picking up effects of variables besides voting context (variables not observed in this model), since residential stability is associated with higher habit strength scores despite them not voting.<sup>17</sup> This further suggests that we should be careful not to draw too many conclusions about the effects of “context stability” on habit strength and voting, when “context stability” is measured as living in the same residence for many years. Although residential stability helps explain some habit formation, frequency of prior voting is the primary driver of habit formation. Nevertheless, the results in this table corroborate the

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<sup>16</sup> I do not display results of a three-way interaction between baseline habit, frequency, and context. Previously I interacted baseline habit with frequency in order to capture non-linear effects and ceiling effects of frequency on habit strength. But failing to account for non-linear effects does not alter the basic substantive conclusions about context in this case.

<sup>17</sup> Controlling for prior voting frequency does not change this finding. Including a prior frequenting variable has no significant effect on the coefficients, predictions, or model fit. The coefficient for frequency is also not significant ( $b=.002$ ,  $p=.39$ ).

previous results showing that effects of residential stability on habit strength are quite different for individuals who vote frequently and those who do not.

**Table 6. OLS Estimates for Post-Election Habit Strength<sup>a</sup>**

	Coef.	S.E.	
Voted Nov. 2, 2010	0.23	(0.02)	***
Context stability	0.18	(0.04)	***
Voted x Context stability <sub>t-1</sub>	-0.17	(0.04)	***
Habit strength <sub>t-1</sub>	0.56	(0.02)	***
Mood <sub>t-1</sub>	0.02	(0.03)	
Internal political efficacy <sub>t-1</sub>	0.03	(0.02)	
Anxiety linked to polls <sub>t-1</sub>	0.00	(0.02)	
Pride linked to polls <sub>t-1</sub>	0.06	(0.02)	***
"Actions" imagery message	0.03	(0.01)	***
"Rewards" imagery message	0.03	(0.01)	***
N	433		
Model F	168.91		
Adjusted R <sup>2</sup>	0.7954		
Root MSE	0.081		

Notes: \*\*\*p<.01, \*\*p<.05, \*p<.1, using two-tailed tests

<sup>a</sup> Coefficients are the same as for change in habit strength from pre- to post-election survey. Habit strength in this model is a raw sum of VHSI items, rather than the weighted sum using factor loadings, and excludes "know where to go" item. All variables are rescaled to range from 0 to 1.

### *Automaticity vs. Identity*

Recall hypothesis 2d: having a stable voting context should strengthen the automaticity component of voting habit strength more than the identity component. Moving residences and polling locations should make the Election Day actions (including planning the trip to the polls and fitting voting into the daily routine) less automatic. Movers are less familiar with how to get to their polling place, must consciously think

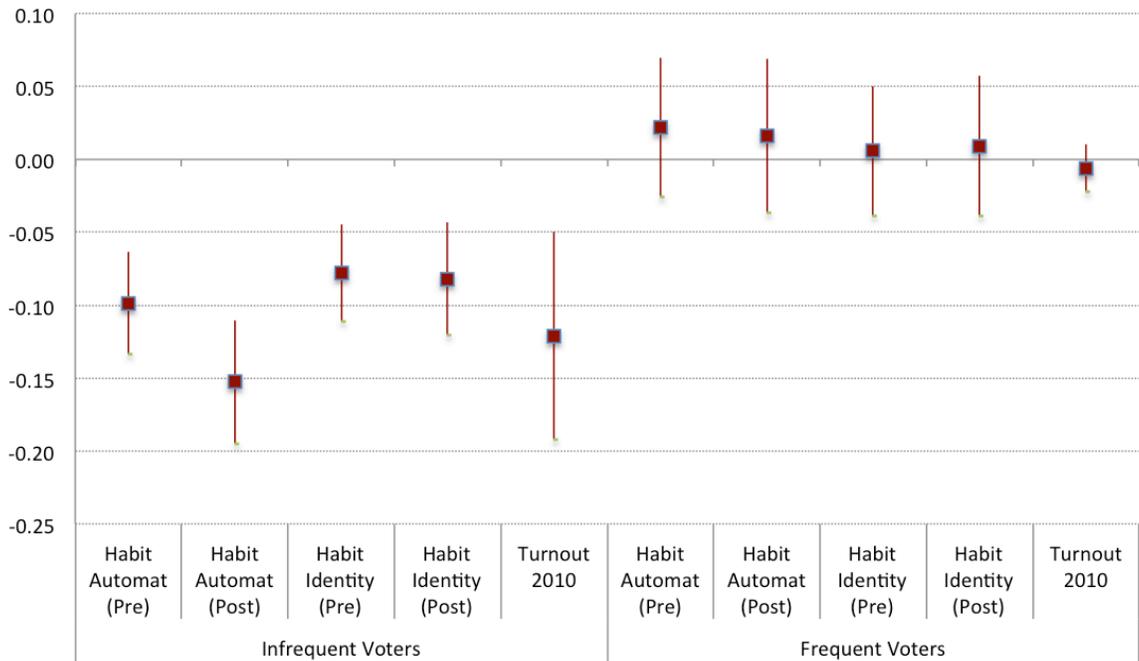
about changing their daily routine more to incorporate voting, and must devote more thought and effort to planning the trip to the polls.

In Chapter 3, I found that the two components of voting habit strength were unique and could be measured separately, despite the .7 correlation between them. In the theory of habitual voting I propose that most of the influences on habit strength affect the automaticity and identity components similarly, and that the two factors will influence other variables similarly too. Thus, for most analyses in the dissertation, it is appropriate and most convenient to treat habit strength as a unidimensional construct. Nevertheless, context disruptions should have a somewhat greater effect on automaticity in voting-related acts than identity as a voter, as I suggest above.

To examine this proposition, I estimate the models from Table 4 separately for the automaticity and identity subscales of voting habit strength. In Figure 3, I display the predicted effect of a context disruption (going from the 90<sup>th</sup> to 10<sup>th</sup> percentile values of residential stability, or from 10-plus years in the home to 1-5 years in the home) separately for frequent and infrequent voters. For illustration I also included the effects on turnout in 2010 from Table 5, column 1.

As I find in my other analyses, only infrequent voters are strongly affected by residential instability. Frequent voters' habit strength levels and turnout are not significantly influenced by moving residences in these results. For frequent voters, the 95% confidence intervals overlap with 0 for all outcome variable estimates. But none of the confidence intervals for the infrequent voters' predicted effects reach 0, indicating that the effects are significant and distinguishable from 0.

**Figure 3. Predicted Effects of Residential Instability on Habit Strength and Turnout, by Frequency of Prior Voting**



As hypothesized, residential instability influences the automaticity in voting acts more than the strength of identity as a voter. The disruptive effect of residential instability on the voting automaticity subscale ranges from -.10 to -.15, depending on whether automaticity-habit scores are measured before or after the election.<sup>18</sup> (The entire scale ranges from 0 to 1.) By contrast, the disruptive effect of residential instability on the voter identity scale is about -.08. Although the difference between the effects on automaticity and identity as a voter is clearly significant in the post-election data, it appears not to be in the pre-election data.

Despite some significant differences between the effects of context on the automaticity and identity subscales in Figure 3, these predictions might underestimate the

<sup>18</sup> I include predictions for both pre- and post-election habit strength since I examine the effects separately below.

overall effect of context. I seek not only to examine whether residential instability is associated with lower automaticity-habit scores for infrequent voters, but also whether considering residential stability helps explain variation in automaticity above and beyond considering frequency of prior voting. I can compare the fit of a model of automaticity in voting that includes variables for prior frequency and controls only, and the fit when both the residential stability and interaction between residential stability and voting frequency are included. Including these two context variables improves the fit of both the automaticity-habit and identity-habit models significantly. I cannot test that the coefficients are 0, since one is an interaction. But the model fit in each case improves. For the automaticity-habit model, the adjusted R-squared value increases by .031 using pre-election data, and .067 with post-election data. For the identity-habit model, the adjusted R-squared value increases by only .017 in pre-election data, and .018 in the post-election data.<sup>19</sup> I report the results for the separate automaticity and identity models in Appendix Table 4.

All results considered, residential (or “context”) stability appears to be more consequential for the automaticity component of voting habit strength. Having moved residences in the past (even 1 to 5 years in the past) makes getting to the polls seem less efficient, more conscious, and something one must think about more. Residential stability is simply less important for one’s identity and how strongly one perceives oneself as a voter.

Moreover, I suspect that context disruptions appear more consequential after the 2010 election (in the post-election data) because the effect is compounded over time.

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<sup>19</sup> Additionally, the root mean squared error is reduced after including the context variables by a healthy .0082 (from .1761 to .1679) with the automaticity model in post-election data, but only .0024 for the identity model. There is also a difference using the pre-election data, but it is slight.

Moving residences reduces infrequent voters' strength of habit—particularly the automaticity component of habit—which subsequently reduces their likelihood of voting. If this leads them to not go to the polls, then their habit strength will be lower still.

There is some evidence for this dynamic. In Figure 3, residential instability is associated with significantly lower turnout for infrequent voters.<sup>20</sup> And not voting is associated with significant reductions in habit strength—for all groups I have examined. Specifically, not voting on November 2, 2010 led respondents to have lower VHSI scores on average than they did in October. So, it should follow that the initial residential instability should be associated with *even lower habit strength* for infrequent voters after the election than before the election—particularly for automaticity in voting, since that component is influenced more by context. Figure 3, in fact, illustrates these basic relationships. Future research might clarify these relationships even further. Using a more nuanced measure of context stability in voting would help. Optimally, I would determine exactly where each respondent voted in each election, rather than length of time at the current residence.

In sum, the results in this section help identify the conditions under which voting contexts (especially the stability of the voting location) shape habits and turnout. Prior researchers have found that residential stability is strongly associated with turnout. But it has remained unclear why the association is so strong, and for whom it is strongest. I broadly argue that residential stability does not have a direct causal influence on turnout, but instead interacts with prior frequency to help people form voting habits over time. I test several specific hypotheses about the effects of context and prior frequency on habit

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<sup>20</sup> I do not control for habit strength in this turnout model because context and frequency primarily influence habit strength—and including it makes the interaction insignificant, as demonstrated above. The turnout model is the same as the one reported in Table 5.

strength, and find generally strong support for each. I demonstrate that residential stability is far more important than stability in the Election Day voting time for habit formation. And the results help confirm the foundational hypothesis of the theory of habitual voting: voting frequently in a stable context is the primary cause of stronger voting habits. Regarding the conditions under which context matters, infrequent voters are the primary group influenced by residential location changes. And I show that moving changes appear to disrupt habit formation by making Election Day acts less automatic and efficient. In the next section I further illuminate the mechanisms underlying voting habit formation by examining how voting generates psychological rewards that incentivize future voting.

### **Psychological Rewards of Voting**

Turnout scholars tend to analyze the factors that influence voting, rather than the consequences of voting and dispositional changes that result from going or not going to the polls. And they tend to treat the predictors of voting as exogenous to turnout behavior. This is a reasonable strategy for static, time-invariant analyses with truly exogenous influences on turnout. For example, campaign phone calls and mailings are mostly exogenous to turnout, and their effects on voting can be fairly accurately assessed in a time-invariant framework, particularly with randomized experimental designs. But even campaign targeting is endogenous to voting in some ways. Scholars find that individuals more likely to vote are more likely to be targeted for mobilization (Rosenstone and Hansen 1993; Leighley 2001). In general, failing to account for the reciprocal relationships between turnout and the factors that influence turnout will lead

scholars to ignore much of the explanation of turnout behavior, and ignore the full effects of some variables on turnout over time.

In particular, I hypothesize in Chapter 2 that voting—especially for the winning candidate—generates specific psychological “rewards,” which make voting in the future more likely (H3). This set of hypotheses concerns path *b* in the structural model displayed in Figure 1. Specifically, I hypothesize:

H3a: Voting decreases Election Day anxiety, increases Election Day pride, increases internal political efficacy, and improves mood (more positive and energetic emotional states over time).

H3b: Voting for the winning candidate (in the race for the highest office) decreases Election Day anxiety, increases Election Day pride, increases internal efficacy, and improves mood more than voting for the eventual losing candidate.

H3c (moderation hypothesis): Voting and voting for the winning candidate changes Election Day anxiety, pride, internal efficacy, and mood *more for individuals with weak habits than strong habits*.

I do *not* hypothesize that voting in a stable context generates more rewards than voting in an unstable context.<sup>21</sup> In general, the role of psychological rewards in promoting habit formation by incentivizing future behavior has not been extensively studied by habit researchers in social psychology. However, the neuroscience and cognitive psychology literatures have studied this dynamic extensively, and suggest that rewards are an essential aspect of understanding habit formation over time (see Balleine and Dickinson 1998). Without some form of “satisfying effects,” as Thorndike notes in his 1911 “law of effects,” habits would not develop. And there are strong suggestions in those literatures, and in political science, as to the types of rewards generated by voting that I highlight in

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<sup>21</sup> I have previously hypothesized and found that voting in a stable context strengthens voting habits—which subsequently increases the likelihood of future voting. However, I am generally agnostic on whether voting when the location is stable over time should generate more psychological rewards, like mood associations with voting and internal efficacy.

Chapter 2. I next briefly review the basic hypotheses, and test them as they relate to internal efficacy, Election Day anxiety, Election Day pride, and positive and energetic mood.

### *Internal Political Efficacy*

I first test the rewards hypotheses as they relate to internal political efficacy. As I suggest in Chapter 2, successful completion of any task should increase one's feeling of efficacy in that domain. Bandura's research highlights the powerful effects of task completion on feelings of confidence and power to exert influence on situations. Voting is no different. Successfully participating in politics by going to the polls should alter one's personal sense of political efficacy. Specifically, voting should boost one's sense of internal efficacy (H3a).<sup>22</sup> And voting for the winning candidate in a major race should increase efficacy even more than voting for the losing candidate (H3b). Realizing one voted for the winner should make one's actions seem more influential in this case, as if one controlled the election outcome, generating additional efficacy. One's voting would appear more "successful." Valentino et al. (2009) demonstrate empirically using ANES panel data that voting, especially for the winner in presidential elections, is associated with higher internal efficacy levels over time. Furthermore, Javeline and Brown (2003) argue that political participation, especially protest, should improve mental and physical health outcomes largely because it helps people overcome feelings of loss of control. Loss of control is functionally equivalent to a loss in internal efficacy. Both

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<sup>22</sup> Internal efficacy is self-referential, and going to the polls or not going to the polls is carried out by oneself. It should thus have larger effects on one's self-conception than on conceptions of the broader political system. (External efficacy refers to how malleable the political system as a whole is to one's actions.)

hypothesized effects (voting and voting for the winner boosting internal efficacy) should occur more for individuals with weak than strong voting habits (H3c).

If confirmed, these propositions would challenge a large body of scholarship on political efficacy. Scholars have found internal political efficacy to be fairly stable over time. If the simple act of voting in a gubernatorial election increases efficacy, then this would further highlight the need to account for the two-way causation between voting and its psychological “rewards”—and no longer treat it as just an exogenous predictor of turnout.

I measure internal political efficacy with four standard survey questions.<sup>23</sup> Combining the responses yields a reliable scale using both the pre-election and post-election data (unstandardized alpha values of .838 and .816, respectively). Following the modeling strategy at the start of the chapter, I model post-election internal efficacy as a function of pre-election internal efficacy and voting on November 2.

Since I include baseline efficacy in the model, I only need to control for factors that influence the likelihood of internal efficacy changing in the time between the two surveys, from late October to November, and factors that influence the rate of change. Thus, I control for the number of days before the election the respondent took the pre-election survey<sup>24</sup>, and the time of day she took the pre-election survey.<sup>25</sup> I also control

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<sup>23</sup> Respondents are asked how strongly they agree or disagree with the following statements on a 5-point scale: I consider myself to be well qualified to participate in politics; I feel that I have a pretty good understanding of the important political issues facing our country; I feel that I could do as good a job in public office as most other people; I think that I am better informed about politics and government than most people.

<sup>24</sup> Respondents could take the first survey up to 5 days before the election. Since any number of political events could have occurred in this time that would influence them more than individuals who took it right before the election, this is an important control.

<sup>25</sup> Mood-related responses might be influenced by the time of day one takes the survey. If individuals who vote are more likely to take surveys at a particular time, this should be controlled

for whether respondents received an actions or rewards imagery message (randomly assigned), since these are correlated with many of the independent and dependent variables.

Additionally, I control for the respondents' congressional district of residence. I proposed in Chapter 2 that regional norms about voting and participation should influence voting through the psychological rewards. In other words, when people live in areas where voting is highly valued, they should feel better, less anxious, more proud, and more efficacious as a result of voting, and thus vote at a higher rate. Although I would not need to control for regional norms in this modeling framework since they are time-invariant (and I control for baseline levels of the dependent variables), certain regions might have greater rates of change of psychological rewards. My sample includes only Minnesotans. Thus, I control for the respondents' congressional district of residence. I do not report the coefficients for CDs or the administration time variables for clarity in presentation.

Finally, I control for party identification<sup>26</sup> in all vote-for-winner models. By doing this I ensure that the coefficient of interest represents voting for Dayton (the presumed winning candidate in the gubernatorial race) and not just the effects of party. Democrats might also be naturally prone to faster increases in internal efficacy from October to November for other unobserved reasons.

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for. My variable for time of day takes on 21 values, with 5 corresponding to 5 a.m., 6 to 6 a.m., 13 to 1 p.m., etc. (3 respondents took the survey around 1 a.m., and 1 around 2 a.m. I assign values of 25 and 26 for these respondents.)

This assumes a linear relationship between the hour of day and efficacy levels, which is a reasonable assumption in this case. A model for internal efficacy with this variable had a better fit to the data than a model with separate dummy variables that divided respondents into three-hour time groups.

<sup>26</sup> High values of party identification indicate stronger identification as a Democrat on a 7-point scale.

The primary results are displayed in Table 7, models 1-3. The model 1 results demonstrate that voting on November 2, 2010 is associated with significantly higher internal political efficacy scores. Since I control for baseline levels of internal efficacy (as reported in the pre-election survey), the coefficient represents the effect of voting on *change in efficacy from the pre-election to post-election survey*. Although the magnitude of the effect is not particularly large, it is significant at the .05 level (using a one-tailed test). I conclude that voting does, in fact, cause increases in internal efficacy.

Voting does not have this effect on external political efficacy, as I suggested. Estimating an identical model but replacing internal with external efficacy yields a coefficient for external efficacy that is not significant ( $p=.67$ ). For all three models in Table 7, using external efficacy does not produce models with significant coefficients for voting or any of the theoretically important interactions.

I next examine whether habit strength moderates the effects of voting on internal efficacy (testing H3c). The model fit is not improved by including an interaction between habit strength and voting. This suggests that the effects of voting on internal efficacy are not significantly different for habitual and non-habitual voters. And this is perhaps the first serious strike against the theory. It clearly contradicts H3b. But it is not an entirely devastating strike. Even if voting produces the same degree of efficacy change for habitual and non-habitual voters, efficacy might still be responsible for the formation of voting habits. It could just be that efficacy levels influence future turnout less for individuals with strong voting habits (the path from rewards to future turnout in the structural model). And there are other rewards of voting—*anxiety linked to going to the polls and mood*—that might do the heavy lifting of habit formation in its early stages.

But this result nevertheless suggests that further evidence is needed to demonstrate how habits are formed for individuals with weak habits.

In model 3 in Table 7, voting for the winning candidate appears not to influence internal efficacy levels at first glance. The vote-for-winner coefficient is not statistically significant. While there might not be an underlying relationship, testing hypotheses about winner-voting is extremely difficult in these data. The primary reason is that the 2010 governor's race in Minnesota had no clear winner until the end of November due to a recount. The margin of victory immediately after the election was small enough to trigger an automatic recount. The election results were still unclear two weeks after the election—when the post-election survey was administered—since the recount did not conclude until later. By mid November it was reasonably clear that the Democrat, Mark Dayton would eventually win—for individuals reading the newspapers closely. So I expect to see some effects of the winner-voting variable in these data. But supporters of Republican gubernatorial candidate Tom Emmer still had reason to be optimistic with the legal challenges.

**Table 7. Effects of Voting and Voting for the Winning Candidate on Post-Election Internal Efficacy and Anxiety & Pride Linked with Polls**

	Internal Political Efficacy			Anxiety Linked with Polls			Pride w/ Polls
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Voted Nov. 2, 2010	0.03 (0.02) **	-0.01 (0.04)	0.03 (0.02) **	-0.07 (0.03) ***	-0.13 (0.08) **	-0.07 (0.03) ***	-0.03 (0.03)
Habit strength	--	-0.03 (0.06)	--	--	-0.13 (0.10)	--	--
Voted x Habit strength	--	0.06 (0.06)	--	--	0.11 (0.11)	--	--
Voted for winner/Dayton	--	--	-0.003 (0.01)	--	--	0.001 (0.02)	0.05 (0.02) **
Party identification	--	--	0.005 (0.02)	--	--	0.02 (0.03)	-0.06 (0.03) **
Actions imagery message	0.02 (0.01) *	0.01 (0.01)	0.02 (0.01) *	-0.04 (0.02) **	-0.04 (0.02) **	-0.04 (0.02) **	0.01 (0.02)
Rewards imagery message	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	-0.02 (0.02)	-0.02 (0.02)	-0.02 (0.02)	-0.01 (0.02)
Pre-election dep. var.	0.77 (0.02) ***	0.76 (0.02) ***	0.77 (0.02) ***	0.50 (0.04) ***	0.50 (0.04) ***	0.50 (0.04) ***	0.63 (0.03) ***
Intercept	0.15 (0.03) ***	0.17 (0.04) ***	0.15 (0.03) ***	0.16 (0.04) ***	0.23 (0.07) ***	0.14 (0.05) ***	0.26 (0.05) ***
N	505	505	505	513	513	513	510
Model F	106.5	92.3	92.0	16.25	14.18	14.14	33.98
Adj R <sup>2</sup>	0.731	0.731	0.730	0.279	0.279	0.278	0.493
Root MSE	0.105	0.105	0.105	0.185	0.185	0.185	0.183

Notes: Entries are OLS coefficients with standard errors in parentheses. \*\*\*p<.01, \*\*p<.05, \*p<.10, using one-tailed tests. All variables rescaled to range from 0 to 1. All models control for congressional district of residence, time of day resp. took pre-election survey, and number of days before Nov. 2 resp. took pre-election survey.

Given the large uncertainty about the winner, the effects of “voting for the winner” variables in this chapter will be weaker than they would be under normal election circumstances. So these analyses represent particularly hard tests of the hypotheses.<sup>27</sup> But regardless of the reason, voting for the winning candidate appears not to significantly influence internal efficacy levels in these data. I take this as evidence against H3b as it relates to efficacy. I turn now to examining the effect for the other psychological rewards of voting.

#### *Anxiety Linked with Going to the Polls*

I now test hypotheses 3a, 3b, and 3c as they relate to the role of anxiety associated with going to the polls on the formation of voting habits. Reductions in Election Day anxiety should be an important mechanism by which voting is perpetuated over time. Wood et al. (2002) demonstrate clearly that individuals associate habitual behaviors with more positive emotions (i.e., pride) and less negative emotions (i.e., anxiety). However, rather than conceptualizing task-linked emotions as a byproduct of habit formation, as Wood et al. (2002) do, I argue that task-linked emotions (i.e., anxiety associated specifically with Election Day actions) play a causal role in the habit formation process. They provide an incentive for the action to occur in the future.

I measure anxiety linked with Election Day voting using one question in both the pre- and post-election surveys. Respondents were given the following instructions and question: “Now we’d like to know about the feelings you have when you vote—not any particular candidates or elections, but the general feelings you have when you go to vote

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<sup>27</sup> The other reason testing the hypothesis about winner-voting is difficult is the measurement error in reporting vote choice. People might report they voted for the winner when they did not. People are not fond of admitting having supported a losing candidate, even a candidate who was only partially certain to have lost.

on Election Day.... How anxious does voting usually make you feel?” Response options include extremely, very, somewhat, a little, and not at all anxious. The instructions are intended to generate responses that reference mental associations with traveling to the polls and other aspects of turnout unrelated to the candidates and issues.

The results in Table 7 confirm that voting is associated with fewer Election Day feelings of anxiousness. This confirms H3a as it relates to anxiety. Turning out on November 2 is associated with .07 to .08 lower Election Day anxiety values. (Anxiety ranges from 0 to 1). Figure 4 displays some of the predictions. Voting appears to be associated with somewhat greater reductions in anxiety for non-habitual voters than habitual voters. However, the difference in effect sizes between habitual and non-habitual voters is not statistically distinguishable from 0. As with the results for most other psychological rewards, I find that voting and voting for the winning candidate does *not* produce significantly different effects sizes for habitual and non-habitual voters. Or at least the difference is not detectable in these data. Overall, the available data generally refute H3c (that the psychological rewards of voting influence non-habitual voters more than habitual voters).

#### *Pride Linked with Going to the Polls*

In model 7, I only display the results from the vote-winner model, since the direct effect of voting on pride is not statistically significant, and there is no significant moderating effect of habit strength. I utilize a question on the pre- and post-election surveys to estimate the effects on pride in the same way that I do for Election Day anxiety. (The question asks about “feelings you have when you vote—not any particular candidates or elections”).

While the direct effect of voting on pride linked with going to the polls is not significant, the effect of voting for the winning candidate is large and significant. Predictions show that voting for the winning candidate for the highest office in the election (i.e., voting for Democrat Mark Dayton) is associated with .053 higher pride scores than voting for one of the losing candidates. (This effect on pride is over and above the effect of voting only.) This effect is particularly notable given that it is in addition to the effect of just voting, and that the pride question is intended to measure non-partisan, candidate-blind mental associations with turnout. The instructions ask respondents to specifically think about the feelings associated with voting and not a candidate or specific election. Of course, respondents might have the last election in mind when answering this question. But the result is still substantively important.

Also, one should remember that all models control for baseline levels of the dependent variable—in this case pre-election pride linked with voting. Because of this, the 2010 voting coefficient does not incorporate the effects of all omitted variables associated with both pride and voting tendencies. As a further check that the coefficient does not include such error, I conduct a sensitivity test by including a control for frequency of prior voting in the past 12 elections. The coefficient for this variable is not statistically significant and has almost no effect on the voting coefficient, or any other coefficients in the model. This means that the 2010 voting coefficient is not simply capturing the effects of being a frequent voter (or effects of variables related to being a frequent voter). Instead, it represents the effect of the simple act of casting a ballot in the 2010 election relative to not casting a ballot (or casting a ballot for the winning candidate

versus for the losing candidate). Overall, these results lead me to reject H3a and H3c as they relate to pride, but accept H3b.

### *Positive and Energetic Mood*

In this section I examine whether voting and voting for the winning candidate is responsible for changes in mood, and for whom. I conceptualize “mood” as the general experience of positive or negative affect over time. By “general experience...over time” I mean that (1) moods are relatively diffuse emotional states not linked to a particular activity or stimulus, and (2) moods are less momentary and more stable over time than emotions.<sup>28</sup> I hypothesized that voting, especially for the winner, makes one’s mood more positive—and that this effect is strongest for individuals with weaker habits. In the previous section I analyzed survey questions that asked respondents about their emotions (i.e., anxiety and pride) associated with the acts of voting on Election Day in general. In this section, I analyze questions about mood states entirely unrelated to the voting context. There are many reasons why individuals’ moods might change over a two- to three-week period between the pre-election and post-election survey questions. The election may not be a particularly significant one for many individuals relative to other life events. But in the context of the survey, which focuses almost entirely on turnout, the election should be highly accessible in memory. And if voting is significantly associated with changes in mood, despite all the other changes that occur as noise in the estimate, this would be powerful evidence in support of the hypotheses. I am apt to believe that the survey primes voting-related constructs, and that responses about mood reflect their associations with their voting or non-voting behavior. Nevertheless, we

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<sup>28</sup> Psychologists have theorized less about mood than about affect, and have offered a number of definitions. Nevertheless, this conceptual definition appears to fit well with most accounts in psychology. See Baas et al. (2008, 781-2) for one theoretical perspective on mood.

might take the results at face value that the simple act of voting produces measurable changes in self-reported positive mood (happiness) and energetic mood over several weeks.

I measure mood with two questions I adapt from the SF-36v2 health survey.<sup>29</sup> The SF-36v2 contains a battery of questions designed to measure mental health status and vitality. I ask respondents “How much of the time in the past two weeks... Have you been happy?” and then “Did you have a lot of energy?” Response options include all of the time, most of the time, some of the time, a little of the time, or none of the time. The “happy” item is intended to measure the positive-negative (valence) dimension of mood. And the “energy” item is intended to measure the energetic-non-energetic (arousal) dimension of mood.<sup>30,31</sup> As noted in Chapter 2, research on the circumplex model of emotions finds that positive-negative valence and arousal are the two primary dimensions of emotions.

I rely on validated voting information, and not self-reported voting for the analysis. In the post-election survey, self-reported voting responses may be biased by the

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<sup>29</sup> For background on the questions, see <http://www.sf-36.org/tools/SF36.shtml>.

<sup>30</sup> The SF-36v2 mental health and vitality items are not intended to measure mood as much as they are mental health, in part to assist practitioners in diagnosis of emotional states that reflect depression, anxiety, or other disorders. Responses presumably reflect a range of emotional states more negative than in the general population. I am instead interested in a more general positive-negative (valence) emotional states, and energetic-non-energetic (arousal) emotional states for this analysis. And I only included 5 of the SF-36v2 items in my own surveys due to space constraints: related to being happy, worn out, very nervous, feeling calm and peaceful, and having a lot of energy. Of those items, the “happy” question appears to measure positive-negative mood most effectively, and “energy” measures energy-arousal most effectively.

Additionally, I adapted the original items in both the pre- and post-election surveys to ask about feelings in “the past two weeks” rather than “the past four weeks.” This was needed for the analysis to reflect changes that occurred since the election in the post-election survey—since I administered the post-election survey *two* weeks after the election.

<sup>31</sup> I initially combined the “calm-peaceful” item with “happy” to measure positive-negative mood, but quickly discovered that calm-peaceful scores are correlated with energy. I intended at least one of the mood items to measure positive-negative (valence) exclusively.

types of emotional states I seek to measure change in. People who feel worse after not voting should be more likely to report voting when they did not. But given that the survey is mostly about voting, I suspect that post-election responses to the mood questions may be biased in part by the person's voting behavior (even if measured with validated information from the state voter file). The survey questions should prime voting in memory, so the respondent's responses about general mood may be influenced by the knowledge that she either voted or failed to vote. However, to the extent that this is occurring, this would actually produce a *better* measure of mood. Although my empirical measures assess change in mood as a result of voting, optimally I would prefer to measure the mood evoked when thinking about voting or not voting—because this is primarily what should influence turnout behavior in the future. Nevertheless, both mood constructs should be highly related.

**Table 8. Effects of Voting and Voting for the Winning Candidate on Post-Election Mood<sup>a</sup> (N=501)**

	Positive Mood			Energetic Mood		
	[1]	[2]	[3]	[4]	[5]	[6]
Voted Nov. 2, 2010	0.04 (0.02) **	0.04 (0.05)	0.05 (0.02) ***	-0.01 (0.02)	0.02 (0.06)	-0.02 (0.02)
Habit strength	--	0.11 (0.07) *	--	--	0.10 (0.08) *	--
Voted x Habit strength	--	-0.03 (0.08)	--	--	-0.07 (0.08)	--
Voted for winner/Dayton	--	--	-0.01 (0.02)	--	--	0.04 (0.02) **
Party identification	--	--	-0.02 (0.02)	--	--	-0.06 (0.02) ***
Actions imagery message	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.02)	0.00 (0.02)	0.00 (0.02)
Rewards imagery message	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.01 (0.02)	0.01 (0.02)	0.01 (0.02)
Pre-election dep. var.	0.62 (0.03) ***	0.61 (0.03) ***	0.61 (0.03) ***	0.64 (0.03) ***	0.62 (0.03) ***	0.63 (0.03) ***
Intercept	0.19 (0.04) ***	0.14 (0.05) ***	0.21 (0.04) ***	0.21 (0.04) ***	0.16 (0.05) ***	0.24 (0.04) ***
Model F	27.9	24.9	24.8	30.1	26.3	26.8
Adj R <sup>2</sup>	0.4	0.4	0.4	0.4	0.4	0.4
Root MSE	0.123	0.12	0.12	0.139	0.139	0.138

Notes: Entries are OLS regression coefficients with standard errors in parentheses. \*\*\* p<.01, \*\* p<.05, \* p<.10, using one-tailed tests.

All models control for congressional district of residence, time of day respondent took pre-election survey, and number of days before Nov. 2 respondent took pre-election survey. All variables rescaled to range from 0 to 1.

<sup>a</sup> Mood represents emotional states in the 2 weeks prior to each survey.

The results are presented in Table 8. The models result in column 1 allow me to test H3a (that voting improves mood over time). The coefficient for the turnout variable

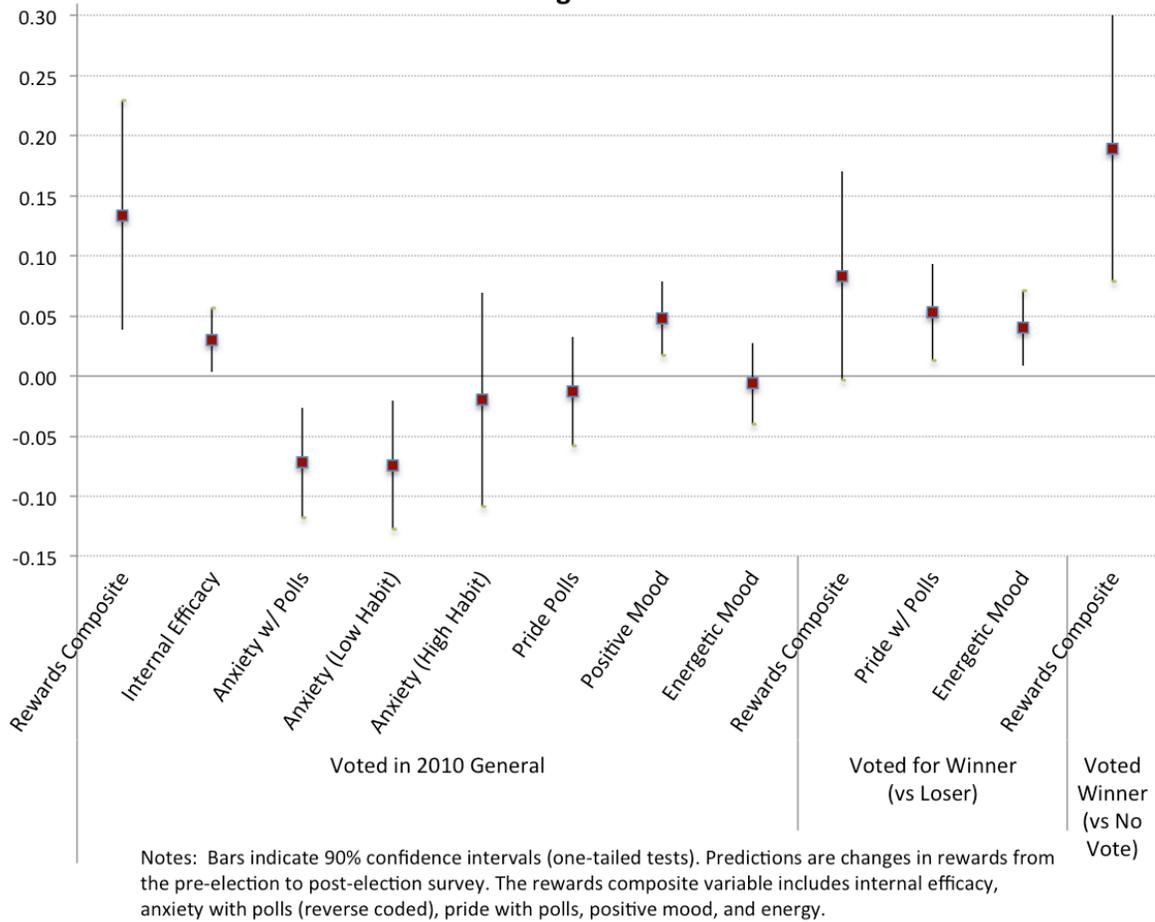
is statistically significant, indicating that voting is associated with statistically significant increases in positive mood over time. Again, note that I control for pre-election mood; so coefficients represent the change in mood from pre- to post-election survey time. As with the results for other psychological rewards, the magnitude of the effects is modest. Mood is measured on a 0-to-1 scale, so a .04 shift is not particularly large. However, when the changes for all voting rewards are compounded, the effect size is much larger. Figure 4 in fact displays the predicted effects for a simple composite measure of all the rewards.<sup>32</sup> When the changes for all the voting rewards are compounded, the effect size is much larger. And if the experience of rewards has a larger-than-average effect on future turnout for some groups (which I test in the next section), then the rewards changes will be larger in size still.

The results in model 2 do *not* allow me to confirm H3c. Voting does not increase positive mood for individuals with weak voting habits more than strong voting habits. The coefficient for the interaction between mood and habit strength is not significant. Finally, the model 3 results do not allow me to confirm H3b. Voting for the winning candidate is not significantly associated with increases in positive mood over time.

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<sup>32</sup> To construct the composite variable, anxiety linked with polls is reverse-coded to reflect the hypothesized direction.

**Figure 4. Predicted Effects of Voting and Voting for Winning Candidate on Changes in Rewards**



Whereas voting by itself appears to cause improvements in the positive-negative dimension of mood, it appears *not* to make individuals more energetic. The results do not confirm H3a and H3c as they relate to energy/arousal. However, the model 6 results indicate that voting for the eventual winner is associated with significantly more energetic mood states. The coefficient for the winner-vote variable is significant and positive.<sup>33</sup>

<sup>33</sup> Interestingly, I also find that voting for the eventual winner significantly decreases feelings of calm-peacefulness. I suspect this result is due to the high-profile controversy surrounding the recount in Minnesota at the time, and Dayton voters wondering if he would win the legal challenges. But this interpretation may be imputing too much political interest and enthusiasm to Dayton voters in Minnesota. In general, the results about calm-peacefulness do not appear to speak to the theory directly

Figure 4 presents a summary of the effects from the models of psychological rewards in this section. Taken together, the results demonstrate that voting by itself appears to have modestly sized but significant effects on the “rewards” of voting. Since each reward represents a separate construct, I estimate the effects for the rewards in most of the models separately. However, it is useful to see the effects sizes when the rewards are combined in a single composite measure (reverse-coding the anxiety variable). The effect for that composite variable is displayed in Figure 4 as well. When all the effects of voting on rewards are compounded, their magnitude is larger. But overall, the results support H3a on the whole. However, the results also clearly demonstrate that habit strength does not moderate the effects of voting on the rewards in my data—rejecting H3c. And there are some mixed results for H3b. Voting for the eventual winner increases Election Day pride and energetic mood modestly (relative to voting for the losing candidate). But there is no effect for the other rewards. In the next section, I examine the substantive importance of this finding further.

### **Habit Strength, Rewards, Context, Future Voting**

In the previous sections of this chapter, I examine the effects of voting—including voting for the winning candidate and voting in a stable location—on the strength of voting habits and the psychological rewards of voting. Specifically, I estimate the habit-strengthening effects of residential stability and voting over time. And I confirm that voting produces distinct psychological rewards—reductions in anxiety linked with going to the polls, increased political efficacy, and more positive mood. Voting for the winning candidate also modestly boosts Election Day pride and self-reported energetic mood. I

assess whether some of these effects are stronger for individuals with weak habits, but find no evidence for that particular moderating effect.

These are important first steps for confirming the theory of habitual voting. If voting does not generate any psychological changes in the rewards variables, then those variables cannot influence turnout in a direct, unmoderated way. Likewise, it was first necessary to demonstrate that voting, especially in a stable context, strengthens voting habits. Otherwise, habit strength cannot mediate or moderate the effects of past voting on future voting. I now begin the final (and more psychologically rewarding) step of estimating the interactive effects of habit strength, psychological voting rewards, voting context, and winner-voting on future turnout.

The results in the final section of Chapter 3 on predictive validity demonstrate that habit strength has large and significant effects on turnout. Using a simple direct turnout model (albeit misspecified<sup>34</sup>), I find that habit strength is the most powerful direct influence on turnout—of all the influences I examine in my data. Given that I measure and compare the effects of many other variables known to powerfully influence turnout (political interest, efficacy, and many “costs” and “motives” of voting), those results suggest that habit strength may be one of the largest predictor of turnout of the major theoretically relevant constructs—at least among those that I examine. But voting habit strength does not only influence future turnout directly. I hypothesize that it moderates and mediates other effects on turnout. I focus primarily on those effects on this section.

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<sup>34</sup> The turnout model at the end of Chapter 3 is meant as a validity check. A properly specified turnout model would more closely resemble the models in this section.

Regarding the path from voting rewards to future turnout in the structural model, I hypothesize that experiencing more rewards of voting increases the likelihood of voting, especially for individuals with weak habits. Specifically:

H4a: Experiencing decreased anxiety, increased pride, increased internal efficacy, and improved mood as a result of voting increases the likelihood of voting in the future.

H4b (moderation hypothesis): Experiencing decreased anxiety, increased pride, increased internal efficacy, and improved mood as a result of voting increases the likelihood of voting in the future *more for individuals with weak habits than strong voting habits*.

Regarding the other proposed causal influences on future turnout, I hypothesize:

H5: Strengthening voting habits increases the likelihood of voting.

H6 (moderation hypothesis): The motives and costs of voting influence future turnout more for individuals with weak than strong voting habits.

H7: The effects of prior voting and residential stability on future turnout are mediated by voting habit strength.

Unfortunately, I have no measure of validated or self-reported turnout in 2011 or 2012 that will allow me to simultaneously test all of the hypothesized relationships as they relate to turnout.<sup>35</sup> Therefore, in this section I rely on two modeling approaches. I estimate models of self-reported likelihood of voting in the 2012 presidential election—that include all rewards variables. And I also estimate models of 2010 turnout—that do not contain all the rewards variables. Both modeling strategies are limited in several ways, which I discuss below. But together they allow me to effectively test the remaining hypotheses.

The primary methodological difficulties in this section arise when testing the rewards hypotheses (H4a and H4b). Throughout the dissertation I argue that voting

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<sup>35</sup> As soon as the 2012 election occurs, I will have access to this information.

causes certain psychological changes in anxiety, pride, efficacy, and mood. The evidence in the previous section confirms the changes related to anxiety, efficacy, and positive mood. Some of those changes should be permanent: as one votes more, one's association of anxiety with the polls should permanently decrease. Some of the changes may be temporary—especially mood-related changes. But both types of changes in rewards should influence the likelihood of future voting. In the case of temporary mood-related changes, individuals should still associate voting with positive emotional states in memory—even if temporary—which should increase the likelihood of voting in the future. Individuals expect to receive that feeling when they vote again.

However, individuals who do *not* vote also experiences changes in efficacy, anxiety, pride, and mood. And so simply including variables for mood, efficacy, anxiety, and pride in a model of turnout or future turnout likelihood will not test the hypotheses. I must assess the changes that result exclusively from voting. I do this by including interactions between November turnout and each of the post-election rewards, controlling for pre-election rewards—in the model of 2012 voting likelihood. The coefficients for the interaction terms will represent the effects of the change in rewards between the pre-election and post-election survey for individuals who voted. I also estimate a model of turnout in 2010, but since I do not measure rewards before and after the 2009 election, I cannot test any effects of changes in rewards as a result of voting. So I rely on the 2012 likelihood model to test all the main hypotheses, and results from the 2010 turnout model to confirm that the primary results (except for the results about the rewards) do not change as a result of using voting likelihood instead of validated turnout as the dependent variable.

One problem with modeling the effects of changes in voting-related rewards this way (using interactions between turnout and rewards) is that I must include the turnout variable in the model. Statistically, when including the interaction terms I must include terms for the main effects too. This requirement is unfortunate, since I hypothesize that prior turnout does not influence future directly. The model specification now assumes that it does. As I will demonstrate below, this manner of estimating the rewards effects does not influence my ability to test any of the hypotheses much. But it does prevent me from estimating a single large path analysis that simultaneously tests the relationships between all variables in the theory—and to estimate indirect effects of variables not in the turnout model most accurately.

I first estimate models of self-reported likelihood of voting in 2012. At the end of the post-election survey, respondents are asked how likely they are to vote in the next presidential election in 2012. I use a set of branching questions, and generate an 8-point scale representing the likelihood level. There is not much variation in voting likelihood: 90.3% of respondents reported being extremely likely to vote in the next presidential election. But the lack of variation does not bias the model estimates. It makes detecting underlying relationships harder because of fewer degrees of freedom.

The results are presented in Table 9. I include all the rewards variables necessary to test the hypotheses. The bolded lines are of interest for testing the rewards hypotheses. In model 1, the bolded coefficients represent the overall effect of experiencing rewards as a result of voting—together for habitual and non-habitual voters. Experiencing more efficacy from voting is associated with a higher likelihood of future voting. And experiencing more Election Day pride is associated with a lower likelihood of voting.

The remaining rewards coefficients are not significant. These results speak to H5a (that the voting rewards are associated with higher likelihood of voting for all groups). They provide support for the hypothesis as it relates to internal efficacy, but disconfirm the hypothesis as it relates to the other rewards.

I now test H4b. Do the rewards have different effects on future voting likelihood for individuals with weak and strong habits? The results confirm that all of the rewards have the hypothesized effect on future turnout likelihood, except for Election Day pride.<sup>36</sup> The first set of bolded coefficients in model 2 (vote x rewards) indicates that for individuals with weak habits, internal efficacy experienced from voting increases the likelihood of future voting; Election Day anxiety decreases the likelihood; pride increases the likelihood (relative to non-voters with low rewards). For individuals with strong habits, the relationships are the opposite of those for individuals with weak habits.

For positive and energetic mood, I examine the differential effects among individuals with weak and strong habits separately. Positive and energetic mood are correlated, and the multicollinearity biases the coefficients in model 2. For analyses earlier in the chapter, the correlation between positive and energetic mood has not influenced my ability to examine the substantive effects of each when included in the same model. However, given the fewer degrees of freedom in this model, the multicollinearity is a problem. Thus, I display the results for each mood variable separately in model 3 and 4. For individuals with weak habits, positive mood experienced from voting increases the likelihood of future voting; energetic mood experienced from voting increases the likelihood of future voting. The composite

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<sup>36</sup> As with all analyses in this chapter, the results are not altered if I control for frequency of past voting in the past 12 election from 2004-2009.

rewards variable in model 5 confirms the basic relationship between rewards and future likelihood of voting. In sum, for individuals with weak voting habits, experiencing more efficacy, less anxiety, and improved mood boosts the self-reported likelihood of voting in 2012.

These results provide strong support for the theory. The Election Day pride coefficients are *not* in the hypothesized direction. Something about the experience of pride appears to be different from the other psychological effects of voting. And I cannot determine the reasons for the difference using these data. However, in general, experiencing more “rewards” from voting increases the likelihood of voting in the future for individuals with weak habits. This would confirm that the rewards incentivize voting early in the habit formation process (for individuals with weak habits). For individuals with strong habits, the experience of rewards does not appear to incentivize voting.

The predictions further support these findings. For individuals with weak habits, going from low voting-related rewards to high voting-related rewards is associated with a .063 increase in the predicted likelihood of voting in 2012.<sup>37</sup> The difference is significant (the 90% confidence interval ranges from .031 to .095). For individuals with strong habits, going from low voting-related rewards to high voting-related rewards is associated with a .07 reduction in the predicted likelihood of voting in 2012. This difference is also significant (the 90% confidence interval ranges from -.020 to -.135).

Substantively, these predicted changes are reasonably large. The 2012 likelihood scale ranges from 0 to 1, so an increase of .063 for non-habitual voters is meaningful.

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<sup>37</sup> To predict “low voting-related rewards,” I set 2010 voting to 0 and the composite rewards variable to the 10<sup>th</sup> percentile value. For “high voting-related rewards,” I set 2010 voting to 1 and the composite rewards to the 90<sup>th</sup> percentile value. Habit strength is set to the 10<sup>th</sup> percentile value.

Based on an estimation of the relationship between validated turnout in the 2010 election and self-reported likelihood of voting in the election, every 1% increase in self-reported likelihood for non-habitual voters is associated with an increase in actual voting likelihood of about .87%. Specifically, I estimate a model of turnout as a function of habit strength and self-reported likelihood of voting,<sup>38</sup> and find that that a change of .063 for non-habitual voters (respondents with habit values in the bottom half of the range) at their mean level of self-reported likelihood is associated with a .055 increase in likelihood of actual voting (the 95% confidence interval ranges from 0.041 to 0.070). How the likelihood changes over the years is a separate issue. But if the associations in memory between rewards and voting are easily primed by the voting context in the next election (a basic finding in social psychology and habit research), then the decay in the effects of rewards on likelihood between 2010 and 2012 will not be too great. Overall, increasing psychological rewards from voting significantly increases the likelihood of voting in the future for individuals with weak voting habits.

Habit strength should also influence the future likelihood of voting directly. I hypothesize in H5 that strengthening voting habits increases the likelihood of voting. I estimate this effect with the above model, leaving out the interactions with habit and prior voting (prior voting does not influence voting likelihood directly). The predicted change in likelihood of strengthening voting habits (going from the 10<sup>th</sup> percentile to 90<sup>th</sup>

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<sup>38</sup> I estimate the following parameters:

OLS Coefficients and Standard Errors for 2010 Turnout	
Self-reported likelihood of voting, pre-election	1.44 (0.64)
Habit strength	-2.63 (1.24)
Habit x Self-reported likelihood	3.49 (1.31)
Intercept	-0.49 (0.48)

N=675, LR  $\chi^2=180.52$

percentile value of habit strength) is .111. (The 90% confidence interval ranges from .091 to .131).

To test H6, I examine the effects of several “motives” and “costs” of voting on future likelihood, and determine whether habit strength moderates their effects. Aldrich et al. (2011) find, using a less-than-optimal measure of voting habits, that the effects of many variables in turnout models are moderated by habits. I include variables for general political interest, campaign contact, and strength of party identification in the model. Most of the costs and motives variables in my data are specific to the 2010 election, and are not appropriate to include in the model of 2012 turnout likelihood. However, general political interest and strength of party identification should be mostly time-variant, and so they should influence future turnout intentions. Campaign contact refers to whether anyone from one of the campaigns in the 2010 races contacted the respondent. The propensity to be contacted by a campaign varies some over time, but should be relatively stable. Thus, it should influence future turnout likelihood.

The results in model 1 in Table 9 demonstrate that these costs and motives variables are not significantly associated with likelihood of future voting when they are unmoderated. In the remaining models in Table 9, I interact the costs and motives with habit strength. This allows me to determine whether their effects vary for individuals with strong and weak habits. The results in models 2-5 confirms that habit strength does, in fact, moderate the effects of general political interest, campaign contact, and strength of party identification on future likelihood of voting. The coefficients for the interaction terms are generally negative and significant, and the coefficients for the main effects terms are generally positive and significant. This indicates that each of these variables is

more strongly related to turnout likelihood for individuals with weak habits than individuals with strong habits. The results confirm H6.

**Table 9. OLS Estimates for Likelihood of Voting in 2012**

	[1]	[2]	[3]	[4]	[5]
Habit (post)	0.14 (0.02) ***	0.31 (0.16) **	0.30 (0.16) **	0.21 (0.13) *	0.15 (0.13)
Vote on Nov. 2, 2010	0.11 (0.04) ***	0.14 (0.10) *	0.07 (0.11)	0.06 (0.09)	-0.04 (0.10)
Rewards composite (post)	-- --	-- --	-- --	-- --	-0.53 (0.14) ***
Internal efficacy (post)	-0.17 (0.04) ***	-0.68 (0.09) ***	-- --	-- --	-- --
Anxiety with voting (post)	-0.01 (0.04)	0.16 (0.09) **	-- --	-- --	-- --
Pride with voting (post)	0.32 (0.04) ***	0.49 (0.11) ***	-- --	-- --	-- --
Positive mood (post)	-0.07 (0.07)	-0.06 (0.16)	-0.15 (0.13)	-- --	-- --
Energetic mood (post)	-0.03 (0.06)	-0.13 (0.14)	-- --	-0.42 (0.11) ***	-- --
<b>2-way Interactions</b>					
Vote x Habit (post)	-- --	-0.11 (0.19)	-0.05 (0.18)	-0.04 (0.15)	0.09 (0.16)
<b>Vote x Rewards composite (post)</b>	-- --	-- --	-- --	-- --	<b>0.49 (0.17) ***</b>
<b>Vote x Internal efficacy (post)</b>	<b>0.14 0.04 ***</b>	<b>0.46 (0.12) ***</b>	-- --	-- --	-- --
<b>Vote x Anxiety with voting (post)</b>	<b>0.00 (0.04)</b>	<b>-0.33 (0.12) ***</b>	-- --	-- --	-- --
<b>Vote x Pride with voting (post)</b>	<b>-0.32 (0.04) ***</b>	<b>-0.41 (0.12) ***</b>	-- --	-- --	-- --
<b>Vote x Positive mood (post)</b>	<b>0.06 (0.07)</b>	<b>0.22 (0.19)</b>	<b>0.21 (0.15) *</b>	-- --	-- --
<b>Vote x Energetic mood (post)</b>	<b>0.02 (0.06)</b>	<b>-0.17 (0.17)</b>	-- --	<b>0.18 (0.13) *</b>	-- --
Habit (post) x Rewards composite (post)	-- --	-- --	-- --	-- --	0.79 (0.24) ***
Habit (post) x Int efficacy (post)	-- --	1.02 (0.16) ***	-- --	-- --	-- --
Habit (post) x Anxiety w/ voting (post)	-- --	-0.30 (0.16) **	-- --	-- --	-- --
Habit (post) x Pride w/ voting (post)	-- --	-0.67 (0.21) ***	-- --	-- --	-- --
Habit (post) x Positive mood (post)	-- --	0.15 (0.28)	0.27 (0.23)	-- --	-- --
Habit (post) x Energetic mood (post)	-- --	0.05 (0.26)	-- --	0.60 (0.20) ***	-- --
<b>3-way Interactions</b>					
<b>Habit (post) x Vote x Rewards comp (post)</b>	-- --	-- --	-- --	-- --	<b>-0.73 (0.26) ***</b>
<b>Habit (post) x Vote x Int efficacy (post)</b>	-- --	<b>-0.77 (0.18) ***</b>	-- --	-- --	-- --
<b>Habit (post) x Vote x Anxiety (post)</b>	-- --	<b>0.48 (0.18) ***</b>	-- --	-- --	-- --
<b>Habit (post) x Vote x Pride (post)</b>	-- --	<b>0.59 (0.22) ***</b>	-- --	-- --	-- --
<b>Habit (post) x Vote x Pos mood (post)</b>	-- --	<b>-0.35 (0.30)</b>	<b>-0.37 (0.25) *</b>	-- --	-- --
<b>Habit (post) x Vote x Energ mood (post)</b>	-- --	<b>0.29 (0.27)</b>	-- --	<b>-0.32 (0.22) *</b>	-- --
<b>Motives/Costs</b>					
General political interest	0.02 (0.02)	0.25 (0.07) ***	-0.09 (0.05) *	0.02 (0.06)	0.00 (0.06)
Campaign contact	0.00 (0.01)	0.08 (0.03) ***	0.14 (0.02) ***	0.14 (0.02) ***	0.12 (0.03) ***
Party ID strength	0.01 (0.01)	0.15 (0.04) ***	0.20 (0.04) ***	0.25 (0.04) ***	0.22 (0.04) ***
Habit (post) x General polit. Interest	-- --	-0.29 (0.08) ***	0.11 (0.07) **	-0.01 (0.07)	0.01 (0.07)
Habit (post) x Campaign contact	-- --	-0.09 (0.03) ***	-0.17 (0.03) ***	-0.16 (0.03) ***	-0.14 (0.03) ***
Habit (post) x PID Strength	-- --	-0.17 (0.05) ***	-0.22 (0.05) ***	-0.28 (0.05) ***	-0.25 (0.05) ***
<b>Baseline Rewards - Controls</b>					
Rewards composite (pre)	-- --	-- --	-- --	-- --	-0.01 (0.03)
Internal efficacy (pre)	0.02 (0.03)	0.01 (0.02)	-- --	-- --	-- --
Anxiety with voting (pre)	0.01 (0.01)	0.01 (0.01)	-- --	-- --	-- --
Pride with voting (pre)	-0.01 (0.01)	0.00 (0.01)	-- --	-- --	-- --
Positive mood (pre)	0.00 (0.02)	0.01 (0.02)	0.00 (0.02)	-- --	-- --
Energetic mood (pre)	-0.02 (0.02)	-0.01 (0.02)	-- --	-0.01 (0.02)	-- --
Intercept	0.78 (0.04) ***	0.68 (0.08) ***	0.69 (0.08) ***	0.78 (0.06) ***	0.82 (0.07) ***
N	434	434	456	458	434
Model F	12.7	15.9	21.1	23.6	22.5
Adjusted R-squared	0.35	0.54	0.38	0.41	0.41
Root mean squared error	0.056	0.047	0.054	####	0.05

Notes: Entries are OLS regression coefficients with standard errors in parentheses.

\*\*\*p<.01, \*\*p<.05, \*p<.10, using one-tailed tests. All variables rescaled to range from 0 to 1.

I now estimate models of 2010 validated turnout, to confirm that the results for self-reported likelihood of voting hold for actual turnout. As noted above, I seek to estimate the turnout-boosting effects of the “rewards” that are generated specifically from voting. Individuals might become more politically efficacious or positive in their emotional states for reasons entirely unrelated to voting. Reading political news stories on a regular basis might boost efficacy, or a person might get a raise at work, improving mood. But in this chapter I seek to test the proposed mechanisms underlying voting habit formation; and changes to mood and efficacy from other sources in life are not part of this theory. For the 2012 likelihood estimates, I can isolate the effects of turnout-generated rewards by interacting the rewards variables with the variable for prior turnout, and controlling for baseline (pre-election, pre-voting) rewards values. But for the 2010 turnout model, I cannot do this. Unfortunately, simply including the rewards variables in the model will not provide an optimal test of the rewards hypotheses. The rewards variables are measured with error, since they reflect changes in rewards resulting from voting *and* sources other than voting. But finding effects that are in the same direction as those above would corroborate the results. We know that voting produces psychological rewards from earlier in the chapter—it makes people more efficacious, less anxious on Election Day, and other changes. So if the results demonstrate that individuals with weak voting habits vote at a higher rate when they experience changes in efficacy, Election Day anxiety, etc.—even when it does not originate from voting itself—then this would corroborate the previous findings.<sup>39</sup>

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<sup>39</sup> The survey questions used to measure Election Day anxiety and pride actually reference voting specifically, so there should be less error associated with these variables.

Thus, I include the rewards variables by themselves in the model. (I also interact them with habit strength to test H4b, about the rewards' differential effects for habitual and non-habitual voters.) And I include a wide array of "motives" and "costs" of voting variables in the model. Specifically, I include a variable for interest in the candidates and issues in the elections for governor and House of Representatives.<sup>40</sup> I also include a variable indicating how strongly respondents care who wins the governor's race.<sup>41</sup> And I include variables for strength of party identification used in previous models, campaign contact, level of anger about the candidates and issues in the election, and enthusiasm for the candidates and issues. Finally, I interact the motives and costs variables with habit strength, to test H6, about their differential effects for habitual and non-habitual voters.<sup>42</sup>

The results are presented in Table 10. The models fit the data exceptionally well. In fact, model 2 (and the similarly specified models in Table 11) have R-squared values between .35 and .38. And the models do not even include the optimal measures of voting rewards. Moreover, they do not include variables for prior voting, which may account for additional variation in turnout. This suggests that the current set of variables accounts for a large share of the variation in turnout, and that the model is well specified.

Next, I confirm the previous results related to H5 (that habit strength influences turnout directly). The coefficient for the habit strength index in model 1 represents the

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<sup>40</sup> The question reads, "How interested have you been in the candidates and issues in this year's elections for governor and Congress?" with responses for "very," "somewhat," "not too interested," and "not at all interested."

<sup>41</sup> The question reads, "Generally speaking, how much do you personally care about who wins the election for governor on Tuesday?" with response options for "care a lot," "care a little," and "don't care very much who wins."

<sup>42</sup> The survey also includes questions about perceived closeness of the election and strength of preference for the candidates, but these are not statistically significant in these models. Thus, I exclude them. The strength of preference variable is most likely not significant because it is highly correlated with the variable for interest in the candidates and issues in the election ( $r=.516$ ), which is also included in all models.

most unbiased estimate of the direct effect of habit strength on turnout so far in the dissertation. Prior voting and residential stability variables are not included in this model—which is appropriate since prior voting and residential stability do not influence future voting directly.<sup>43</sup> Again, the results confirm hypothesis 5, that habit strength increases the likelihood of voting. Going from the 10<sup>th</sup> percentile to 90<sup>th</sup> percentile value of the habit strength variable is associated with a 0.134 increase in the likelihood of voting (the 90% confidence interval ranges from .069 to .200).

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<sup>43</sup> Of course, habit strength has an even larger effect on 2010 turnout when considering the indirect effects of habit strength in previous years. Turnout in 2008 influences habit strength in that year, which influences turnout in 2009, which influences the experience of rewards and the strength of habit in that year, which both influence turnout in 2010. But this coefficient nevertheless represents the full direct effect of habit strength.

**Table 10. Probit Estimates for 2010 Turnout (Pre-election data, N=611)**

	[1]	[2]
Habit strength	1.59 (0.40) ***	5.51 (1.58) ***
Rewards composite	-0.40 (0.60)	2.65 (1.58) **
Habit x Rewards composite	-- --	-5.31 (2.44) **
Interest in candidates/issues	1.27 (0.38) ***	3.37 (1.13) ***
Campaign contact	0.09 (0.16)	-0.62 (0.53)
Strength of party ID	-0.11 (0.27)	-0.68 (0.81)
Care who wins gov. race	0.90 (0.31) ***	2.08 (0.93) **
Angry about candidates/issues	0.80 (0.30) ***	0.71 (0.84)
Enthusiastic about cand/issu	-0.50 (0.40)	-4.32 (1.47) ***
Habit x Interest in cand/issu	-- --	-3.04 (1.59) **
Habit x Campaign contact	-- --	1.05 (0.75) *
Habit x Strength party ID	-- --	0.77 (1.12)
Habit x Care wins gov. race	-- --	-1.80 (1.38) *
Habit x Angry cand/issu	-- --	0.16 (1.23)
Habit x Enthusias. cand/issu	-- --	5.59 (1.97) ***
Intercept	-1.27 (0.35) ***	-3.46 (1.03) ***
LR $\chi^2$	136.1	156.5
Pseudo R <sup>2</sup>	0.30	0.35

Notes: Entries are probit coefficients with standard errors in parentheses. \*\*\*p<.01, \*\*p<.05, \*p<.01, using one-tailed tests. All variables rescaled to range from 0 to 1. Rewards composite includes internal efficacy, anxiety with polls (reverse coded), pride with polls, positive mood, and energetic mood.

I now confirm the previous results related to the effects of the rewards variables.

The rewards composite variable is not associated with a higher likelihood of voting in model 1. However, the rewards have different effects for individuals with strong and weak habits. The results in model 2 indicate that the voting rewards are associated with a higher likelihood of voting for individuals with weak habits, and lower turnout for individuals with weak habits. Below I discuss the effects of the specific rewards variables, which individually have a somewhat weaker association with turnout than in Table 9. And although these rewards variables are measured with some error—since they include changes in efficacy, mood, Election Day anxiety and pride unrelated to voting—the results support the theory. And the results further support H4b, that the rewards effects vary by habit strength.

In Table 10, I examine the effects of the rewards together in a composite variable. This is useful for confirming the basic rewards hypotheses, and the results are somewhat easier to interpret. However, each of the psychological rewards is a distinct construct that should be treated as such in models of turnout. In Table 11, I present the results of the identical model as Table 10, model 2, but including the rewards variables separately. As I suggest earlier, assessing the effects of the rewards is much harder than for Table 10. And in model 1, the rewards appear not to be significantly related to turnout, besides anxiety in voting. This result is important to confirm for the theory—because throughout this chapter I have found that Election Day anxiety is one of the important mechanisms underlying habit formation. More than some of the other rewards, voting is associated with large reductions in Election Day anxiety, which makes future a large effect on Election Day anxiety, which increases the likelihood of voting in the future for non-habitual voters.

However, the rewards variables are correlated in these data, which further makes detecting significant effects on turnout difficult. I therefore estimate models with each reward separately in models 2-6. The results in these models demonstrate that Election Day anxiety is associated with a lower likelihood of voting for non-habitual voters, and more positive moods and more energetic moods are associated with higher turnout for non-habitual voters. These results further support the hypotheses that the rewards vary by habit strength level. And they “close the loop” between voting, rewards, and future voting. Specifically, I have found that voting boosts level of mood, and decreases anxiety with voting, and boosts internal efficacy. These results now show that those changes are associated with higher turnout for individuals with weak voting habits. I

accept the strong previous evidence in Table 9 for the internal efficacy effects among non-habitual voters, despite the lack of evidence for that in Table 11—because Table 11 rewards variables have more error.

The results in model 1 also demonstrate that several “costs” and “motives” of voting strongly influence turnout. Individuals who are more interested in the candidates and issues in the midterm races, care who wins the governor’s race, and are angry about the candidates and issues in the election are more likely to vote. The coefficients for those variables are significant at conventional levels. However, the coefficients for campaign contact and strength of party identification are not significant.

**Table 1.1. Probit Estimates for 2010 Turnout (Pre-election data)**

	[1]	[2]	[3]	[4]	[5]	[6]
Habit strength	5.83 (2.18) ***	3.81 (1.22) ***	2.54 (1.13) **	3.11 (1.16) ***	4.87 (1.95) ***	5.24 (1.50) ***
Rewards						
Internal efficacy	-0.89 (1.17)	0.59 (1.04)	-- --	-- --	-- --	-- --
Anxiety with voting	-1.71 (1.23) *	-- --	-1.82 (1.13) *	-- --	-- --	-- --
Pride with voting	-0.78 (1.09)	-- --	-- --	-0.14 (0.95)	-- --	-- --
Positive mood	1.86 (1.74)	-- --	-- --	-- --	2.37 (1.42) **	-- --
Energetic mood	1.72 (1.35)	-- --	-- --	-- --	-- --	2.13 (1.09) **
Habit x Rewards						
Habit x Internal efficacy	0.43 (1.80)	-2.01 (1.61)	-- --	-- --	-- --	-- --
Habit x Anxiety with voting	2.90 (1.88) *	-- --	3.08 (1.74) **	-- --	-- --	-- --
Habit x Pride with voting	0.74 (1.51)	-- --	-- --	-0.11 (1.36)	-- --	-- --
Habit x Positive mood	-1.04 (2.68)	-- --	-- --	-- --	-3.02 (2.17) *	-- --
Habit x Energetic mood	-4.40 (2.12) **	-- --	-- --	-- --	-- --	-4.58 (1.72) ***
Interest in candidates/issues	4.02 (1.21) ***	3.29 (1.10) ***	3.47 (1.10) ***	3.46 (1.10) ***	3.62 (1.14) ***	3.48 (1.11) ***
Campaign contact	-0.69 (0.55)	-0.63 (0.50)	-0.64 (0.51)	-0.71 (0.50) *	-0.59 (0.51)	-0.71 (0.50) *
Strength of party ID	-0.95 (0.85)	-0.32 (0.77)	-0.45 (0.79)	-0.43 (0.77)	-0.76 (0.81)	-0.78 (0.79)
Care who wins gov. race	2.62 (1.04) ***	1.82 (0.90) **	1.74 (0.92) **	1.93 (0.89) **	2.47 (0.95) ***	2.35 (0.94) ***
Angry about candidates/issues	1.50 (0.95) *	0.73 (0.84)	1.25 (0.83) *	1.01 (0.80)	0.81 (0.79)	0.67 (0.80)
Enthusiastic about candd/issues	-4.33 (1.59) ***	-3.77 (1.41) ***	-3.56 (1.44) ***	-3.80 (1.44) ***	-4.12 (1.49) ***	-3.99 (1.46) ***
Habit x Interest in candd/issues	-3.65 (1.69) **	-2.74 (1.56) **	-3.29 (1.55) **	-3.40 (1.56) **	-3.25 (1.59) **	-3.03 (1.57) **
Habit x Campaign contact	1.18 (0.78) *	0.95 (0.71) *	1.14 (0.73) *	1.25 (0.72) **	0.99 (0.72) *	1.18 (0.71) **
Habit x Strength party ID	1.04 (1.18)	0.18 (1.09)	0.54 (1.11)	0.56 (1.08)	0.94 (1.12)	1.02 (1.10)
Habit x Care wins gov. race	-2.48 (1.53) *	-1.32 (1.34)	-1.33 (1.35)	-1.50 (1.33)	-2.24 (1.39) *	-1.98 (1.37) *
Habit x Angry candd/issues	-0.74 (1.35)	0.21 (1.24)	-0.76 (1.20)	-0.40 (1.18)	-0.07 (1.16)	0.01 (1.17)
Habit x Enthusias. candd/issues	5.53 (2.11) ***	4.77 (1.89) ***	4.14 (1.91) **	4.52 (1.94) ***	4.72 (1.97) ***	4.80 (1.93) ***
Intercept	-3.97 (1.40) ***	-2.51 (0.77) ***	-2.02 (0.75) ***	-2.29 (0.77) ***	-3.91 (1.31) ***	-3.44 (1.00) ***
N	611	641	642	642	623	625
LR $\chi^2$	167.7	161.4	160.0	157.4	157.9	167.8
Pseudo R <sup>2</sup>	0.38	0.34	0.34	0.33	0.34	0.36

Notes: Entries are probit coefficients with standard errors in parentheses.

\*\*\*p<.01, \*\*p<.05, \*p<.01, using one-tailed tests. All variables rescaled to range from 0 to 1.

I hypothesize in H5 that the motives and costs of voting influence future turnout more for individuals with weak than strong voting habits. Habitual voters should vote at a higher rate than non-habitual voters regardless of how much they care much about the issues and candidates in the election. Their conscious intentions and election-specific motives should matter less for their turnout.<sup>44</sup> Model 2 provides generally very strong support for this hypothesis. Interest in the candidates and issues is strongly and significantly associated with higher turnout for individuals with weak habits, but lower turnout for individuals with strong voting habits. Habitual voters appear to vote regardless of their interest in the upcoming election. Non-habitual voters who care more who wins are also significantly more likely to vote than non-habitual voters who care less. And habitual voters who care more who wins the governor's race are significantly less likely to vote relative to non-habitual voters who care less. Enthusiasm for the candidates and issues actually has the opposite effect as these variables. Enthusiasm is more strongly associated with turnout for habitual voters.

The results also demonstrate that habit strength and voting intentions are distinct concepts—and that the differences between them are predicted by the theory. Some political scientists might suggest that the voting habit strength index is simply a measure of voting intentions, and that the index is inappropriate to use in models of voting likelihood, or that its effects are inappropriate to compare to those of variables like political interest, efficacy, and feelings about the candidates. Of course, psychologists are now comfortable with the concept of “habit,” and have conducted extensive research on the topic for over a decade, comparing the relative influence of behavioral intentions

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<sup>44</sup> Factors that alter the rewards of voting, and threaten their identity as a voter, or reduce their automaticity in voting directly should still affect their turnout. But interest in the issues and candidates, and election-specific motives should not be very strongly related to their turnout.

on individuals with strong and weak habits for many actions (see Wood and Neal 2007; Ouellette and Wood 1998; Neal et al. 2011), and developed scales measuring habit. One such scale, developed by Verplanken and Orbell (2003), motivates my measurement strategy for voting habits, as I discuss in Chapter 3. For psychologists, intentions are clearly distinct from habits. However, political scientists are apt to be less comfortable with distinctions between intentions and habit—and the concept and measurement of habit more generally—despite what appears to be a slow infiltration of the psychology habit literature into political science (i.e., Aldrich et al. 2011). Some of this discomfort is sure to arise with the voting habit index in this dissertation because several of the items seem “intention-like.”

As I will demonstrate, the habit strength index and voting intentions are correlated, but there is also significant divergence, and the divergence between them is predicted by the theory of habitual voting. In general, self-reported likelihood of voting (i.e., intentions) are correlated with many variables that turnout researchers study. In my data, intentions are highly correlated with interest in the midterm election ( $r=.58$ ), general interest in politics ( $r=.45$ ), how much one cares about the midterm races ( $r=.57$ ), and internal efficacy ( $r=.39$ ). And intentions are correlated with habit strength ( $r=.61$  for 2010 vote intentions and  $r=.48$  for 2012 vote intentions). These correlations are expected, since individuals who are more interested in politics, care more about who wins the election, and have stronger voting habits are all more likely to vote and intend to vote. But each of these concepts is distinct.

With regard to habit strength and intentions, these variables are highly correlated at the upper range of habit strength: individuals with the highest habit scores almost

always report being extremely likely to vote. But at the lower range of habit strength, there is significant variation in intentions. 61% of non-habitual voters (individuals in the bottom half of the habit range of values) report being extremely likely to vote in my pre-election sample. And the correlation between intentions and the automaticity subscale is even lower. This is to be expected because the automaticity items measure an aspect of the ability to vote—and not the desire to vote. The automaticity items measure the ease in traveling to the poll, fitting voting into the daily routine on Election Day, not skipping or forgetting voting even if one is busy, and conscious thought involved in planning the trip. But even for the identity subscale, the correlation with intentions is far from perfect. Many respondents who have low identity scores report being extremely likely to vote.

So who are the individuals for whom habit scores and intentions scores differ? Is it possible that the habit strength index does, in fact, measure intentions but that it cannot correctly classify likely and unlikely voters at the bottom range of the habit index? The results from Tables 9 and 10 demonstrate that the answer is “no.” The individuals who have weak habits and intend to vote are often people who care a lot more than usual about the upcoming election. They should not vote much, have no strong identity as a voter, weak automaticity and efficiency in voting, but still care a lot about that particular election, its candidates, and its issues. And so they report being likely to vote. The coefficients for the interactions in Table 10 demonstrate that for non-habitual voters, the variables for “care wins” and “interest in candidates and issues in midterm” are strongly and highly related to voting. But for habitual voters, they are not. And the interactions in Table 9 demonstrate that for non-habitual voters, interest in politics and strength of partisanship are highly related to voting intentions. But for habitual voters, they are not.

I examine this further by analyzing the difference between values of general political interest and midterm election interest scores for different groups. Individuals with weak voting habits should intend to vote only when they are far more interested in the upcoming election than they usually are in politics. Respondents in my sample have an average general political interest score of .733, an average interest in the midterm election candidates/issues of .744, and an average difference between the two interest variables of .011. I examine the differences for 8 categories of habit strength and likelihood of voting, reported in Table 12. Respondents who report being extremely likely to vote in 2010 have, as expected, higher difference scores. But for individuals with weak habits who report being extremely likely to vote (the category of individuals who diverge on intentions and habit strength), their political interest difference is much higher. Individuals in the lowest quartile of habit strength have an average of .050 higher midterm interest scores than general interest scores. This difference is higher than for any other category, and significant at the .05 level. This analysis demonstrates that voting intentions are distinct from voting habit strength, and that the differences are in theoretically expected ways.<sup>45</sup>

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<sup>45</sup> The voting habit strength index is also more widely applicable and useful in turnout research than a voting intentions variable, since the VHSI is relatively time-invariant. Voting intentions, on the other hand, are related only to the election that they reference.

**Table 12. Mean Difference between Midterm and General Political Interest, by Habit Strength and Voting Likelihood**

Habit	Self-Reported Likelihood		Total
	Not Highest	Highest	
Lowest	-0.089*	0.050*	-0.034
	112	74	186
	0.010	0.031*	0.027
	34	138	172
Highest	-0.167	0.022*	0.012
	10	163	173
	-0.067	0.023*	0.021
	5	186	191
Total	-0.072	0.029	0.006
	161	561	722

\* Difference is significant ( $p < .05$ , one-tailed test)

Cell N is displayed below each mean

Finally, I test hypothesis 7. I propose that the effects of prior voting and residential stability on future turnout are mediated by voting habit strength. In Table 5, I show that the interaction between prior voting and residential stability is not significantly associated with turnout after accounting for the strength of voting habits. This is a first step towards confirming the mediation hypothesis. Alternately, I might include variables for prior voting and residential stability in my final models of turnout and voting likelihood, and determine whether they still influence turnout after accounting for the dynamics of habit formation. In my final 2012 likelihood model, a variable for prior turnout (voting in 2009) is not statistically significant ( $p = .75$ ) when included; neither is a variable for frequency of prior voting from 2004 to 2009 ( $p = .913$ ), nor a variable for residential stability ( $p = .27$ ). In my final 2010 turnout model, a variable for prior turnout (voting in 2009) is not statistically significant ( $p = .77$ ) when included in the model, nor is a variable for residential stability ( $p = .34$ ). However, the variable for frequency of prior voting from 2004-2009 is significant when included in the final turnout model ( $p < .01$ ).

Given that this prior voting variable is an index of all lagged dependent variables for the previous 12 elections, I expect it to account for some additional variation in 2010 turnout. Nevertheless, these results demonstrate that the final models are well specified—to a remarkable degree. Prior voting has dominated the effects of all other variables in previous political science models of turnout. In fact, results by Gerber et al. (2003) demonstrate that voting in the last election is the single largest predictor of turnout. And this variable (turnout in the prior election, in 2009) is not significantly associated with future likelihood of voting or turnout in my data after accounting for the dynamics of habit formation. The result suggests that prior voting does not influence turnout directly. Residential stability, which also dominates many existing turnout models, also does not influence turnout directly.

## **Discussion**

In this chapter, I find that the strength of voting habits is central to understanding the over-time causes of turnout. Specifically, the results demonstrate that (1) individuals with stronger voting habits are far more likely to report being likely to vote in future elections, and are also more likely to actually go to the polls in the 2010 election. (2) Moreover, voting habits are largely formed through repeated voting, especially in a stable location over time, as hypothesized. (3) Voting and voting in a stable location appear not to influence turnout directly, but indirectly by strengthening voting habits, which then powerfully influence turnout. (4) And once voting habits are established, residential moves appear to have a minimal effect on turnout. But for non-habitual voters, context disruptions can be quite detrimental for habit formation and turnout.

Additionally, the results demonstrate that (5) voting generates several specific psychological rewards: increased internal efficacy, decreased Election Day anxiety, and a more positive mood. (6) And voting for the winning candidate moderately increases Election Day pride and energetic mood. (7) I do *not* find that voting habit strength moderates the effects of turnout on the experience of psychological rewards. In other words, individuals with weak habits do not have greater changes in internal efficacy, Election Day anxiety and pride, and mood after leaving the polls than individuals with weak habits. (8) However, voting habits do moderate the effects of the rewards on future turnout. The results demonstrate that most of the psychological rewards I examine—other than Election Day pride—are more strongly related to turnout for non-habitual voters. Conversely, for those with strong voting habits, experiencing psychological rewards after going to the polls did not boost the likelihood of voting much—and often decreased it. Thus, it appears as though the rewards of voting incentivize voting in the early stages of habit formation, as hypothesized. Overall, the results provide strong evidence for the hypotheses related to all but one of the causal paths in the theory.

Finally, I demonstrate in this chapter that (9) most of the “costs” and “motives” variables examined in political science models of turnout are moderated by habit strength. As expected, election-specific motives like interest in the candidates and issues powerfully affects turnout—but only for individuals with weak habits. For strongly habitual voters, such motives do not shape their turnout much. This final result corroborates those by Aldrich et al. (2011) who also find evidence of this causal path, but using a far-from-optimal measure of habit.

In Chapter 2, I argue that the primary deficiencies in prior turnout research relate to model specification and underdeveloped theories. Many variables are included in models of turnout in a kitchen-sink approach that assumes each has a direct effect on turnout. Such models also assume that the predictors of voting are exogenous to turnout, and not shaped by turnout themselves. This chapter's results demonstrate that few variables influence turnout in a direct and unmediated and unmoderated manner. Gerber et al. (2003) and others have found that voting in the last election is one of the largest—if not the single largest—predictor of turnout. But the findings in this chapter show that prior turnout does not influence future turnout directly, but instead is mediated by the strength of voting habits. Specifically, voting—especially in a stable location over time—strengthens habits, which then influence turnout directly in the future. I find that measures of past voting are not significantly associated with turnout or future likelihood of voting after accounting for habit-related dynamics. The results also suggest that future turnout studies should account for the strength of voting habits. Doing so generates models with enormously improved fit relative to existing models in political science.

## Chapter 5

### **Theory-Based Targeting: Effects of Actions and Rewards Messages on Habitual and Non-Habitual Voters**

In the 2012 election cycle, campaigns, outside organizations, and parties spent approximately \$7 billion dollars in federal elections, about as much as the quarterly profits of Chevron, the second most profitable company in America that year (Harper 2013; Fortune 2012). Between \$300 and 500 million of that spending was estimated to be on mobilization through direct mail, phone calling, and in-person canvassing.<sup>1</sup> And groups increasingly utilize “microtargeting” strategies to mobilize potential voters. Rather than rely on a uniform mobilization strategy for all voters, campaigns frequently use information from voter registration lists and surveys to tailor messages for specific groups. Microtargeting efforts have grown enormously since 2004, when George W. Bush’s campaign was credited with effectively using information about residents in states like Iowa and Florida to direct mobilization efforts at its likely supporters and ignore individuals likely to vote for Kerry (see Barreto and Nuño 2011; Dreazen 2006).

Similarly, companies develop commercial messages with different words, tones, and emotion-provoking images for different groups of potential customers, and include them in different types of media—television, radio, and newspaper—at different times, in order to increase aggregate sales most effectively. Individuals likely to purchase a Ford Focus do not respond to the same advertising strategies as those who buy Ford trucks. But while there are numerous studies on consumer targeting methods, there is strikingly

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<sup>1</sup> Although many campaigns and political groups do not report expenditures by type of activity, information from presidential campaigns and consultants (Center for Responsive Politics 2013; Bergo, Armendariz, and Perry 2006) suggests that payments for direct mail, phone calling, and canvassing likely comprised between 4 and 7 percent of election-related spending in the 2012 cycle, or about \$300 to 500 million.

little research in political science on how different groups of potential voters respond to different types of mobilization messages. Studies demonstrate which modes of contact (i.e., phone calls or face-to-face contact) mobilize individuals most effectively, and have discovered several categories of message content that boost turnout. However, prior research often treats the population to be mobilized as homogenous and fails to differentiate between the types of messages that influence some people more than others. If habitual and non-habitual voters are motivated to vote by different factors, as demonstrated in previous chapters, then mobilization messages should have enormously different effects on individuals with strong and weak voting habits.

Understanding heterogeneity in message effects is essential both for real-world GOTV campaigns and for testing the theory of habitual voting. For GOTV campaigns, utilizing different mobilization strategies for different subgroups of voters can boost aggregate turnout much more efficiently than a uniform strategy for all potential voters (Imai and Strauss 2011). In elections decided by a few percentage points, using effective targeting strategies can certainly change the outcomes.

Moreover, several propositions in the theory of habitual voting can be tested by examining effects of mobilization messages for individuals with different voting habit strengths. I argue in Chapter 2 that habitual and non-habitual voters go to the polls for distinct reasons. Messages that highlight specific concrete Election Day actions required to vote should have different effects for habitual and non-habitual voters, since Election Day actions are less automatic for non-habitual voters. And messages that remind individuals of the psychological rewards of voting might boost turnout more for habitual voters, since they are intimately familiar with how those rewards are experienced. All

messages should influence the intention to vote more than actual turnout for non-habitual voters because they cannot translate their intentions into behavior as effectively.

Although this chapter's empirical findings do not all conform to expectations, they nevertheless clarify several important psychological mechanisms underlying habitual voting and voter mobilization message effects. Prior mobilization studies have rarely examined these underlying mechanisms. In particular, research on actions-related messages (Nickerson and Rogers 2010; Goldstein et al. 2007) has not estimated the message effects separately for habitual and non-habitual voters or examined attitudes or psychological factors that may be responsible for the effects.

In this chapter, I draw on the theory of habitual voting to derive hypotheses about the types of messages that should influence individuals with strong and weak voting habits, and test them using a unique experimental design. Specifically, I examine a message that highlights the psychological rewards of voting (i.e., mood enhancements, increased political efficacy, and decreased anxiety associated with going to the polls) and a message focusing on concrete Election Day actions (i.e., travel and planning-related behaviors). I embed the messages in a survey experiment with Minnesota registered voters conducted during the state's 2010 gubernatorial election, and measure turnout using official state records following the election. I utilize the Voting Habit Strength Index (VHSI) to assess the effects of the messages on habitual and non-habitual voters. The results demonstrate that both the rewards and actions messages have negligible effects on turnout when estimated for all potential voters, but the actions message has significantly different effects for individuals with strong and weak voting habits. In particular, the actions message strengthens voting intentions for non-habitual voters, but

not for habitual voters. I find that the actions message *decreases* non-habitual voters' likelihood of actually voting. Overall, the results confirm that voting habit strength is an important moderator of the effects of actions-related voter mobilization messages.

Understanding how and why habitual and non-habitual voters respond to these messages has several implications for real-world GOTV campaigns, research on turnout and mobilization, and also real-world GOTV strategies. The results demonstrate, for the first time, that the strength of voting habits is an important moderator of the effectiveness of actions messages. Habitual and non-habitual voters respond differently to different types of message content. And non-habitual voters are clearly the most difficult to mobilize with any form of content examined in this chapter. This suggests that future research can better explain variation in mobilization and turnout effects by explicitly accounting for heterogeneity by strength of voting habits.

Scholars might also develop more effective message content by drawing on the findings about the psychological mechanisms underlying the actions message effects—especially the mechanisms related to centrality of voting to the self-concept. For GOTV campaigns, utilizing a measure of habit may allow them to boost aggregate turnout at a lower cost. Additionally, campaigns should not risk targeting non-habitual voters using an actions message like the one in this study. The actions message demobilized individuals with weak habits. Such messages that highlight Election Day actions and mental planning processes have already been adopted by some campaigns, and touted by some campaign consultants.<sup>2</sup> But if actions messages demobilize non-habitual voters,

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<sup>2</sup> The Analyst Institute, which assists campaigns and organizations in mobilizing voters, now routinely advocates using some actions-planning messages in GOTV phone scripts and mailers. An Analyst Institute presentation on “best practices” that references these messages is available at: [http://uploads.democracyforamerica.com/0008/6279/Analyst\\_Institute\\_DFA\\_GOTV.pdf](http://uploads.democracyforamerica.com/0008/6279/Analyst_Institute_DFA_GOTV.pdf)

this strategy is risky. I conclude the chapter by discussing implications for mobilization research and real-world GOTV strategies in further detail, and appropriate methods for estimating mobilization effects.

### **Background and Prior Research**

Political scientists have studied the effects of voter mobilization techniques for decades. Despite contributing extensively to our knowledge of the effectiveness of different modes of contact (i.e., face-to-face canvassing, phone calling, and direct mail), mobilization researchers frequently ignore the content of messages. And studies typically fail to account for heterogeneity in message effects. I argue that mobilization researchers would be benefited enormously by developing more comprehensive theories of message effects rooted in the psychological mechanisms underlying those effects, and using those theories to derive hypotheses about how different messages influence different subgroups of voters. In this section I describe the three waves of prior mobilization research, their omissions, and how the current study addresses those omissions.

The first wave of mobilization studies focused on the mode of contact, including direct mail, phone calling, text messaging, email, and door-to-door canvassing. In his classic field experiment, Gosnell (1927) examined the effects of mobilization mailings on turnout in Chicago. Randomly assigning households in city blocks to treatment and control groups, with the treatment group receiving a mailing, he found that mailings significantly boosted turnout. Many decades later, Green and Gerber (2000a, 2000b, 2001) assessed the effects of door-to-door canvassing, direct mail, and telephone calls on turnout, improving upon earlier research designs by statistically accounting for the failure

to treat some individuals in the treatment group<sup>3</sup> and relying on much larger samples. The authors found personal face-to-face contact to be most effective. Studies on the mode of mobilization contact also examined email and text messaging strategies.<sup>4</sup>

Overall, research on mode of contact has contributed significantly to our understanding of how to boost turnout among potential voters. However, the literature includes several conflicting findings. Numerous studies find that face-to-face contact stimulates turnout most effectively, but several also find phone calls to boost turnout (e.g., Nickerson 2007b) and instances of highly effective direct mail mobilization (e.g., Gerber, Green, and Larimer 2008). The discrepancies may result in part from researchers ignoring the content of the message. The first wave focused entirely on *how* to contact voters, ignoring *what to say* to voters. This focus is understandable given that many mobilization researchers work closely with actual GOTV campaigns to carry out their field experiments; campaigns can randomize the type of contact and set aside some households to form a control group on behalf of researchers, but cannot as easily control the specific words and messages that staff deliver to potential voters. It may be that studies showing phone calls to be most effective rely on convincing phone scripts with strong messages, and studies showing phone calls to be least effective rely on poor phone scripts.

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<sup>3</sup> Assignment in many field experiments on mode of contact is often based on household, but prior studies often failed to account for household members not exposed to the contact. Field experiments now routinely estimate intent-to-treat (ITT) and treatment-on-treated (TOT) effects for this reason.

<sup>4</sup> Nickerson (2007a) and Bennion and Nickerson (2010) assess the effectiveness of email-based inducements to vote and register, finding that emails do not effectively stimulate either turnout or registration. Dale and Strauss (2009) demonstrate that text messages can increase turnout by about 3%. And Nickerson (2007b) finds that professional phone banking for GOTV campaigns can be just as effective as volunteer phone banking; the quality of the mobilization phone call determines its effectiveness.

Researchers have begun to examine the content of mobilization messages more explicitly in recent years. To date, the largest body of research on message content explores the effects of “social pressure” messages. Gerber et al. (2008) randomly assigned 180,002 households to receive successively more severe social pressure mailings, including ones displaying the voting histories of household members, and mailings displaying neighbors’ voting histories and threatening to reveal to the neighbors whether they vote in the upcoming election. Gerber and colleagues found that the most severe social pressure message—displaying neighbors’ voting histories—increased turnout by 8% relative to the control group receiving no mailing. This effect is considerable for such a cheap mode of contact, postcards.<sup>5</sup>

Messages rooted less explicitly in social pressure can also boost turnout. Particularly relevant to the current study, Nickerson and Rogers (2010) find that messages that highlight the concrete actions involved in voting, by asking individuals how, when, and where they will vote, significantly boost turnout. The actions-planning messages increase turnout by 4.1% among those contacted, with the effect concentrated among individuals in single-voter households. Goldstein et al. (2007) also find evidence of large turnout-enhancing effects of actions-planning messages.

Despite advances in research on message content, the mobilization literature has failed to explore message content in sufficient depth. Much of the research focuses on social pressure mailings, specifically postcards that display people’s voting histories or other “social norm” messages. Researchers argue that revealing a person’s voting history on a postcard induces her to comply with the social norm of voting (see Gerber et al.

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<sup>5</sup> More recently, studies demonstrate that social pressure can be applied in a gentler, less shaming-based manner while still boosting turnout (see Mann 2010; Grose and Russell 2008; Panagopoulos 2010).

2008). But prior studies, whether on social pressure or other message types, usually fail to explicate and test the underlying psychological mechanisms. Perhaps feelings of shame mediate the effects of the social pressure messages. Or, vote history mailings may influence turnout for people who do not strongly embrace voting as a social norm, but vote because political participation is an essential component of their identities. In general, mobilization studies would be benefited by the development of more comprehensive theories of message influence rooted in psychological theories.

The second significant shortcoming in prior mobilization research is the lack of attention to heterogeneous treatment effects. Researchers often fail to identify the subgroups that are most influenced by a particular form of mobilization contact or message. Several studies highlight the importance of identifying heterogeneous treatment effects in mobilization studies. Imai and Strauss (2011) demonstrate that using different forms of mobilization contact for different subgroups can increase aggregate turnout for a GOTV campaign much more efficiently than a uniform mobilization strategy. And the authors outline the most appropriate statistical approaches for assessing treatment effects among subgroups (also see Feller and Holmes 2011). However, these studies offer little guidance in identifying the subgroups that may be most influenced by certain messages more than others.

In a more theory-driven study on heterogeneous mobilization effects, Arceneaux and Nickerson (2009) develop a model that considers who is mobilized by face-to-face contact (“high-propensity” or “low-propensity” voters) and when they are mobilized (high or low salience elections). Re-analyzing results from 11 randomized face-to-face voter mobilization field experiments to test the model, they find that in-person canvassing

stimulates turnout the most among low-propensity voters in prominent elections. High-propensity voters are stimulated the most in lower salience elections. While Arceneaux and Nickerson (2009) take an important step towards identifying the types of people most likely to be mobilized in particular electoral contexts, they ignore the issue of message content entirely. Their results examine face-to-face contact, failing to consider the types of words, phrases, tones, styles, and content of communication may be more influential for some voters more than others.

Recent studies have begun to explore subgroup differences in message effects further. Gerber and Rogers (2009) find that messages claiming the upcoming election will have high turnout influence infrequent voters more than frequent voters. Feller and Holmes (2011) re-analyze data from the Gerber et al. (2008) field experiment and demonstrate that the strongest social pressure messages have a greater effect on frequent voters. Others have examined the effects of messages on single-voter households (Nickerson and Rogers 2010) and intermittent voters (Niven 2004).

However, the explanations offered for these subgroup differences are frequently post hoc: they are generally presented as “findings” rather than tests of specific hypotheses about the effects of messages on different subgroups derived from broader theories. Imai and Strauss (2011) specifically warn about the ability to draw conclusions after locating subgroup differences in a post hoc manner: although the mobilization mode/message is randomly assigned in experiments, the subgroup characteristics are not. They note that researchers discovering subgroup differences after data is collected may be more likely to find statistically significant interaction effects resulting from overfitting the model rather than identifying true underlying interactions. Moreover, researchers

usually do not explicate or test the psychological mechanisms underlying the differential mobilization effects.

There is another common theme in recent studies on subgroup differences in mobilization message effects: many find that voting history (or a variable related to voting history) determines how, when, and who is mobilized by different messages. The education of voters, their geographic location (urban vs. rural residence), type of election (local vs. midterm vs. presidential), racial or ethnic background, and income levels usually do not moderate the effectiveness of messages. The underlying moderating variable these studies seek to capture is most likely voting habit strength.

The final shortcoming in prior studies on subgroup differences in message effects is that most restrict their samples to unrepresentative groups of voters. Importantly, researchers often use the variable of interest in the current study—frequency of prior voting (a rough proxy for the strength of voting habits)—to limit their samples, thereby limiting their ability to generalize about the effects of the messages on frequent and infrequent voters. Researchers sometimes exclude individuals who always votes, individuals who never vote, and young voters. Nickerson and Rogers (2010) only include individuals who voted in one or no primary elections in the preceding eight years, thereby excluding many frequent voters. This exclusion eliminates 29.6% of their sample. Gerber et al. (2008) make several exclusions, including one based on prior voting.<sup>6</sup> Panagopoulos (2011) only examines people who voted in a previous election (since his gratitude mailings thank them for voting before) and who live in single-voter

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<sup>6</sup> The authors remove everyone who did not vote in the 2004 general election, people who had a high probability of voting absentee, those who had a high probability of voting in the Democratic primary rather than the Republican primary, and everyone who lived in a mailing route with few numbers of households (see Gerber et al. 2008, 36-7).

households. Restrictions based on frequency of prior voting are the norm in mobilization studies.

Such sample exclusions are often justified on the grounds that mobilization contact is not effective for individuals who always vote and never vote.<sup>7</sup> However, researchers generally do not cite studies in support of their claims about the mobilizability of frequent and infrequent voters. Because of such exclusions, political scientists are still unsure how mobilization messages influence clearly habitual and clearly non-habitual voters.<sup>8</sup> Most importantly, the exclusions produce unrepresentative samples, yielding results that do not generalize to broader populations. Are “frequent voters” in a sample without true frequent voters equivalent to “moderately frequent voters” in the population? To limit the costs of conducting field experiments, it may be sensible to exclude individuals thought to be difficult to mobilize. But high-percentage sample exclusions based on frequency of prior voting pose significant threats to external validity. I overcome this limitation in my study by contacting a random sample of registered voters in Minnesota, making no explicit sample exclusions during recruitment.<sup>9</sup> I discuss possible implicit sample restrictions generated by the research design in the conclusion.

In sum, there is a rapidly growing body of studies on mobilization and message content in political science, but their findings do not build effectively on each other due

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<sup>7</sup> For example, Nickerson and Rogers (2010, 195) justify their exclusion “to avoid targeting citizens who were extremely likely to vote, by definition unlikely to be responsive to the experimental procedures.”

<sup>8</sup> Some of the claims about the mobilizability of frequent and infrequent voters are surely accurate. The results of the current study, in fact, suggest that voters with the weakest voting habits are least likely to be mobilized. However, many of the claims may not be accurate. I find that habitual voters can be effectively mobilized in midterm elections.

<sup>9</sup> I recruit participants from a list of registered voters, which closely resembles the Minnesota population, due in part to the ease of Election Day registration in the state.

primarily to their lack of theoretical framework. A theory of habitual voting focused on the psychological causes of voting habit formation would provide guidance about the types of messages that effectively mobilize individuals. A focus on habit formation is beneficial for several reasons. First, prior studies have not yet effectively defined “habitual voters” or demonstrated how to identify them empirically (for one of the more effective attempts see Aldrich et al. 2009). Studies refer to presidential election voters, “seldom” voters, intermittent voters, major election voters, and disaffected voters. Malhotra et al. (2009) even discusses “habitual voters,” but this group comprises only about 3% of their sample.<sup>10</sup> In general, every individual has an underlying level of voting habit strength that many mobilization studies refer to, but have not conceptualized or measured effectively. In previous chapters, I conceptualize and measure habit strength—a psychological construct—using the Voting Habit Strength Index (VHSI).<sup>11</sup> This chapter’s analysis relies on the VHSI.

Additionally, deriving propositions about message effects using a theory of habitual voting directs needed attention to the psychological mechanisms underlying those effects. Prior research is relatively silent on how individuals process mobilization messages. Ultimately, to understand how and when messages mobilize people requires devoting attention to voters’ psychological processing and not only their readily identifiable characteristics (like education and age) and observable electoral behavior (like frequency of past voting).

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<sup>10</sup> Malhotra et al. (2009) decide, somewhat arbitrarily, that “habitual voters” are people who vote in all elections.

<sup>11</sup> The construction of the index is motivated by Verplanken and Orbell’s (2003) index for daily and weekly habits. See Chapter 3 for a further discussion, and for results of the validity and reliability analyses.

## **Hypotheses**

In this section I draw on the theory of habitual voting detailed in Chapter 2 to derive hypotheses about the types of mobilization messages that stimulate turnout the most for voters at different levels of voting habit strength. Specifically, I offer propositions about the effects of actions- and rewards-related messages on individuals with strong and weak voting habits. I explain why the strength of voting habits is an important moderator of the effectiveness of these messages.

Results from the previous chapter demonstrate that voting on Election Day induces several distinct psychological changes that make voting more likely to occur in the future. Voters experience temporary improvements in mood, a greater sense of political efficacy, and less anxiety associated with Election Day activities. These are the primary “rewards” of voting I examine. And voting produces more longstanding changes in an individual’s identity as voter and automaticity in voting-related acts like traveling to the polls. But is it possible to induce such psychological changes in individuals without them actually voting? Can mobilization messages delivered prior to Election Day boost potential voters’ automaticity in voting—as if they voted—or produce a similar turnout-enhancing effect as experiencing the rewards of voting?

I examine two mobilization messages that should boost turnout by inducing such changes. First, one could help potential voters form a mental plan to vote by emphasizing the concrete actions in going to the polls. Emphasizing the actions in voting should make them more automatic to carry out on Election Day, thereby increasing the likelihood of actually voting. Second, one might tell potential voters that they will experience psychological rewards after going to the polls. Recipients can be told that

they will feel more efficacious, less anxious, more proud, and experience an improved emotional state after voting.

Regarding actions-planning messages, research demonstrates that helping people form mental plans to carry out an action, by discussing the concrete where/how/when aspects of the action, mentally prepares them to carry it out when the real-life setting is encountered. Such a plan should increase the likelihood that the action occurs.

Gollwitzer (1999) summarizes the extensive body of research on the effect of forming concrete action plans, which he describes as “implementation intentions” (see also Sheeran and Orbell 2002).

Forming implementation intentions has been shown to increase the likelihood of voting in two studies. Nickerson and Rogers (2010) conduct a field experiment in which 287,228 registered Pennsylvania Democrats are contacted by phone, randomly assigned to receive either an implementation intentions script, no contact, or simply asked whether they will vote. Subjects in the implementation intentions condition are asked what time they would vote, where they would be coming from, and what they would be doing before they left for the polls—intended to help them form a mental voting plan. The implementation intentions script increased turnout by 4.1% relative to the control, and 9.1% among single-voter households. Imai et al. (2007) also found that messages to form implementation intentions boosted turnout in two field experiments. Imai et al. estimated treatment effects in the 3-5% range for in-person voting, and slightly greater effects for early voting.

Using imagery-based wording in an actions-planning message should enhance the message’s mobilization effect. Prior research demonstrates that messages that utilize

imagery wording influence behavioral outcomes at a significantly higher rate than messages that provide the same information in a non-imagery-based format (see Gregory, Cialdini, and Carpenter 1982; Thomas, Hannula, and Loftus 2007). Therefore, I offer the following hypothesis:

H<sub>1</sub>: Imagery-based messages that facilitate the formation of a mental voting plan by highlighting the concrete actions in voting (“actions-planning messages”) increase the likelihood of voting.

An imagery-based actions-planning message is likely to have different effects on turnout for habitual and non-habitual voters. However, the theory of habitual voting presented in Chapter 2 does not offer a clear hypothesis about the direction of these subgroup effects. Thus, I offer two competing hypotheses:

H<sub>1a</sub>: Actions-planning messages increase turnout more for individuals with *weak* than strong voting habits.

H<sub>1b</sub>: Actions-planning messages increase turnout more for individuals with *strong* than weak voting habits.

Voting is less automatic for individuals with weak voting habits. Those individuals must think more about how, when, and where to vote in order to successfully cast a ballot. An actions-planning message may therefore help non-habitual voters overcome their deficit in voting automaticity. Overcoming the deficit in voting automaticity should make them more likely to intend to vote, and subsequently to go to the polls. These propositions about the automaticity-related mechanisms underlying the message effects on intentions and turnout are most consistent with H<sub>1a</sub>.

However, other propositions about automaticity in the theory of habitual voting suggest that the actions-planning message might have a greater turnout-enhancing effect for habitual voters. Voting is more automatic for individuals with strong than weak

voting habits, so they should be able to translate their intentions into actual turnout more easily. The actions-planning message may strengthen the intention to vote among both groups, but if non-habitual voters cannot translate those intentions into turnout as easily, then the message may actually boost turnout more for habitual voters. These propositions would support H<sub>1b</sub>.

I make two additional hypotheses about the psychological mechanisms underlying the message effects, which may be clear by now. I clarify them here. I assume here that the actions-planning message influences intentions to vote and turnout primarily by making voting more automatic—and *not* by influencing individuals' identities as voters. Recall that automaticity and identity are the two dimensions of voting habit strength. The dimensions are correlated, but some of the rewards of voting influenced one more than the other. I offer the following hypothesis:

H<sub>1c</sub>: Actions-planning messages increase the likelihood of voting by making Election Day actions more automatic.

The second hypothesis I make about the mechanisms underlying the messages concerns the distinction between intentions to vote and actual turnout. Since voting is less automatic for non-habitual voters, and since they cannot translate their intentions into actual voting behavior as easily, I argue that mobilization messages that strengthen non-habitual voters' *intention* to vote will have a weaker effect on *actual* turnout for them. For non-habitual voters, the effects of mobilization messages should always be greater for the intention to vote than actual voting. Although the data in this study allow me only to test this proposition for rewards and actions messages, it should also apply to all types of mobilization messages.

H<sub>2</sub>: For individuals with weak voting habits, all mobilization messages increase the *self-reported* likelihood of voting more than the likelihood of actually voting.

As will be clear in the discussion of the results, such a distinction would have enormous implications for research on turnout. Turnout research often uses intentions to vote (i.e., self-reported likelihood of voting) as the dependent variable—under the assumption that factors that strengthen intentions have similar effects on actual turnout for all subgroups.

I now offer several hypotheses about the effects of rewards-related messages. Results in Chapter 4 indicate that when individuals with weak habits vote, the rewards they experience—greater political efficacy, decreased Election Day anxiety, improved mood, increased pride in some situations—are more likely to boost their likelihood of future voting. However, when non-habitual voters do *not* make it the polls, the psychological changes they experience generally do not influence likelihood of future voting. This finding makes sense: psychological changes due to non-voting life circumstances should not significantly influence turnout. So the findings do not provide clear guidance about the expected effects of rewards-related mobilization messages, which are intended to be a substitute for experiencing rewards as a result of actually voting.

Nevertheless, a key aspect of the theory of habitual voting has not yet been tested that has several important implications for rewards-related messages. The psychological rewards of voting become causally linked to the act of voting as one votes more. After several repetitions of voting and the subsequent experience of the rewards, the rewards become causally linked, presumably by being recorded in procedural memory. Habitual voters are intimately familiar with how those rewards are experienced, so appealing directly to them by reminding them of those feel-good aspects of going to the polls

should stimulate turnout. Essentially, if people vote to “feel good” and habitual voters recognize this most, then reminding them of that should boost turnout primarily for them.

H<sub>3</sub>: Messages highlighting the psychological rewards of voting (especially mood, efficacy, and Election Day anxiety and pride) increase the likelihood of voting.

H<sub>3a</sub>: Messages highlighting the psychological rewards of voting (especially mood, efficacy, and Election Day anxiety and pride) increase the likelihood of voting for habitual voters more than non-habitual voters.

Notably, when asked how good they would feel throughout Election Day if they voted, 75.7% of habitual voters in the survey of Minnesota registered voters described below responded with “extremely good” or “very good.” Only 51.4% of non-habitual voters responded with “extremely good” or “very good.”<sup>12</sup> Therefore, messages emphasizing the primary psychological rewards of voting should have a weaker effect on turnout for non-habitual voters.<sup>13</sup>

Similarly, telling a person who has never volunteered at a food bank that she will “feel good” if she volunteers may not immediately induce her to volunteer. She can

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<sup>12</sup> This question was asked of a randomly selected one-third of the sample. Habitual voters here are defined as respondents with habit strength values above the median value. See Chapter 3 for a detailed description of the coding of habit strength. Appendix Table 2 provides a brief description of the items.

<sup>13</sup> Social psychology and neuroscience research on habit formation demonstrates that when behaviors become fully habitual (and thus automatic), individuals actually experience lower contingency between a behavior and its rewarding outcomes (see Wood and Neal 2009, 8). This notion is not mutually exclusive with the proposition above for two reasons. First, voting is different than most behaviors studied by psychologists and neuroscientists; it never reaches the highest levels of automaticity. Habitual TV watchers come to be less aware that TV watching produces certain rewards with repetition (lower experienced contingency). But because voting never reaches the same high level of automaticity as TV watching, habitual voters will still be somewhat aware that the action produces the rewards. They will never come to dissociate them; the causal linkage never disappears entirely.

Second, levels of experienced contingency are not entirely related to the types of messages/stimuli that incentivize behavior. Even if a habitual voter does not *experience* as strong of a causal linkage between the rewards and voting, she is *more aware* that the rewards and voting are related. So reminding her of the mood-, efficacy-, and emotion-related rewards still incentive voting more than if she were a non-habitual voter. As Wood and Neal (2009, 8) note, “When experienced contingency is low, rewards incentivize performance but are not highly salient.”

guess that she will experience positive emotions from volunteering (primarily as a result of acting in accordance with social norms) but since she has not experienced those emotions and does not causally link them to the act of volunteering yet, this strategy should be less effective than for habitual food bank volunteers. Habitual food bank volunteers know exactly how they will feel during and after their volunteering; so reminding them of the feel-good rewards will be more effective.

Additionally, even if non-habitual voters are motivated to vote with messages about mood and other rewards, they are less able to effectively translate their intentions into actual voting behavior. As just discussed, voting is less automatic for non-habitual voters, who must dedicate more thought and effort to plan how and when they will fit voting into their daily routine and carry it out. If small impediments to voting present themselves that day, they are more likely than habitual voters to skip voting. Of course, most people who strongly intend to vote will usually do so; but the lack of automaticity in voting-related planning tasks is likely to prevent at least some non-habitual voters from translating their intentions into actual voting.

Before testing these hypotheses, it is worth considering why GOTV campaigns might seek to stimulate turnout among non-habitual *and habitual* voters. If habitual voters always vote, why assess the effects of mobilization messages on their turnout? The answer is that no one always votes. Even individuals in the 90<sup>th</sup> percentile of voting frequency in my sample only voted in an average of 50% of primary and general elections they were eligible to vote in from 2004 and 2009. 50% is an impressive rate for all primary and general elections in local, midterm, and presidential election years. But even voters with a strong habit of voting might still be mobilized.

## Method

I used state voting records to call a random sample of Minnesota households with at least one registered voter to recruit them to take an online survey prior to the election. See Chapters 3 and 4 for a description of the survey administration. Besides registered voters who did not list a phone number and therefore could not be contacted, I made no sample exclusions during recruitment. Individuals who agreed to participate were emailed the secure link to the online survey 5 days prior to the election. Individuals who did not complete the survey by the day before the election were emailed a reminder.

Respondents were randomly assigned to receive one of three versions of the survey, which were identical other than the text of mobilization message (and instructions and three associated questions) included near the end of the survey.<sup>14</sup> One version contained the actions message, one contained the rewards message, and one contained no mobilization message. The instructions for the actions and rewards message read:

*Next there will be a short description of voting on Election Day, followed by three questions. It is very important that you read the description and the questions carefully since they are about the upcoming election.*

The actions-planning message then included the following text and questions:

*Please take a moment to think about how you would vote on Election Day. Think about what time voting would fit best into your daily routine – whether it would be in the morning, at lunch, or near the end of the day before polls close. What time of day will you leave for the polls on Tuesday if you vote?*

*Now imagine where you will be when you leave for your polling place. Picture yourself getting ready to leave, and then traveling to the polls – whether you drive, walk, or get there some other way. How will you get to the polls on Tuesday if you vote?*

*Now think about getting to the building where you will vote. You give your name to the poll worker, get your ballot, and fill it out. Finally, you get your “I Voted”*

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<sup>14</sup> Randomization was carried out in a block design. University of Minnesota Survey Services formatted the survey for online presentation, administered the randomization, and collected survey responses.

*sticker, put it on, and leave the building having voted. If you need to, close your eyes for a second and hold this image in your mind. What will you be doing before you leave for the polls on Tuesday?*

The rewards message included the following text and questions:

*Please take a moment to think about why you would vote on Election Day. Many are saying that this will be one of the most important elections for Minnesota and the country in recent years. Think about how the election could change the balance of Congress for Democrats and Republicans. How important is it for you to express your preferences by voting this year?*

*Now think about how close the election for governor might be. Imagine the way you will feel if you vote for your preferred candidate. How do you think you would feel knowing that you voted for your preferred candidate for governor on Tuesday?*

*Finally, imagine the feelings you will experience throughout the day if you vote. Think about receiving your “I Voted” sticker and what you might say if you talk to your family members, coworkers, or friends. You feel proud that you did your civic duty, and do not feel guilty for not voting. If you need to, close your eyes for a second and hold this image in your mind. How good do you think you will feel throughout the day on Tuesday if you vote?*

The actions-planning message is intended to highlight the concrete actions involved in voting on Election Day to help respondents plan when, where, and how they would vote. It is roughly modeled on implementation intentions messages in previous studies that ask people to plan how they will act when they encounter a particular real-life situation. The rewards message is intended to highlight the psychological rewards of voting by emphasizing the improved mood (by referencing “feelings”) that would be experienced as a result of voting (especially for one’s preferred party candidate), greater pride and less guilt on Election Day, and other positive feelings. Both messages rely on imagery techniques (inducing respondents to imagine the actions or rewards on Election Day) in order to maximize the mobilization effect.

It should be noted that these experimental manipulations are more subtle than those used in prior mobilization experiments. The messages were included as text near

the end of the online survey—that respondents were not forced to read carefully, or even read at all—and were received 4 or 5 days before the election for most people. Over half of the sample completed the survey the Thursday or Friday before Election Day. Prior research demonstrates that email-based mobilization messages have been wholly ineffective in mobilizing people (Nickerson 2007a; Bennion and Nickerson 2010), and that the effects of mobilization messages decay quickly over time. Therefore, the results in this study about the effects of actions and rewards messages delivered online should underestimate their effects if used in mailings or phone calls delivered closer to Election Day.

The experimental groups were balanced with respect to almost all observed characteristics. Appendix Table 1 reports the means and significant differences among the groups.<sup>15</sup> I measure turnout in the 2010 election by matching respondents to official public voting records after the election. To assess the effects of the messages on habitual and non-habitual voters, I rely on the VHSI. As detailed in Chapter 3, the construction of the VHSI is motivated by psychology research on habitual behaviors, including the index used by Verplanken and Orbell (2003). I construct the VHSI from a battery of questions designed to measure the degree of automaticity in voting-related acts and the centrality of voting to one’s identity—the two primary features of habitual voting. I combine the items in a single additive index representing voting habit strength. The measurement analysis in Chapter 3 demonstrates that the scale is a valid and reliable measure of the strength of voting habits.

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<sup>15</sup> I also report balance statistics for respondents who intend to vote (“extremely likely” and “very likely” voters), since portions of the analysis are conducted for these respondents.

Using the VHSI to measure the strength of voting habits has numerous conceptual and empirical advantages over using a measure based on frequency of prior voting. Conceptually, voting habit strength is a psychological construct that should be measured directly. Frequency of prior voting might be used as a rough proxy for habit strength, but is conceptually distinct. In my sample, voting habit strength is correlated with frequency of prior voting, but the correlation is far from perfect. As demonstrated previously, some relatively infrequent voters have strong voting habits and some relatively frequent voters have weak voting habits (mostly because they vote based strictly on the issues and support for specific candidates).

Using the VHSI also avoids the need to make arbitrary distinctions about voting history to determine a person's voting habit strength. For example, researchers might use voting history information to define as a habitual voter someone who votes in 100% of all general and primary elections (virtually no one), 75% of general elections and 50% of primary elections, 100% of general elections and 25% of primary elections, 50% of general elections and always 50% of local elections, or numerous other combinations of election types and frequency. Constructing frequency-based measures of habit is also difficult because researchers must know which prior elections a person was eligible to vote in (over 18 and lived in the state) to avoid counting recently eligible voters as infrequent voters. Due to conceptual and empirical advantages, researchers in social psychology have now largely abandoned measures of habit based on prior frequency, and relied heavily on direct measures of the psychological strength of habits similar to the VHSI.

## Results

### *Intention to Vote*

I first determine whether, and to what extent, the actions-planning and rewards messages strengthen the intention to vote—for all potential voters, and separately for habitual and non-habitual voters. Respondents are asked how likely they are to vote at the start of the pre-election survey and also near the end. Because the survey is identical between the three experimental conditions other than the mobilization messages section, any differences in final voting intentions between the conditions can be attributed to the mobilization messages, other than randomly occurring differences. Specifically, I estimate the effects of the messages on the final self-reported likelihood of voting in the 2010 election.<sup>16</sup> I control for baseline likelihood of voting (assessed at the start of the survey before the messages section), so that the estimates of the message effects may be interpreted as the change in intention to vote over the course of the survey.

I also include controls for characteristics that differed across experimental groups—among all respondents, among non-habitual voters (respondents below the median VHSI value), and among habitual voters (those above the median VHSI value). These controls include: strength of preference for the respondent's preferred gubernatorial candidate, campaign contact, residential length, and general interest in politics.<sup>17</sup> The difference in mean habit strength scores between the actions and control

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<sup>16</sup> The likelihood of voting is measured using an 8-point scale, and combining responses from a set of branching questions. The initial question asks if the respondent is likely or unlikely to vote in the upcoming election, and the follow-up questions asks if the respondent is extremely, very, somewhat, or slightly [likely/unlikely] to vote.

<sup>17</sup> Some experimental groups also differed significantly in the congressional district of respondents' residence. But to preserve degrees of freedom, and because the 7 congressional district dummies are not significant predictors of turnout and voting intentions in most models, I exclude them. I also exclude age, even though it differed significantly between the actions and

groups also just reaches significance at conventional levels in the full sample ( $p=.093$ ). However, there are no significant between-condition differences in habit strength among non-habitual voters or among habitual voters. I include habit strength in all models regardless. Controlling for these factors helps provide unbiased estimates of the effects of the messages, for all respondents and also among individuals with strong and weak voting habits.<sup>18</sup>

Although the mobilization messages are randomly assigned, habit strength is not. Any factors that are associated with habit strength and also turnout in 2010 might be included in the model with the message-habit interactions in order to obtain unbiased estimates of the message effects for habitual and non-habitual voters. For example, failing to control for campaign contact may yield a biased coefficient for the interaction between habit and the actions condition if campaign contact is the more important moderator of that message than habit strength. Campaign contact is associated with both habit strength and turnout. All models control for strength of candidate preference, campaign contact, residential length, and general interest in politics. However, because of the limited degrees of freedom, and because so many other variables are associated with both habit strength and turnout, I do not control for all of them. Instead, I separately examine the potential moderating influence of other variables in follow-up analyses, and compare them to the strength of habit in moderating the messages. Also, I reduce the

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rewards groups (for all habit levels). Age is strongly associated with habit strength, and would bias the theoretically important estimates.

<sup>18</sup> Mutz and Pemantle (2011) demonstrate convincingly that, as long as the mechanism of random assignment in an experiment is not faulty, conducting randomization checks and including covariates in models of treatment effects to account for any differences across experimental groups is unneeded. The mechanism of random assignment in this experiment was not faulty, and based on a simple block design for individual respondents. Nevertheless, I include covariates for factors that differed across condition to follow standard practice.

likelihood of finding false moderating effects of habit strength because those moderation tests are not post-hoc. They were the primary moderation tests examined in 2008 and 2009 randomized field experiments on mobilization messages. Moreover, I use two-tailed significance tests throughout the analysis to limit the risk of such false discoveries.

I estimate all models of self-reported likelihood of voting using ordinary least squares. As in other chapters' analyses, I exclude individuals who already voted by absentee ballot, since the messages could not influence their intentions to vote. The estimates are presented in Table 1. The coefficients for the actions-planning and rewards message variables in model 1 represent the average treatment effect for all respondents, indicating that respondents receiving them did not display a significant increase in their likelihood of voting. Neither variable reaches conventional levels of significance. As emphasized above, I use two-tailed tests for all significance tests in this chapter, since I do not always have strong directional hypotheses. These results provide initial disconfirmation of H1 and H3, which propose that the actions and rewards messages boost turnout for all potential voters.

**Table 1. OLS Estimates for Self-Reported Likelihood of Voting (N=691)**

	[1]	[2]
Actions message	0.006 (0.006)	0.055 (0.024) *
Rewards message	0.001 (0.006)	0.010 (0.023)
Actions x Habit	-- --	-0.062 (0.030) *
Rewards x Habit	-- --	-0.011 (0.029)
Habit strength	0.083 (0.016) *	0.105 (0.023) *
Baseline self-report. likelihood	0.907 (0.014) *	0.909 (0.014) *
Intercept	0.013 (0.012)	-0.005 (0.017)
Model F	1039.8 p<.01	835.9 p<.01
Adj. R <sup>2</sup>	0.923	0.924
Root MSE	0.066	0.066

Notes: \* p<.1 using two-tailed tests. Standard errors in parentheses. All models include controls for campaign contact, residential length, interest in politics, and strength of candidate preference. The control variable estimates are included in Appendix Table 3.

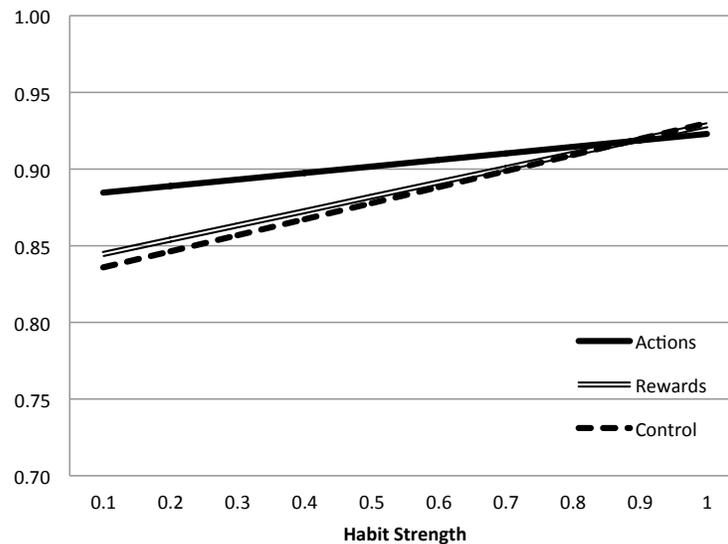
However, the results in model 1 mask large differences for voters at different habit strength levels. Model 2 includes the interactions between the message dummy variables and habit strength. The results demonstrate that the actions-planning message strengthened the intention to vote far more among individuals with weak voting habits than strong habits (the coefficient for the actions-habit interaction is statistically significant). Figure 1 presents the predictions for individuals at all values of habit strength based on the model in Table 1. Predictions are calculated at decile values of habit strength (.1, .2, up to the highest value<sup>19</sup>). I set all other independent variables to their median values for all respondents, as with all predictions in this chapter.

Differences between the actions and control values are significant at all values of habit strength from .1 to .6. Differences between the rewards and control condition values are not significant at any value, nor are the differences between the actions and

<sup>19</sup> For the highest value of habit strength, I calculate the predictions setting the VHSI to .999 rather than 1, since they are more stable at .999. I display the highest-value predictions since the habit strength variable is highly skewed and many respondents have values above the ninth decile.

rewards values. The differences can be conceptualized as the average change in self-reported likelihood (measured from 0 to 1) over the course of the survey. A change from “somewhat” to “very” likely would be .125. Individuals with habit values of .4 had a predicted increase in self-reported likelihood of .03. Considering that the two likelihood questions were asked within 5 to 10 minutes of each other, the motivation for respondents to be consistent over time was enormous. Indeed, most people kept the same response over the course of the survey, but fully one-third of the respondents who changed their second response to being more likely to vote were individuals in the bottom half of VHSI values who received the actions message.

**Figure 1. Predicted Effects of Messages on Self-Reported Voting Likelihood, by Habit Strength**



Note: Predictions are calculated using model in Table 1. Other independent variables are set at median values. Differences between the actions and control predictions are significant at .1 level (using two-tailed tests) for habit strength values of .1-.7.

These results provide initial support for  $H_{1a}$ —that actions-planning messages boost the likelihood of voting among individuals with weak voting habits. And they initially disconfirm  $H_{1b}$ —that actions-planning messages increase the likelihood of voting most among those with strong habits. I use the term “initial” because the dependent

variable is self-reported likelihood of voting, rather than validated turnout. H<sub>1a</sub>, with no additional qualifications, would not be confirmed if the results for validated turnout models in the following section differed considerably.

### *Voter Turnout*

I next determine whether, and to what extent, rewards and actions mobilization messages influence turnout. I use validated voting information from public records obtained after the election. For this analysis, I excluded individuals who could not be matched to the post-election records, since their turnout information is unavailable. See Chapter 4 for a discussion of the matching process.

I estimate all turnout effects using probit models. I include the same controls as in the models of self-reported likelihood. The results in the first column of Table 1 demonstrate that neither the rewards nor actions message increase turnout for all respondents. The coefficients for neither message variable reach conventional levels of significance.

**Table 2. Probit Estimates for Validated Voting (N=699)**

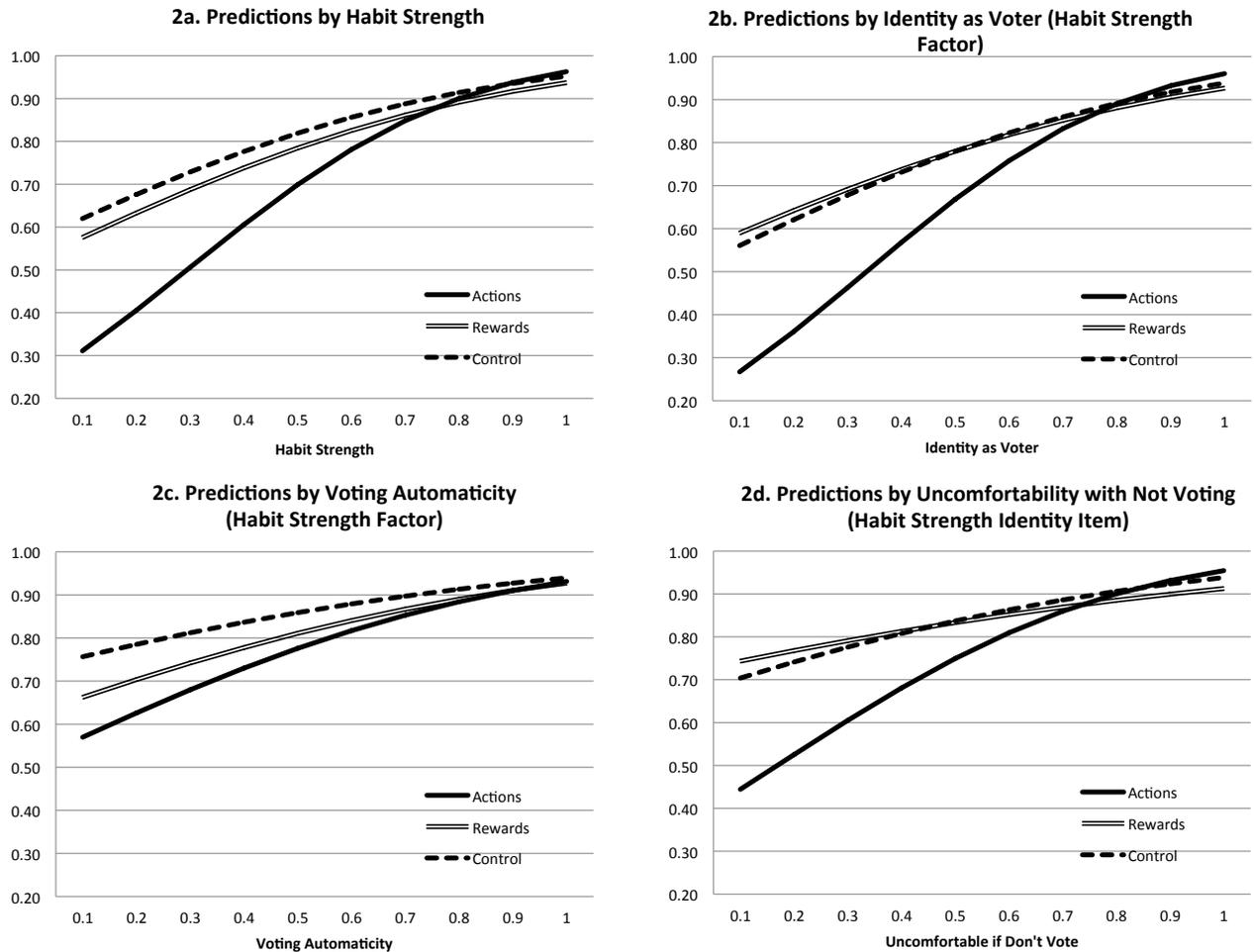
	[1]	[2]
Actions message	-0.209 (0.174)	-0.900 (0.523) *
Rewards message	-0.141 (0.174)	-0.112 (0.499)
Actions x Habit	-- --	1.020 (0.761)
Rewards x Habit	-- --	-0.024 (0.720)
Habit strength	1.817 (0.346) *	1.516 (0.558) *
Intercept	-1.402 (0.241) *	-1.216 (0.343) *
Wald $\chi^2$	134.6 p<.01	136.2 p<.01
Pseudo R <sup>2</sup>	0.276	0.281

Notes: \* p<.1 using two-tailed tests. Robust standard errors in parentheses. All models include controls for campaign contact, residential length, interest in politics, and strength of candidate preference. The control variable estimates are included in Appendix Table 4.

However, these results mask some differences in the effectiveness of the messages for individuals with strong and weak voting habits. Model 2 in Table 2 includes the interactions between habit strength and the message dummy variables. The coefficients for the interaction terms are not significant at the .1 level. But the coefficient for the actions message main effects term is statistically significant. And the main effects coefficient is negative. At first glance then, the actions message appears to have a modest *demobilizing* effect among individuals with weak habits. The rewards message appears to have no statistically noticeable effect relative to the control condition among individuals at any habit strength level.

Figure 2a displays the predictions generated using the model with interactions in Table 2. The predicted differences between the actions and control conditions are statistically significant at habit strength values of .1 to .5. The differences between the rewards and control conditions are not significant at any habit strength levels, and neither are the differences between the actions and rewards conditions. These results stand in stark contrast to those for voting intentions in the previous section. The actions-planning message increases the self-reported likelihood of voting among non-habitual voters, but *decreases* the likelihood of actually voting for that group. The actions message has no effect on either self-reported or actual likelihood of voting for habitual voters that is discernable from 0. (However, the results hint at a turnout-enhancing effect among habitual voters, which I discuss below.) Overall, the evidence supports H<sub>1b</sub>. But it leaves several questions unanswered: How can a message produce different effects for voting intentions and validated turnout? What are the mechanisms underlying those effects?

**Figure 2. Predicted Effects of Messages on Validated Voting, by Habit Strength**



Note: Differences between the actions and control predictions are significant at the .1 level (using two-tailed tests) for habit strength values of .1-.5 in Fig 2a, .1-.2 in 2b, no habit values in 2c, and .1-.4 in 2d.

Before examining these questions, I address a sample exclusion issue.

Interestingly, the differential effects of the messages for habitual and non-habitual voters are somewhat more pronounced when analyzing only individuals who intend to vote.

Although 88.7% of the sample reported being extremely or very likely to vote at the beginning of the survey, 11.3% reported being unlikely to vote, or only somewhat or slightly likely. These may be people who never vote in midterm elections, are

completely uninterested in the candidates or issues, or know they will be moving residences or will not have time to vote. Indeed, only 26% of this group voted.

I avoid examining the strong-intention voters in more depth because the basic relationships observed in the full sample hold among the strong-intention voters, and also because I am primarily concerned in this chapter with obtaining unbiased estimates of the message effects for all habitual and non-habitual voters. Individuals with weak voting intentions are more likely to have weak voting habits. Excluding them would undoubtedly bias the estimates of the message effects—a significant problem in prior mobilization studies. Nevertheless, it is interesting that the actions message has a slightly stronger positive effect on turnout relative to the control among habitual voters (that almost reaches conventional significance levels) in the strong-intentions sample. (The actions message still has a demobilizing effect among non-habitual voters.) And the rewards message effects resemble the actions message effects more closely among those who intend to vote. This suggests that restricting study samples to individuals who intend to vote may uncover more turnout-enhancing effects of mobilization messages, at least among habitual voters. In particular, the actions message—which helps people form a mental plan to vote by highlighting the concrete actions involved in voting—may be most helpful for people who intend to vote.<sup>20</sup>

I next examine whether the observed moderating effect of habit strength on turnout may be spurious. For all potential voters, the messages had no apparent effect. But for individuals with weak habits, the actions message strengthened voting intentions and reduced the likelihood of Election Day voting. Because I did not include a large

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<sup>20</sup> In studies of implementation intentions messages, the treatments are only effective for people who intend to change their behavior (Orbell and Verplanken 2010).

range of controls, perhaps an omitted variable—like frequency of prior voting, or self-reported likelihood of voting—is the true moderating variable. I find no evidence that the habit strength moderation effect is spurious. I estimate the identical models as in Table 2, but replace habit strength and the habit-message interactions with frequency of prior voting and habit-frequency interactions. The results are displayed in Table 3, model 1.<sup>21</sup> I also perform the same variable substitution using self-reported likelihood of voting. Those results are in model 2. The coefficients for all interaction and main effects terms in both models are far from significant. Frequency of prior voting and self-reported likelihood of voting appear not to moderate the effects of the messages on turnout.

**Table 3. Probit Estimates for Validated Voting**

	[1]	[2]
Actions message	-0.18 (0.40)	-0.43 (0.72)
Rewards message	0.11 (0.40)	0.45 (0.59)
Actions x Frequency	0.08 (1.51)	-- --
Rewards x Frequency	-0.97 (1.52)	-- --
Frequency prior voting	4.34 (1.07) *	-- --
Actions x Intentions	-- --	0.28 (0.78)
Rewards x Intentions	-- --	-0.69 (0.65)
Intentions to vote	-- --	2.99 (0.51) *
Intercept	-1.27 (0.31) *	-2.29 (0.46) *
N	703	701
Wald $\chi^2$	109.6 p<.01	162.0 p<.01
Pseudo R <sup>2</sup>	0.307	0.417

Notes: \* p<.1 using two-tailed tests. Robust standard errors in parentheses. All models include controls for campaign contact, residential length, interest in politics, and strength of candidate preference. The control variable estimates are included in Appendix Table 5.

I now examine the psychological mechanisms underlying the message effects. Why does the actions message significantly increase the self-reported likelihood of voting among weak-habit individuals and significantly decrease the likelihood of actual

<sup>21</sup> As in all models in this chapter, all variables are rescaled to range from 0 to 1.

turnout among them? I hypothesized in  $H_{1c}$  that the actions message influences turnout by altering individuals' automaticity in voting. Forming a mental voting plan by imagining the concrete actions on Election Day (and answering questions to solidify the actions plan) should make those actions more efficient to perform when the real-life setting is encountered, and less dependent on awareness, control, and intentionality to perform.

This can be consistent with either hypothesis about the effects of the actions message: For individuals with the least automaticity in voting, the actions message might increase turnout the most by helping them overcome their automaticity deficit ( $H_{1a}$ ). Or, the actions message might increase turnout the most for those with the greatest automaticity because mobilization messages have a much greater effect on those who can translate their intentions into turnout ( $H_{1b}$ ).<sup>22</sup> Either way, the proposed mechanism by which the actions message influences turnout is via automaticity in voting; increasing it strengthens voting intentions and increases the likelihood of actually voting. I do *not* propose that one's identity as a voter is a key moderator.

I examine these proposed mechanisms by utilizing the separate habit strength subscales as moderators of the actions message. As will be demonstrated, it is difficult to determine with these data whether any or all of the proposed automaticity-related

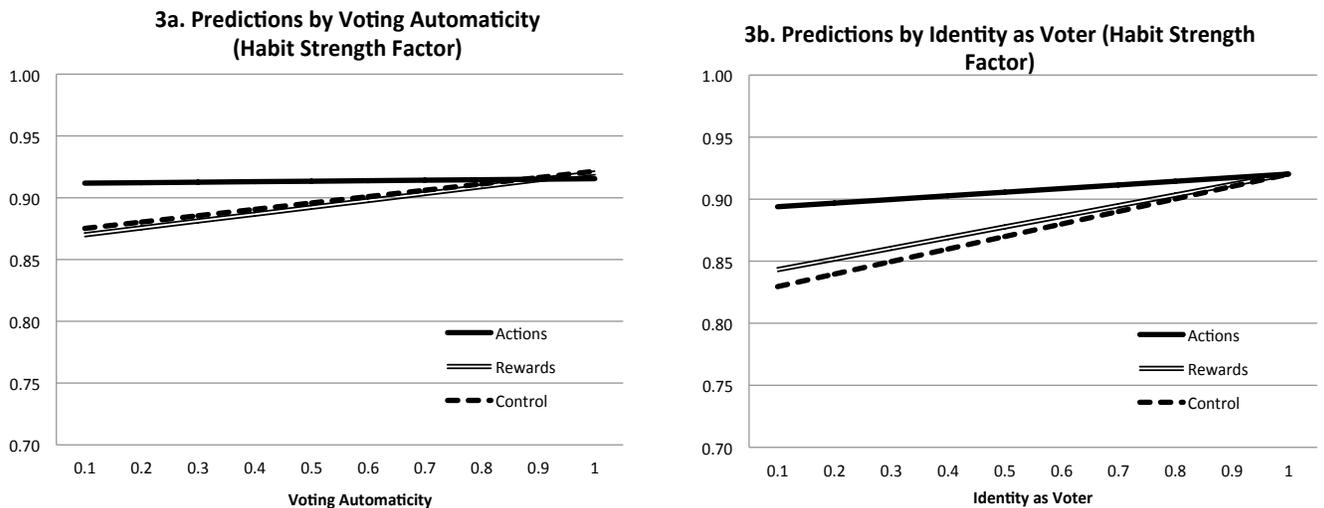
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<sup>22</sup> Further development of the proposed mechanisms underlying  $H_{1b}$ : one cannot gain automaticity in voting (or any complex behavior) easily. Habitual behaviors are automatic because they are repeated over time in a specific context, eventually altering the automatic associations stored in procedural memory. (One's identity as a voter is another psychological change resulting from such behavioral repetition.) So it is difficult to imagine that a simple message read on a computer screen by a respondent might have the same effect as repeated behavior in a stable context. Additionally, individuals with greater automaticity in voting (habitual voters) should be able to translate their intentions into voting much more easily. If the actions message boosts turnout, even slightly, habitual voters should be more likely to make it to the polls.

mechanisms are responsible. But it is clear that one's identity as a voter is an important determinant of whether and how individuals are influenced by the actions message.

Figure 3 displays the predicted effects of the messages on voting intentions at all values of the habit strength subscales. The models used to generate the predictions are identical to those in Table 2, model 2, besides substituting the habit subscales for the VHSI. I also substitute interactions between the subscales and the mobilization messages for the VHSI-message interactions. The results demonstrate that the actions message strengthens voting intentions more for individuals with weak voter identities (see Figure

**Figure 3. Predicted Effects of Messages on Self-Reported Likelihood of Voting, by Habit Strength**



Note: Differences between the actions and control predictions are significant at the .1 level (using two-tailed tests) for habit strength values of .1-.6 in Fig 3a, .1-.8 in 3b.

3b) than with low automaticity in voting (see Figure 3a), even though the direction and magnitude of the effects appear reasonably similar in the two figures. However, including both message-habit subscale interactions simultaneously in a model reveals that the identity subscale is the primary moderator.

Table 4 displays the results with both habit subscales included simultaneously. Since I am primarily concerned with the actions message, I restrict the sample to the

actions and control groups, and omit all variables related to the rewards message. The coefficients for both the automaticity subscale and its interaction with the actions message are not statistically significant in the model. However, the coefficients for the identity subscale and its interaction with the actions message *are* significant. Substantively, this means that the measure of centrality of voting to one’s identity is primarily responsible for the heterogeneous effects of the actions message. For individuals with weak voting identities, reading a message urging them to form a voting plan by imagining Election Day acts strengthens voting intentions most (relative to not reading any message).

**Table 4. OLS Estimates for Self-Reported Voting Likelihood, Among Action & Control Groups (N=429)**

Actions message	0.069 (0.029) *
Actions x Habit-Identity	-0.067 (0.036) *
Actions x Habit-Automaticity	-0.006 (0.038)
Habit-Identity	0.097 (0.031) *
Habit-Automaticity	0.021 (0.027)
Baseline Voting Likelihood	0.894 (0.018) *
Intercept	-0.015 (0.020)
Model F	538.31 p<.01
Adj. R <sup>2</sup>	0.926
Root MSE	0.065

Notes: \* p<.1 using two-tailed tests. Standard errors in parentheses. Models include controls for campaign contact, residential length, interest in politics, and strength of candidate preference. The control variable estimates are included in Appendix Table 6.

When analyzing validated turnout, the identity subscale also appears to be responsible for heterogeneity in actions message effects. (But the direction of the effects

is different than for intentions). The results are illustrated in Figures 2b and 2c.<sup>23</sup> The actions message significantly *decreases* turnout among weak voter-identifying respondents, and has no statistically significant effect for low automaticity respondents. The nonsignificant effects for low-automaticity voters may be the result of the actions message boosting their intention to vote but them not being able to translate their intentions into turnout, thus resulting in no observable voter mobilization effect. This would be consistent with the automaticity-related propositions discussed in relation to H<sub>1a</sub> and H<sub>1b</sub>. But the evidence does not allow me to directly confirm or disconfirm this.

Regardless, the results demonstrate clearly that the demobilization effect of the actions message among non-habitual voters is concentrated among weak-identity voters *and not low-automaticity voters*. Additional evidence for this can be found in Appendix Table 7, which presents the results for the model with the same specification as in Table 4 on self-reported likelihood—but using validated turnout as the dependent variable. The coefficient for the identity main effect is significant in Appendix Table 7, while the automaticity main effect is not. And while the coefficients for the actions message and the actions-identity interaction do not reach conventional significance levels, they are near the cutoff.

One identity subscale item in particular appears to determine which individuals are influenced most by the actions message: the identity item that assesses how uncomfortable an individual will be if she fails to vote in an election for governor (see Chapter 3 for the specific question wording). More than any other single item in these data, responses to this “uncomfortability” question predict whether the actions message

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<sup>23</sup> As with the Figure 3 predictions, I generate the predictions in Figure 2b and 2c using models that simply substitute the habit subscales for the VHSI (and create the appropriate interaction terms).

demobilizes respondents the most.<sup>24</sup> Figure 2d displays the predictions. The actions message significantly decreases turnout among individuals who do not feel uncomfortable if they fail to vote in gubernatorial elections, relative to the control. The effect is significant at all values of uncomfortability from .1 to .4.

Why might the demobilization effects among weak-identity voters (especially those with low uncomfortability scores) be so large? Besides the explanations offered above, the actions message might subconsciously prime the effort involved in voting on Election Day. Unlike many mobilization messages, Nickerson and Rogers (2010) find that a planning message decreases turnout for some individuals (those in two- and three-voter households). And Krupnikov and Levine (2010) found that having people imagine the concrete steps involved in signing up for a listserv about volunteer activities for political action *decreases* their likelihood of actually signing up. Perhaps the message in my study (which relied on vivid imagery descriptions of Election Day) made the effort in voting more accessible. For individuals for whom voting is not central to their identities, and who will not feel uncomfortable if they fail to vote, and less likely to suffer any identity- or esteem-related repercussions from not voting, this additional effort may turn them off.

Because the actions message strengthened the intention to vote but decreased actual turnout, the priming effect is probably unconscious. For weak-identity voters, the effort may not be realized by them until Election Day approaches.<sup>25</sup> In other words, the

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<sup>24</sup> The magnitude of the demobilization effect is greatest, and statistically significant at more habit values, when using the entire VHSI than using the single gubernatorial-uncomfortability item. But the uncomfortability item provides more explanatory value than other individual habit items.

<sup>25</sup> For an initial test of this mechanism, I could use responses to an open-ended question about Election Day thoughts asked immediately after the mobilization messages in the survey. Respondents were asked, "Thinking ahead to Election Day, what things do you guess will be on

effect is not likely mediated by conscious intentions at the time of the survey. Instead, the message might have immediately motivated them to vote and report stronger intentions, but unconsciously primed the effort in voting. Clearly, future research is needed to test these mechanisms. But based on the available evidence, the recommendation for GOTV campaigns is clear: for non-habitual voters, campaigns should motivate them to vote without asking them to imagine all of the steps involved. For GOTV campaigns to stimulate turnout, they might focus on individuals for whom voting is central to their identities (or habitual voters more generally).

Future research might also examine the effects of the rewards and actions messages among likely voters. Although the rewards message generally had no statistically significant effects in this chapter's sample, it had some positive effects among habitual voters in the intend-to-vote sample. For polling firms designing likely voter models, it is particularly important to untangle the relationships between self-reported likelihood of voting, actual turnout, prior voting frequency, and habit strength. The results suggest that the VHSI can help identify individuals who report being extremely likely to vote but fail to turn out. Individuals with weak voting identities reported being more likely to vote after receiving the actions message on average, but less likely to actually vote. Identity-related measures may therefore be fruitful for polling firms seeking to improve their likely voter models. To my knowledge, such measures have not been used this way.

The finding that the actions message had entirely different effects on turnout and voting intentions among weak-habit voters has important consequences for mobilization

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your mind that day?" I could code the open-ended responses for actions- and rewards-related thoughts, and assess whether the actions-related thoughts include more references to effortful actions for non-habitual voters.

studies. Such studies sometimes use measures of voting intentions as dependent variables. While some previous studies have acknowledged the disconnect between voting intentions and actual turnout, most typically assume the difference is similar across types of voters. The results in this chapter suggests that the disconnect exists primarily for non-habitual voters. The results also demonstrate the necessity of using measures of actual turnout rather than voting intentions as the dependent variable in mobilization studies.

Finally, despite some null findings, the fact that the actions message had any observable effects is noteworthy given that prior studies have found email-based messages to be wholly ineffective at increasing turnout.<sup>26</sup> The messages in this study were included near the end of the survey too, meaning that respondents were likely somewhat fatigued when they read them, if they read them. The subtle form of message delivery suggests that including these particular messages in more direct modes of communication (like phone calls) may produce larger effects.

## **Discussion**

The lack of attention to message content—especially the psychological mechanisms underlying mobilization message effects—and the lack of attention to subgroup differences in message effects are significant limitations of prior mobilization research. This study directly addresses these limitations. I draw on the theory of habitual voting detailed in Chapter 2 to derive specific hypotheses about the effects of actions- and rewards-related messages on individuals with strong and weak voting habits. And I

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<sup>26</sup> While the mobilization messages were not sent by email in this study, I relied on email to contact respondents following recruitment. And the medium of message delivery was online (messages embedded in an online survey).

test them with data from an experiment carried out during the 2010 election in Minnesota.

The results illustrate the importance of assessing the effects of messages separately for habitual and non-habitual voters. Estimating the average effect of the messages on all potential voters masks differences in effects across level of voting habit strength. Specifically, messages focused on the concrete how/when/where actions in voting strengthen voting intentions and reduce actual turnout for voters with weak voting habits.

In general, the strength of voting habits is an important moderator of the effectiveness of the imagery-based actions-planning message examined in this chapter. And a person's voting habit strength is likely a critical moderator of a range of other political influences on citizens' voting: electoral reforms, education, interest in specific candidates, other mobilization strategies, and other forms of elite messaging. I demonstrate in previous chapters that specific electoral motives (like interest in candidates and election-specific issues) are more influential for non-habitual voters than habitual voters. And in the following chapter I examine the role of voting habits in moderating the effects of state electoral reforms, including early voting and Election-Day registration laws, on turnout.

The study also examines the important and understudied relationship between voting intentions and actual voting behavior. While most people report being likely to vote prior to most elections, many do not make it to the polls. Based on the theory of habitual voting, I proposed that non-habitual voters are least able to translate their intentions into actual turnout. Indeed, the results demonstrate that while *habitual* voters'

intentions closely match their turnout, the intention-behavior match was weak for *non-habitual* voters. The actions message dramatically increases non-habitual voters' desire to vote, but actually decreases their turnout. As suggested above, this may be because Election Day activities are less automatic and effortless for non-habitual voters, and because they have less identity- and ego-involvement in voting. And the actions message may have depressed their turnout by making the effort of voting more accessible. Future research might rely on less vivid imagery wording—that de-emphasizes the effortful steps in voting—to mobilize non-habitual voters. But the results reinforce the common notion that non-habitual voters are difficult to mobilize.

The discrepancy between intention and behavior has important implications for future mobilization research. Gerber and Rogers (2009) found that a message telling people that the upcoming election will have high turnout boosts turnout for infrequent voters; however, their dependent variable is constructed from a question about the intention to vote. The current study's results demonstrate that the discrepancy between voting intentions and actual turnout exists primarily for individuals with weak voting habits. Since the infrequent voters in Gerber and Rogers' study should have weaker-than-average voting habits, their findings may be an artifact of examining voting intentions. Future studies should rely on validated turnout measures whenever possible, especially when assessing mobilization effects among non-habitual voters.

Future research might also examine the effects of messages that highlight the psychological “punishments” of not voting, rather than the rewards of voting. Since habitual voters feel uncomfortable and are more likely to question their participatory identities if they stay home on Election Day, they should be highly motivated by

messages that emphasize the negative feelings they will experience if they fail to vote. These “punishments” would likely be most effective for habitual voters if they highlighted potential losses in self-esteem experienced from a weakened identity as a voter—rather than highlighting the social repercussions of not voting. “Social pressure” messages used in prior studies display participants’ voting histories on postcards and remind them that voting records are public and they are “being studied.” However, the primary mechanism underlying such messages may not be social at all. Many people, especially habitual voters, vote for ego-defensive reasons—to avoid changes in the self-concept resulting from staying home on Election Day, regardless of what friends, family, or society think. Future researchers might mobilize habitual voters by developing even more direct methods of priming potential losses in self-esteem that result from not voting.

Future research would also benefit from replicating this study using a larger, more representative sample. Although smaller samples make it more difficult to find significant message and interaction effects, replication of experimental studies is necessary to improve their external validity. This study was administered in Minnesota, the state with the highest voting rate. Residents in other states may be less responsive to mobilization nudges. And although I make no explicit sample restrictions during recruitment, the research design may have inadvertently generated a sample with a disproportionate share of habitual voters, limiting the generalizability of the findings. 86% of my sample voted in the 2010 election. This percentage is higher than expected, even for Minnesota. One reason for the disproportionately high voting rate is that I recruit participants using the list of registered voters, who are more likely to vote than

non-registered residents.<sup>27</sup> The design also provides potential respondents with two opportunities to refuse to participate: once by phone during the initial recruitment, and once when the URL to the survey was emailed. Although potential respondents had no reason to believe the study was about voting, they were told it was a “survey about social and political issues.” Less participatory individuals may have been less inclined to take the survey. Nevertheless, the study provides a strong initial test of the effects of rewards and actions planning messages on habitual and non-habitual voters and their underlying mechanisms.

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<sup>27</sup> In Minnesota, non-registered residents can vote because of Election Day registration laws.

## Chapter 6

### The Effects of Early Voting Policies on Habitual and Non-Habitual Voters

The traditional conception of voting as a trip to one's precinct polling location has shifted in the past decade in the U.S. 35 states now allow some form of early voting. Some states allow citizens to cast in-person ballots before Election Day, some have no-excuse absentee voting, and Oregon and Washington now hold all elections by mail (see NCSL 2012). While most voting still occurs in person on Election Day, the share of ballots cast early has risen significantly, from 14 percent in 2000 to 20 percent in 2004, 31 percent in 2008, and 25 percent in 2012. There is little reason to expect this percentage to decline substantially in the future.<sup>1</sup>

Despite the increasing utilization of early voting, scholars still do not know how early voting laws influence the size and composition of the electorate. Does early voting increase the turnout of some voter subgroups more than others? Does early voting “expand the electorate”? Laws allowing early in-person and no-excuse absentee voting are generally enacted under the assumption that those methods boost turnout among non-habitual voters, thereby expanding the electorate. If early voting is more convenient than Election Day voting, and individuals with weak habits are more likely to vote when it is convenient, then early voting laws should increase non-habitual voters' likelihood of voting. In Maryland, for example, early voting was enacted as “a way to attract new voters to the polls, a chief goal of its implementation” (Ford 2012).

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<sup>1</sup> According to a Federal Election Commission report (see Stein and Garcia-Monet 1997, 669), “Every jurisdiction that has introduced early voting agrees that once it has been started, it cannot be stopped. Voters love it.”

However, studies on early voting often find that it has no discernable long-term effect on aggregate turnout, and may even depress turnout slightly after the first election it becomes available in a state. Many suggest that early voting simply serves as a convenience for habitual voters, who would vote on Election Day without it. Berinsky (2005) reviews research on early voting and distinguishes between its effects on “transient” voters and “core” voters, who he describes as more educated, wealthy, and politically interested. Berinsky concludes that early voting usually retains core voters, rather than boosting turnout among transient voters, thereby exacerbating socioeconomic biases in the electorate. Similarly, other researchers discuss the utilization of early voting by individuals with more education, income, political engagement, sophistication, age, length of registration, often implying that *habitual voters* are the typical early voters (also see Gronke and Toffey 2008; Burden et al. 2012; Stein and Vonnahme 2011).

Curiously, some studies find positive effects of early voting on turnout, and turnout for individuals similar to non-habitual voters. Stein and Garcia-Monet (1997) distinguish between early in-person voting at traditional sites (e.g., schools, fire stations, courthouses) and nontraditional sites (e.g., supermarkets, shopping malls, and familiar and highly frequented locations). The authors find that, in Texas, a greater number of traditional sites is not associated with higher early voting or precinct voting rates, but that having more nontraditional sites is associated with higher early voting and overall turnout rates. The results suggest that early voting systems vary significantly in their convenience.

In support of this, Stein and Vonnahme (2008) find that Election Day vote centers in Larimer County, Colorado, which are “centrally located to major population centers,”

increase turnout, primarily among infrequent voters. Stein and Vonnahme do not examine early voting, but their results indicate that convenience may be a function less of “earliness” than of ease of travel and planning. Findings by Dyck and Gimpel (2005) further suggest that the convenience of early voting may be strongly related to geographic proximity and shorter travel times, since living close to an early voting site increases the likelihood of voting.

Prior studies are generally not well suited to determine whether early voting reforms (and which early voting reforms) influence turnout among non-habitual voters. Three areas warrant further scholarly attention. First, studies must draw on empirical evidence and individual level theories of turnout to define “convenience.” They should examine the effects of specific forms of “convenience” in early voting that might influence habitual and non-habitual voters differently. I illustrate below how the dissertation’s theory of habitual voting is useful in doing this. Second, since policymakers and academics are ultimately interested in whether electoral policies “expand the electorate” by bringing in more non-habitual voters,<sup>2</sup> they should analyze the effects by level of voting habit strength. But prior studies of early voting often do not examine subgroup effects, and when they do, they focus on myriad factors ranging from political engagement to education, age, and length of time registered to vote. This diverts attention from the important moderator (habit strength), and also does not allow for many comparisons of subgroup effects across studies of early voting.

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<sup>2</sup> The results in earlier chapters of the dissertation demonstrate that inducing a non-habitual voter to cast a ballot greatly increases her likelihood of voting in the future—far more than inducing a habitual voter to turn out. Thus, mobilizing weak-habit individuals is the most effective way to increase the size of future electorates.

The final way to improve studies of early voting is to utilize individual level validated voting information, rather than county or state level data or self-reported turnout information from surveys. County and state data do not allow for separate estimates for habitual and non-habitual voters. Self-reported turnout data is problematic because many individuals who report voting early at the highest rates also routinely misreport voting in surveys at the highest rates (i.e., the most educated, strong partisans, individuals contacted by a campaign, and more frequent church goers; see Bernstein, Chadha, and Montjoy 2001). Obtaining unbiased estimates of early voting effects among non-habitual voters requires using validated voting data.

In this chapter, I draw on the theory of habitual voting outlined in Chapter 2 to offer hypotheses about convenience, early voting, and the strength of voting habits. I then propose an analysis to be carried out in the future that takes three parts. In the first part, I will estimate the effects of early voting on turnout separately for individuals with strong and weak voting habits. I will rely on data from the registered voter files in Maryland from 2008 to 2012 and Clark County, Nevada from 2002. Since Maryland enacted early voting in 2010, this interrupted time series will provide unique leverage for assessing the causal effects of early voting. Since Clark County enacted early voting in 2000, I cannot easily compare pre- and post-reform turnout rates. But including the Clark County case will allow me to test the remaining hypotheses in the chapter, and provides an appropriate jurisdiction for comparison since its 2002 midterm election was similar in salience to Maryland's 2010 midterm.

In the second part of the analysis, I plan to test hypotheses about convenience in early voting. I will compare the effects of a convenient early voting system (Clark

County's) and an inconvenient system (Maryland's) on turnout for individuals with weak and strong voting habits. Maryland's early voting system is similar to many other states', allowing early in-person voting at a small number of traditional sites, such as schools, libraries, and government buildings. Most counties have one or two sites, and the largest counties have five.<sup>3</sup> By contrast, Clark County offers numerous early voting sites at well-traveled, familiar locations. For many elections, the county has between 80 and 90 sites. It has "perhaps the most aggressive early- and absentee-voting program in the country," according to Dyck and Gimpel (2005, 533). By comparing early voting rates in the two jurisdictions, I can determine whether non-habitual voters are more sensitive to convenience.

In the third part of the analysis, I will determine whether location and traveling distance are the inconveniences responsible for any differential effects between habitual and non-habitual voters. I will build on Dyck and Gimpel's (2005) analysis by examining whether shorter distances to early voting sites are associated with a greater likelihood of early voting among non-habitual voters.<sup>4</sup> I conclude the chapter by discussing the implications for both electoral policymaking and theories of turnout.

### **Heterogeneity by Voting Habit Strength**

There are several types of "early voting," including early in-person voting, voting by mail, and no-excuse absentee voting (see NCSL 2012). Each type allows a registered citizen to cast a ballot prior to Election Day. States have various requirements about

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<sup>3</sup> Pictures and locations of Maryland's early voting sites for the 2012 general election are available at: [http://www.elections.state.md.us/voting/early\\_voting\\_sites.html](http://www.elections.state.md.us/voting/early_voting_sites.html).

<sup>4</sup> For this analysis, I will request to use the data compiled by Dyck and Gimpel, since they attach useful information on each individual's proximity to precinct and early voting sites. If I cannot obtain these data, I will purchase the Clark County file from Catalist, or request it from Clark County directly, and construct the proximity variables myself.

where the early voting takes place (e.g., at a government building, shopping mall, or completing a ballot at one's home and mailing it), and what excuses are permitted to cast an early ballot. States like Delaware only allow early absentee voting for registered voters who sign an affidavit verifying they are sick or prevented by school, work, or an acceptable reason for not going to the polls. States requiring excuses for absentee voting are not considered to have "early voting." 27 states now allow absentee voting without any excuse. 32 allow early in-person voting.

Early voting (whether in person, by mail, or by absentee ballot) is thought by many reformers to boost turnout, especially among non-habitual voters, because it is "convenient." However, reformers rarely explain which features of early voting make it convenient. In more systematic analyses, turnout scholars instead suggest that habitual voters utilize early voting most. But scholars too assume that early voting is convenient—and that any form of voting prior to Election Day must be convenient because it increases the allowable time to vote. The lack of theorizing about convenience may be the result of researchers frequently failing to construct hypotheses about electoral reforms with individual level theories of turnout in mind. Of course, there are few widely accepted theories of turnout to draw on that are both comprehensive and parsimonious. As I emphasize in Chapter 2, researchers have produced a long list of individual level factors shown to influence voting and non-voting at the margins, but few theories relating these factors to one another. And most factors like education and political efficacy are treated as static, failing to explain why individuals vote at different rates and for different reasons when their voting habits become stronger or weaker. Rather than select one or two turnout-boosting factors from the list to construct hypotheses about electoral reforms,

which ignores the broader dynamics of turnout, I will draw on the theory of habitual voting in the dissertation.

So what propositions about early voting does the theory of habitual voting offer? For individuals with weak or non-existent voting habits, voting is less automatic and less central to their identities. Specifically, the actions required to vote—including traveling to the site, finding out how to get there, getting material required to vote, searching for information about the issues, candidates, or voting procedures, and determining how to fit the acts into one’s daily routine—are less automatic. Those actions are also associated with more anxiety for non-habitual voters. So non-habitual voters are more likely to stay home if they face any impediments to voting, however small.<sup>5</sup> And because voting is not central to their identities, non-habitual voters will not face much discomfort or threat of self-esteem loss if they fail to vote (or at least none that cannot be mentally excused).

Therefore non-habitual voters are more likely to cast an early ballot when (a) the actions involved are more automatic and (b) when voting is more central to their identities. The optimal early voting system would produce both changes. But an early voting system cannot easily produce the second change. So to attract non-habitual voters, the system should focus on making early voting more automatic. Automatic behaviors are defined (according to the work of Bargh and other social psychologists discussed in Chapter 2) as behaviors that are performed efficiently and with a lack of control, awareness, and intent.<sup>6</sup> Roughly, voting is automatic when it is performed without having to think much about the steps (basically, with a “lack of control, awareness, and intent”) and without much effort (basically, “efficiently”). “Automatic” and

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<sup>5</sup> By “impediments,” I mean factors that make voting tasks even less automatic, more anxious, or weaken the motivation to vote in any way.

<sup>6</sup> A behavior gains these features after it is repeated frequently in a stable context.

“convenient” are similar terms. And despite the distinctions, I will use the terms interchangeably, and rely on “convenient” most because of its pervasiveness in the turnout literature.

Early voting actions that are *inconvenient* and *not automatic* for most non-habitual voters include: requesting a ballot in writing or in person; traveling to an early voting site she has not frequently traveled to before; and looking up information about where the early voting site is, or how to request a ballot. Information about early voting locations and procedures would be inconvenient if obtained on a government office website, by searching for the phone number and calling a government office (or slightly more convenient if obtained by word of mouth or inadvertently by watching television). In other words, any action that the person has not previously done many times, especially in the same context, should be considered inconvenient.

Early voting actions that are *convenient* and *automatic* for most non-habitual voters include: traveling to a supermarket, mall, or location that one has traveled to frequently in the past; not needing to request a ballot (i.e., having it sent unsolicited to the home in each election, or having it available at the early voting site); not needing to register to cast an early ballot; or at least being able to register at the early voting site; not needing to look up information about early voting locations or procedures (which otherwise would require searching a government website, finding a government office phone number, calling a government office to obtain the information); and voting online.

Most of the actions that are both convenient and inconvenient for non-habitual voters are convenient for habitual voters. Habitual voters have experience carrying out Election Day voting procedures, and many are the same as early voting procedures. For

example, finding information about ballots (how to request absentee ballots and where to send them), issues, and voting locations is something most habitual voters have done before. There is enough overlap between early and precinct voting procedures that habitual precinct voters might therefore be considered habitual early voters for most purposes. And because voting in general is more central to the identities of habitual voters, they are far more likely to exert any effort necessary to carry out actions that are less automatic and convenient to ensure they cast a ballot.

It should now be clear that casting an early ballot in most areas in the U.S. requires actions that are inconvenient and not automatic for non-habitual voters. For example, Maryland's early voting system offers only a handful of early voting sites in each county, and they are at schools, government buildings, and libraries. Most people do not travel to these locations frequently. Finding out how, when, and where to obtain an absentee ballot is also not automatic for non-habitual voters. Clark County, by contrast, has numerous early voting sites (usually 80 to 90). They are at locations frequently traveled to and easy to reach that do not require obtaining directions for most people (e.g., supermarkets and malls). Clark County also uses many mobile early voting sites that move locations every few days leading up to the election, to be closer to even more people. The county's Early Voting Advisory Board works to "identify and select locations which provide all Clark County voters an equal opportunity to vote...[and] includes representatives from various communities of interest such as senior citizens' groups, minority interest groups, political parties and other community organizations."<sup>7</sup>

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<sup>7</sup> See Clark County elections information at: <http://www.clarkcountynv.gov/depts/election/pages/ev.aspx>.

The earliness of early voting is not even particularly convenient or automatic for non-habitual voters. The defining element of early voting is its occurrence before Election Day. Shifting voting to an earlier day disrupts the stable context in which voting usually occurs, and should make it even less automatic for individuals with weak habits. As emphasized in the psychology literature on habit formation and in Chapter 2, habitual behaviors become automatic after they are frequently performed in a stable context.<sup>8</sup> The temporal context is almost entirely stable with Election Day voting, since it occurs on the first Tuesday of November in general elections. But to vote early, the temporal context shifts to a day of the person's choosing, and broadens immensely. So for non-habitual voters, being able to vote early is not particularly automatic. There is no stable, familiar, pre-defined temporal context that automatically triggers the behavior.

The earliness of early voting should not be particularly automatic for either habitual or non-habitual voters; but for habitual voters, it might be somewhat "convenient." Earliness should be one of the few elements of early voting that is convenient but not automatic for them. If I alter the definition of "convenient" somewhat to be "automatic *and fitting in well with a person's needs*"<sup>9</sup>, then early voting may be considered somewhat convenient for habitual voters. It would fit with their need to vote. Voting is more critical to their identities.

Early voting shifts the temporal context of voting, but the results in Chapter 4 demonstrate that context disruptions influence turnout far less for habitual voters. I examined the effects of residential/geographic context disruptions in voting. But the

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<sup>8</sup> I neglect any discussion of the psychological rewards (e.g., improved mood, greater efficacy, less anxiety associated with voting activities) that incentivize voting here, since their experience is not changed much depending on the type of early voting system.

<sup>9</sup> Dictionary definitions routinely mention "fitting in with needs."

same logic should apply to significant temporal context disruptions too. (The logic does not apply to small temporal context disruptions, like voting in the morning rather than afternoon on Election Day.) I found that disrupting the residential context in which people vote—by moving residences—depresses turnout most for individuals with weak habits, and has relatively little effect on habitual voters. Additionally, Brady and McNulty (2011) find that when precinct locations change (exogenously, due to administrative decisions), frequent voters still cast ballots at a rate similar to before the change, but infrequent voters' turnout rates decline. I therefore expect the earliness of early voting to be far more convenient for habitual than non-habitual voters.

In sum, the typical early voting system should be inconvenient for non-habitual voters and somewhat convenient for habitual voters. I offer two specific hypotheses:

H1: Individuals with weak voting habits cast early ballots at a higher rate in jurisdictions with many early voting sites in familiar, frequently traveled locations (relative to jurisdictions with a small number of sites in non-familiar, non-frequently traveled locations).

H2: Jurisdictions with early voting sites in familiar, frequently traveled locations have higher aggregate turnout (relative to jurisdictions with a small number of sites in non-familiar, non-frequently traveled locations).

The first hypothesis relates to the composition of the electorate. With regard to my proposed data, I expect that non-habitual voters cast early ballots at a higher rate in Clark County than in Maryland, all else being equal. Non-habitual voters' share of the electorate should be higher in Clark County because early voting is more convenient for them there. Nevertheless, the overall composition of the electorate in both jurisdictions should still be heavier with habitual voters (their share larger than non-habitual voters' share). After all, voting is still more automatic for individuals with strong habits, and

they should always vote at a higher rate than non-habitual voters, regardless of the legal or institutional context.

The second hypothesis relates to both the size and composition of the electorate. It suggests that more people will cast ballots in elections in which early voting is more convenient. Any effect should be driven primarily by higher early voting rates among non-habitual voters. But habitual voters may also vote early at a slightly higher rate in jurisdictions where early voting is more convenient.

Additionally, the Maryland data allow me to estimate the effects of a typical state's early voting policy on habitual and non-habitual voters in a more precise manner than prior studies. The typical state offers a small number of early voting sites at traditional locations, like Maryland. I offer two hypotheses that are similar to H1 and H2, but for which the comparisons are for the same jurisdiction *without early voting* (rather than a different jurisdiction with convenient early voting sites).

H3: Individuals at all voting habit strengths cast early ballots at a slightly higher rate after a jurisdiction institutes early voting at a small number of traditional locations.

H4: Jurisdictions have slightly higher aggregate turnout after instituting early voting at a small number of traditional locations.

As suggested above, early voting—when instituted in the typical manner—is not particularly convenient for non-habitual voters. For habitual voters, it may afford them an opportunity to align their voting behavior with their needs and identities as voters, to cast ballots early to ensure they will avoid potential discomfort or self-esteem loss from not voting. But the usual manner of early voting is not so convenient that it entices many more habitual voters to vote than would just vote on Election Day without it. So while I

do not expect any of the turnout effects in Maryland to be large, and will likely be near 0, they are useful to estimate.

Is convenience and automaticity the actual moderator of early voting effects for non-habitual voters? If I observe higher early voting rates for them in Clark County than Maryland, how might I confirm that the difference is due to the many early voting sites in frequently traveled locations—rather than other factors? I will control for basic demographic and political characteristics that differ across the jurisdictions, including gender and party affiliation. But it is possible that Clark County more effectively disseminates information about its early voting procedures, or spends more on public relations for election administration. This would not disconfirm my basic proposition that convenience and automaticity increases the likelihood of non-habitual voters voting—since having more information about voting procedures would make the process more convenient and automatic. But it would not allow me to confirm the specific propositions in H1-H4 about the number of early voting sites and their convenient locations.

At first glance it appears as though location and convenience must be having some effect. Citizens in Clark County utilize early in-person voting at far higher rates than citizens in Maryland. About 55% of Clark County ballots were cast early in 2010, and 14% in Maryland. And there are far more early voting sites, especially in frequently traveled locations in Clark County. Clark had about 84 early voting sites that year; Maryland had less than 3 per county. But without conducting any empirical analysis, it's unclear whether non-habitual voters are taking advantage of early voting in Clark at a

higher rate, and whether the individuals voting early would have voted on Election Day otherwise.

To determine whether more early voting sites at commonly traveled to locations boosts early voting rates for non-habitual voters, I will analyze the effects of geographic proximity. I propose that:

H5: Living closer to an early voting site increases the likelihood of early voting more for non-habitual voters than habitual voters.

This should be true if non-habitual voters are more sensitive to location- and travel-related convenience in their early voting behavior.

## **Method**

I will use data from the Maryland and Clark County voter files to conduct the analyses. In the Maryland data, I will select a random sample of 10% of registered voters in the current voter file. Using the voter files is advantageous because they contain validated voting information. Self-reported voting information would likely bias the regression estimates for habitual and non-habitual voters because habitual voters are more likely to misreport voting (see Bernstein et al. 2001 and the discussion of misreporting in Chapter 3).

For all analyses, I will estimate multinomial logistic (MNL) regression models. The dependent variable will have four values: non-voting, Election Day in-person voting, early in-person voting, or absentee voting. Since MNL assumes the independence of irrelevant alternatives (IIA), I will perform a Hausman test to determine the validity of this assumption. There is strong reason to believe the IIA assumption is not valid in this case—that the probability of choosing one alternative is dependent on the presence of other alternatives. For example, it is likely that the decision to vote in person on Election

Day rather than not vote is influenced by the presence of early in-person voting. In fact, there is evidence of this: when presented with the option of early voting, some voters will choose to vote early instead, presumably because it is more convenient.

Nevertheless, some prior research (in fact, the Dyck and Gimpel study using the 2002 Clark County data) found the IIA assumption to be valid using this dependent variable with four outcomes. If the IIA assumption is invalid, I will instead estimate separate binomial logistic models for each two-way comparison of outcomes, limiting the sample to individuals who choose one of the two outcomes. As Long (1997) notes, this strategy is inefficient, but more appropriate if IIA is invalid.

One problem is that the voter files do not include a measure of voting habit strength. However, I have extremely detailed information on every registrant's frequency of prior voting. As I argue in Chapter 2, using a measure of prior voting frequency to estimate the direct effects of habit on turnout is inappropriate, since it captures the effects of factors that influenced voting in the past but are unrelated to habit strength. However, with this analysis I do not seek to estimate the precise direct effects of habit strength on turnout. I will use voting habit strength as a moderating variable—to determine whether early voting systems have a greater turnout-enhancing effect for habitual or non-habitual voters. So this particular statistical concern should not influence the analysis.

Using a measure of prior frequency as a proxy for habit strength might introduce bias for another reason though. Prior frequency is a strong predictor of habit strength, but there are some discrepancies between prior frequency and habit strength. I demonstrated in Chapter 4 that the discrepancies can be explained largely by the stability of the voting location. Individuals who vote in a stable location over time will develop the habit more

quickly (thus, they will have higher habit strength scores relative to their frequency score) and individuals who move residences will develop the habit more slowly (thus, they will have lower habit strength scores relative to their frequency scores). This difference is mainly evident for individuals with weak habits. Highly habitual voters are not influenced as much by residential/geographic context disruptions, as the results demonstrated.

If I hold constant the residential context—by analyzing only individuals who have not moved residences in the prior 10 years, for example—then I should obtain an unbiased estimate of the turnout-enhancing effects of an early voting system by level of habit strength. This is the strategy I use. I will analyze only individuals who have not moved in the prior 10 years, and use frequency of prior voting as a proxy for habit strength.<sup>10</sup> I will measure frequency of prior voting as the number of times voted in the previous 8 midterm and primary elections.

I first test H3 and H4. I will estimate a single MNL regression model for general election voting using data from Maryland in 2008, 2009, 2010, 2011, and 2012. The model will include dummy variables for each of the years, along with controls for gender, partisan affiliation, and county of residence. If the 2010, 2011, and 2012 dummy variable

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<sup>10</sup> There are tradeoffs in using this analytical strategy. But I consider it the optimal strategy. Eliminating movers makes the conclusions about early voting reforms less generalizable. But using an appropriate proxy for habit (by analyzing frequency of past voting and simultaneously dropping movers from the data) is also important. I could instead conduct a nationally representative survey with the VHSI items, and attach information on the convenience of each respondent's county and state early voting procedures. This process would be enormously labor-intensive and likely generate error in the measurement of convenience. For now, as a sensitivity test, I will estimate the MNL models with and without excluding movers to determine whether the exclusion alters the substantive results. If it does not, I will include movers.

coefficients are positive and statistically significant, then H4 is supported.<sup>11</sup> H4 states that instituting early voting in the typical manner (with a small number of early voting sites at traditional locations like government offices and schools) is associated with slightly higher aggregate turnout. Recall that Maryland allowed early voting beginning in 2010.

To test H3, I will also include the habit variable in the model, and interact each year dummy with habit. H3 states that individuals at all voting habit strength levels cast early ballots at slightly higher rates after the typical early voting system is instituted. If this is true, the coefficients for the interactions between habit and the 2010, 2011, and 2012 dummy variables should not be significant, but the main effects terms for the 2010, 2011, and 2012 dummies should be positive and significant. If any of the interactions are significant, it indicates that the early voting system had a differential effect on either habitual or non-habitual voters.

H1 and H2 describe the effects of convenient and inconvenient early voting systems on turnout. I will estimate two models: one for Maryland in 2010, and one for Clark County in 2002. These jurisdictions had similarly salient midterm elections in those years. I will include the habit variable in the model, along with the controls for gender, party affiliation, and county of residence (in the Maryland model).<sup>12</sup> As a sensitivity test, I will also estimate the Maryland model for the most populous counties

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<sup>11</sup> To confirm that the results are not sensitive to decisions about which years of data to include, I will also estimate all MNL models separately for each year. I will focus on comparing coefficients in 2008 and 2012 (since they are both presidential election years) and 2009 and 2011 (since they are both municipal election years).

<sup>12</sup> If I obtain the data from Catalist rather than the states, I will use the racial category variable as an additional control. I suspect that there will be differences in racial diversity between the jurisdictions. And although I do not suspect that those differences will be associated with differential reliance on early voting by habitual and non-habitual voters, I would like to control for it if possible.

only: Baltimore, Montgomery, and Prince George's counties. To the extent that there are any significant differences in the coefficients for the habit variable between the populous-county and full-state models, I will rely on the populous-county model. Clark County (where Las Vegas is located) also has a large population.

Finally, to test H5, I will include the geographic distance variables in Clark County model.<sup>13</sup> H5 states that living closer to an early voting site increases the likelihood of early voting more for non-habitual voters than habitual voters. I will examine the effects of two geographic variables: one measures the distance of the registrant's home to the nearest early voting site, and one measures the distance to the nearest precinct voting site. Both variables are continuous, and will be included in the MNL model. I will interact both with habit strength. Shorter distances to the early voting site indicate more convenient early voting, and should be associated with higher early voting rates for non-habitual voters. The interaction between early voting site distance and habit strength should be negative and significant in order to confirm H5.

## **Discussion**

The results of these proposed analyses will have important implications for both electoral policymaking and theories of turnout. The current scholarly consensus is that early voting influences turnout only minimally and, to the extent that it has any effect, substitutes for Election Day voting among habitual voters. But early voting systems are not uniformly convenient, as many studies assume. Enormous variation exists in the convenience of early voting systems across counties and states. Clark County's system, and the other most convenient early voting systems, may facilitate higher turnout among

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<sup>13</sup> I will obtain the distance variables from the Dyck and Gimpel data, or construct them with assistance from GIS experts if necessary.

individuals with weak habits than typical early voting systems, like Maryland's. The typical system is not particularly convenient for non-habitual voters.

Many scholars appear satisfied to conclude that no election procedure reforms can increase the diversity of the electorate. Berinsky (2005) distinguishes between direct and indirect costs of voting—with direct costs including the inconvenience of registering and voting, and indirect costs including interest in the candidates and issues, political efficacy and engagement. Berinsky argues that electoral reforms aimed at decreasing the direct costs (procedure-related reforms) will not boost turnout or expand the electorate much, since they will not entice “transient” voters to cast ballots; they will instead just make it easier to retain “core” voters. This conclusion is far too tentative to make given the status of empirical research on early voting (and other election procedure reforms), and likely incorrect.<sup>14</sup> We already know that reducing the direct costs of voting increases turnout when the direct costs are truly inconvenient. Election Day registration studies provide the best evidence for this. Instituting Election Day registration clearly increases aggregate turnout in states, especially among typically non-habitual voters (see note 13).

So rather than compare the effects of reforms aimed at reducing the direct and indirect costs, we should distinguish between the many varieties of direct costs. We should examine which direct costs (registration, number and location of early voting sites, online voting procedures) influence behavior most. Expanding Election Day

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<sup>14</sup> Berinsky's claims (2005, 483-4) that Election Day registration (EDR) does not expand the electorate, in particular, are overstated. While EDR may not decrease income inequality between voters and nonvoters in prior studies, institutional reforms may increase turnout for groups besides low-income voters. For example, there is strong evidence to suggest that EDR enhances turnout, especially among young, typically non-habitual voters (Knack and White 2000; Highton and Wolfinger 1998). The Knack and White (2000) study that Berinsky cites as evidence for EDR's failings concludes, “The adoption of EDR is found to be associated with large and significant improvements in the turnout rates of young persons relative to older persons, and of recent movers relative to nonmovers.”

registration beyond a handful of states, or instituting online voting, will surely make voting more convenient for the average non-habitual voter. And this should increase aggregate turnout rates at a faster rate than attempting to make the population more politically interested and efficacious (Berinsky's proposed strategy for influencing the indirect costs of voting).

Such a research agenda will require drawing more heavily on individual level theories of turnout, particularly ones rooted in psychological approaches to understanding habitual voting, and conveniences that facilitate habitual voting. I offer one such analysis in this chapter. Fitting with the theme of the dissertation, the chapter suggests that understanding the psychological influences on turnout is essential for understanding effects of external factors—legal, structural, campaign ones—on turnout.

The results will be increasingly relevant to policymakers in 2014 and 2016. Electoral policies have been viewed in recent years as increasingly important to election outcomes. There were high-profile debates in 2008, 2010, and 2012 about Ohio and Florida's early voting restrictions, and other states' voter identification laws. Such debates transformed the topic of early voting from an often-quiet administrative discussion among state and local officials to a national, often-heated partisan concern. Thus, policymakers should be increasingly eager to pass new laws and regulations to expand and restrict early voting—under the assumption that early voting laws increase turnout.

As of now, research indicates that this assumption is incorrect. But if the propositions in this chapter are confirmed, we should reevaluate prior conclusions. Regardless, future research on early voting is best served by analyzing the conditions

under which early voting is convenient for non-habitual voters. If highly convenient early voting boosts turnout among non-habitual voters, then instituting convenient systems should expand the electorate more quickly than many other reforms. Voting is highly self-perpetuating; enticing a non-habitual voter to cast a ballot generates future, downstream turnout benefits far greater than enticing habitual voters to vote. This would have the additional advantage of making the electorate more representative.

## **Chapter 7**

### **Conclusion**

Voting is the primary means by which citizens express their political preferences in a republican system of government. And voting is the most common direct form of political participation in the United States. About 55 to 60 percent of eligible citizens vote in presidential elections. About 40 percent vote in midterm elections. These voting rates might be interpreted as surprisingly low or surprisingly high.

Many media pundits and proponents of civic engagement fall into the “surprisingly low” category, lamenting that turnout is not higher and calling for additional electoral reforms. Some are fond of highlighting perceived apathy among younger citizens and calling for greater individual motivation. Some blame political polarization. But they argue that turnout rates should be higher. If, after all, voting is the primary means by which citizens express their political preferences with a republican system of government, then everyone should be doing it. This assumes that people vote to influence policy outcomes by changing the composition of legislatures and other bodies of elected officials to reflect their specific preferences. And this, of course, is illogical. Even Downs (1957) acknowledges the limits of his rational calculus of voting and that an individual’s vote will never be pivotal in influencing policy outcomes.

Alternatively, we might be astonished that people vote at all—and see turnout rates as surprisingly high. Voting is effortful, and there are no formal repercussions for not turning out. Understanding that voting generates changes in our self-concept, automatic behavioral tendencies, and emotional states helps explain the act. So rather than asking people to vote to influence the composition of government or vote out of

civic duty, we might find a way of harnessing individuals' desire to maintain a positive self-image or feel good by going to the polls. We should do this by finding a way of letting us maintain our illogical beliefs about voting being altruistic and impacting policies.

The broad perspective in this dissertation is more in line with the view that turnout is surprisingly high. We likely learn less about turnout and methods of boosting turnout when we enshrine it as an extraordinary activity that always has altruistic motives as some pundits and proponents of civic engagement contend. Instead, we can understand it most effectively by treating it like many other activities—including donating blood, recycling, attending church, and attending sporting events—that generates psychological rewards and can be perpetuated by strengthening the habit. If there is anything special about voting, it is just that it generates more psychological rewards than the average activity carried out in a day. Of course, from the perspective of society, voting is quite important and when aggregate turnout drops, we see more inequality in participation (Lijphart 1997) and weaker political representation. But from the individual's perspective, understanding turnout from the perspective of influencing policy outcomes or even altruism is insufficient.

The dissertation undertakes an in-depth examination of the role in habit in voting. Political scientists have long hypothesized about that role, and it is necessary to explicate it fully. In Chapter 2, I draw on the large body of research on habitual behaviors to specify the concept of habit, the components of the habit formation process, and offer a theory of turnout focused on habit formation. While there is scholarly debate about some components, the perspective offered in Chapter 2 has strong support from a wide array of

prior research. Researchers also clearly acknowledge that behaviors with fewer opportunities for repetition—like blood donation, voting, and sporting event attendance—may not reach the same strength of habit as daily and weekly activities. But habit scholarship has addressed annual and biannual activities and not been dismissed.

It is useful here to discuss several “meta” issues regarding the likely skepticism inherent among political science researchers (including myself at times previously) about how habitual voting is. I touch upon this elsewhere in the dissertation. Socially important behaviors like voting are more likely to be viewed as activities dominated by intentions and strong conscious motivation, in part because of their social import. But there is no evidence that socially important behaviors evade the basic processes underlying repeated behavior to my knowledge. Of course, voting will never become as automatic as riding a bike or brushing teeth. But I argue and provide evidence that voting still generates some of the features of the psychological disposition indicative of habits—automaticity linked to the context, and centrality of voting to the self-concept.

The degree of acceptance of the habitual voting evidence rests in part on the measure of voting habit strength. In Chapter 3, I develop and seek to validate a measure of the strength of voting habits using an index of self-reports, motivated by Verplanken and Orbell’s (2003) index for daily and weekly activities. The index directly measures the psychological disposition of voting habit strength. If future political scientists accept that the VHSI validly represents the habit construct, then the evidence for habitual voting is fairly clear. Voting habit strength significantly shapes turnout through a range of causal paths emphasized in theory and evidenced throughout the dissertation. If future political scientists do *not* accept that the VHSI validly represents these constructs, I

believe their methodological burden is to determine why the VHSI and voting are so strongly associated, especially through the theoretically outlined paths.

The results in Chapter 3 demonstrate that the significant association between the VHSI and voting persists after controlling for political efficacy, emotions about the candidates and issues in the upcoming election, general political interest, interest in the candidates and election issues, perceived closeness of the election, strength of candidate support, and many other variables. The only control variable to significantly attenuate the association is self-reported likelihood of voting—which also attenuates the effects of many other variables, like efficacy, interest in politics, and interest in the candidates and issues in the election. (Estimating models of turnout before including likelihood, these constructs are generally statistically significant, but many are not after controlling for self-reported likelihood of voting. In simple direct-effects specifications, this occurs with habit strength too.) This indicates that perhaps many predictors of turnout are mediated by voting intentions. But such evidence should not rule out their causal effects. Even if interest in the candidates and strength of candidate preferences is fully mediated by intentions, those interest-related constructs would still be useful to explain turnout. The same is true of habit.<sup>1</sup>

There is likely at least some measurement error in the voting habit strength index. As the VHSI is the first significant attempt to measure habit strength in the domain of voting, I imagine it would. The index could do a better job discriminating among individuals with very strong habits. I perhaps underestimated in the question construction how important voting was to many individuals' voting identities, for

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<sup>1</sup> See the second half of Chapter 4 for an examination of the systematic differences between voting intentions and habit strength.

example, and did not draw on a broader range of more discriminating identity items. The blood donation literature, in particular, might be surveyed to assist in constructing improved identity-relevance measures (Masser et al. 2008). More systematic assessments of the automaticity component of the index, besides the response time analysis I conduct in Chapter 3, would be also convincing. In general, I believe that if researchers can find a way of measuring the variation in the strength of voting habits, that variation will prove useful in explaining voting outcomes. There appears to be significant underlying variation, even if absolute habit strength scores are lower than for daily and weekly habits. And the VHSI appears to validly captures that variation.

In Chapters 4, 5, and 6, I provide evidence that understanding habits is useful in explaining voting outcomes. Specifically, in Chapter 4, I test the key hypotheses in the theory of habitual voting, and find strong support for many of them. The findings about residential mobility—that once habits are formed, residential mobility disruptions are relatively inconsequential for voting—are particularly interesting for the existing turnout scholarship. It fits with prior research findings that residential mobility decreases turnout rates, and improves upon it significantly by explaining why and under what conditions this occurs. I also find that the time of day people vote is relatively inconsequential for voting habit formation and turnout, which clarifies what we mean by “contextual stability.”

Chapter 4 has important implications for the specification of turnout models in political science. I present strong evidence that many of the “costs” and “motives” variables examined in political science research are moderated by voting habit strength. This builds on Aldrich et al. (2011), who found evidence for this using a rough

conceptualization and measurement of habit. Future research on turnout should account for the heterogeneous effects of costs and motives in future research. The most effective way would be using a direct measure of voting habit strength like the VHSI.

Chapter 4 also tests many theoretically important propositions about the reciprocal relationships between turnout and psychological rewards. The results demonstrate that voting generates several specific psychological rewards: increased internal efficacy, decreased Election Day anxiety, and a more positive mood. Moreover, most of the psychological rewards I examine—other than Election Day pride—are more strongly related to turnout for non-habitual voters. This is strong evidence that the rewards incentivize voting in the early stages of the habit formation process. These findings are particularly consequential since prior psychology research has often failed to conceptualize and measure “rewards.”

Voting for the winning candidate moderately increases Election Day pride and energetic mood. I discuss this effect briefly now, since it is not heavily emphasized in Chapters 2 and 4. The winning-candidate propositions must all be tested further in future studies. I suspect there are some effects not uncovered in these data, largely because of Minnesota’s contentious recount at the time, and there being no obvious election winners that November until after the post-election survey was administered. But I suspect that the effects of voting for the winning candidate on mood and efficacy (and then on future turnout) are less consequential to habit formation. After all, one cannot even know who the “winner” is until later. The winning candidate effects are therefore less strongly associated with the specific Election Day performance context. Without being as strongly linked to the context, the effect should be more separate from the usual habit

formation processes. In general, I suspect researchers scouring future data for winner-voting effects on turnout and efficacy and mood will be less fruitful (but see Valentino et al. 2009).

In Chapters 5 and 6, I examine in further depth what factors “make” and “break” voting habits. Chapter 5 examined the results of a randomized experiment on the effects of specific mobilization messages on turnout. Interestingly, my initial interest in the topic of habit formation began with thinking about behavioral scripts and the effects of imagery scenarios on behavior (in Mark Snyder’s psychology Ph.D. seminar on the self). These ideas generated curiosity about the predictors of behavior, mental representations underlying repeated behavior, and scripts—which are related to but different from habits. The idea for the experiment in Chapter 5 was rooted in, and probably overly motivated by, my prior conceptions of habits as behavioral scripts. I surmised that mobilization messages might “fill in” some of the psychological pieces lacking in non-habitual voters, particularly with strong imagery language, thus substituting for the habit and increasing the likelihood of voting. But it is clear that one of the reasons the messages in Chapter 5 are not shown to be particularly powerful is that priming behavioral scripts, even through powerful imagery wording, cannot substitute for behavioral experiences learned through repeated action in a stable context in the habit formation process. As I discuss briefly elsewhere, mobilization messages do not have the same effect on turnout as the actual act of voting. So the mechanisms underlying the message effects in Chapter 5 are probably somewhat different from those in habit formation.

Nevertheless, there are several interesting propositions and findings with important implications related to habit strength in Chapter 5 analysis. Primarily, actions-

planning messages have the potential to decrease the likelihood of voting for non-habitual voters. Voter mobilization research has begun to specifically examine the effects of actions-planning messages (Nickerson and Rogers 2010), which motivate my study, and which I build on. Unlike with other mobilization message types, prior research has found that the actions-planning-type messages (there are different operationalizations) can sometimes demobilize some groups. The results in the experiment demonstrate that clearly this is primarily for non-habitual voters. Moreover, I demonstrate at several points that it is voters with weak voting identities that are most demobilized, and whose turnout effects are most different from habitual voters (or strong-identity voters). I believe this has some broad implications, which are not quite realized in this study. Individuals with weak identities as voters may be the ones most sensitive to mobilization messages and contact in general. Future research will need to test this systematically.

The other clear empirical result in Chapter 5 is that the actions-planning message had entirely different effects for habitual and non-habitual voters. While the underlying mechanisms are not always clear, I surmise that this is because of the way individuals with weak voting habits translate their intentions into behavior. I cannot test these mechanisms specifically, and suspect that intentions-related changes caused by *external* sources, like mobilization messages, and *experiential* sources like the habit formation process, influence future behavior differently. But the finding nevertheless has important implications for studies of turnout and mobilization, which frequently use voting intentions as dependent variables intended to proxy for actual turnout. In some situations this strategy could be reasonable, as when differences between habitual and non-habitual voters are not expected to be great. But when significant heterogeneity by habit strength

level exists, researchers should use validated turnout as the dependent variable in mobilization and turnout studies.

Chapter 6 presents a research design for examining the effects of early voting policies on habitual and non-habitual voters. I offer specific propositions about what “convenience” and “convenience-boosting” electoral policies mean in terms of psychological automaticity and identity as a voter. The implications are unclear until I carry out the study, but if the propositions are confirmed, I believe the most important implication will be that early voting only boosts turnout for non-habitual voters when states and localities offer many nontraditional sites (e.g., supermarkets, shopping malls, and familiar and highly frequented locations) at commonly traveled locations.

In general, I conclude that the dissertation helps answer the question, “Why do people vote?” with a more clear, parsimonious, and empirically supported explanation than offered in prior research. There are many aspects of the voting process not described in this dissertation at all, like the role of genetics, personality, and social norms. Personality-related factors like openness to experience might mediate or moderate some of the habit-related effects. But the theoretical framework presented might be expanded quite easily to incorporate these effects. For example, it is likely that many social norms for voting that come from one’s family or specific social circles—the ones thought to be defined by education by Rolfe (2012)—shape turnout by determining when and to what extent one receives psychological rewards from voting. Rolfe’s analysis does not examine any of the underlying mechanisms. The habit-related mechanisms are critical to understand since, if social norms are mediated by habit processes like rewards, then one’s social structure is only consequential at early stages in the voting habit formation process.

Scholars will have a more accurate picture of why people vote by understanding the conditions under which other factors (like social norms or political interest) matter, including when they are most likely to disrupt voting habits with lifelong implications. I suspect that turnout research will accumulate far more quickly when scholars begin to focus more on the important, proximate, habit-related causes of turnout.

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## Appendix

**Appendix Table 1. Balance of Covariates Across Experimental Conditions and Habit Strength Group**

	Weak Habit Voters			Strong Habit Voters		
	Actions	Rewards	Control	Actions	Rewards	Control
Age	42.75	40.36	41.93	52.20	50.13	50.46
Non-white	0.07	0.09	0.08	0.04	0.04	0.01
Race/ethnicity missing	0.12	0.12	0.12	0.09	0.05	0.06
High school	0.10	0.07	0.08	0.11	0.10	0.09
College/some college	0.55	0.52	0.53	0.53	0.55	0.52
Post-college	0.27	0.36	0.30	0.30	0.33	0.37
Education DK/missing	0.08	0.05	0.09	0.05	0.02	0.02
Length at residence	0.64 <sup>a</sup>	0.55 <sup>b</sup>	0.54	0.79	0.76	0.78
Free time in day	2.48	2.57	2.69	3.07	2.86	2.80
Campaign contact	0.43	0.54 <sup>a,b</sup>	0.42	0.52	0.63 <sup>a,b</sup>	0.50
Interest in politics	0.61	0.69 <sup>a,b</sup>	0.61	0.82	0.81	0.84
Self-reported likelihood voting	6.63	6.66	6.74	7.97	7.95	7.94
CD 1	0.11	0.09	0.12	0.09	0.17 <sup>a,b</sup>	0.06
CD 2	0.24 <sup>a</sup>	0.17	0.12	0.11	0.17	0.12
CD 3	0.11	0.15	0.12	0.21	0.10 <sup>b</sup>	0.15
CD 4	0.10	0.18 <sup>b</sup>	0.12	0.16	0.11	0.13
CD 5	0.13	0.16	0.14	0.09	0.09	0.13
CD 6	0.16	0.12	0.12	0.11	0.16	0.13
CD 7	0.09	0.04 <sup>a</sup>	0.12	0.11	0.10	0.07
CD 8	0.05	0.05	0.10	0.11	0.10	0.17

Notes: Weak habit voters defined as respondents below median habit strength value. Strong habit voters are respondents above median value.

<sup>a</sup> Significantly different from control ( $p < .1$  using two-tailed test)

<sup>b</sup> Significantly different from actions ( $p < .1$  using two-tailed test)

**Appendix Table 2. Balance of Covariates Across Experimental Conditions**

	Actions	Rewards	Control
Age	47.78	45.24 <sup>b</sup>	45.61
Non-white	0.05	0.06	0.05
Race/ethnicity missing	0.11	0.09	0.13
High school	0.10	0.08	0.08
College/some college	0.54	0.53	0.51
Post-college	0.28	0.34	0.32
Education DK/missing	0.08	0.05 <sup>a</sup>	0.09
Length at residence	0.72 <sup>a</sup>	0.65 <sup>b</sup>	0.64
Free time in day	2.80	2.72	2.74
Campaign contact	0.47	0.58 <sup>a,b</sup>	0.44
Interest in politics	0.72	0.75 <sup>b</sup>	0.71
Self-reported likelihood voting	7.35	7.32	7.26
CD 1	0.10	0.13	0.10
CD 2	0.17	0.16	0.12
CD 3	0.17	0.12	0.13
CD 4	0.13	0.15	0.13
CD 5	0.11	0.12	0.13
CD 6	0.13	0.14	0.13
CD 7	0.10	0.07	0.10
CD 8	0.08 <sup>a</sup>	0.07 <sup>a</sup>	0.13

Notes: <sup>a</sup> Significantly different from control ( $p < .1$  using two-tailed test)

<sup>b</sup> Significantly different from actions ( $p < .1$  using two-tailed test)

**App Table 3. OLS Estimates for Self-Reported Likelihood of Voting (N=691)**

	[1]	[2]
Actions message	0.006 (0.006)	0.055 (0.024) *
Rewards message	0.001 (0.006)	0.010 (0.023)
Actions x Habit	-- --	-0.062 (0.030) *
Rewards x Habit	-- --	-0.011 (0.029)
Habit strength	0.083 (0.016) *	0.105 (0.023) *
Baseline self-report. likelihood	0.907 (0.014) *	0.909 (0.014) *
Residential stability	-0.010 (0.008)	-0.011 (0.008)
Campaign contact	0.000 (0.005)	-0.001 (0.005)
Interest in politics	0.028 (0.013) *	0.029 (0.013) *
Strength candidate pref.	-0.012 (0.007) *	-0.012 (0.007) *
Intercept	0.013 (0.012)	-0.005 (0.017)
Model F	1039.8 $p < .01$	835.9 $p < .01$
Adj. R <sup>2</sup>	0.923	0.924
Root MSE	0.066	0.066

Notes: \*  $p < .1$  using two-tailed tests. Standard errors in parentheses.

**App Table 4. Probit Estimates for Validated Voting (N=699)**

	[1]	[2]
Actions message	-0.209 (0.174)	-0.900 (0.523) *
Rewards message	-0.141 (0.174)	-0.112 (0.499)
Actions x Habit	-- --	1.020 (0.761)
Rewards x Habit	-- --	-0.024 (0.720)
Habit strength	1.817 (0.346) *	1.516 (0.558) *
Residential stability	0.680 (0.216) *	0.705 (0.217) *
Campaign contact	0.157 (0.139)	0.154 (0.139)
Interest in politics	0.915 (0.325) *	0.910 (0.335) *
Strength candidate pref	0.584 (0.204) *	0.584 (0.204) *
Intercept	-1.402 (0.241) *	-1.216 (0.343) *
Wald $\chi^2$	134.6 p<.01	136.2 p<.01
Pseudo R <sup>2</sup>	0.276	0.281

Notes: \* p<.1 using two-tailed tests. Robust standard errors in parentheses.

**App Table 5. Probit Estimates for Validated Voting**

	[1]	[2]
Actions message	-0.18 (0.40)	-0.43 (0.72)
Rewards message	0.11 (0.40)	0.45 (0.59)
Actions x Frequency	0.08 (1.51)	-- --
Rewards x Frequency	-0.97 (1.52)	-- --
Frequency prior voting	4.34 (1.07) *	-- --
Actions x Intentions	-- --	0.28 (0.78)
Rewards x Intentions	-- --	-0.69 (0.65)
Intentions to vote	-- --	2.99 (0.51) *
Residential stability	0.42 (0.23) *	0.60 (0.24) *
Campaign contact	0.11 (0.14)	0.15 (0.15)
Interest in politics	1.04 (0.32) *	0.63 (0.37) *
Strength candidate pref	0.72 (0.21) *	0.31 (0.22)
Intercept	-1.27 (0.31) *	-2.29 (0.46) *
N	703	701
Wald $\chi^2$	109.6 p<.01	162.0 p<.01
Pseudo R <sup>2</sup>	0.307	0.417

Notes: \* p<.1 using two-tailed tests. Robust standard errors in parentheses.

**App Table 6. OLS Estimates for Self-Reported Voting Likelihood, Among Action & Control Groups (N=429)**

Actions message	0.069 (0.029) *
Actions x Habit-Identity	-0.067 (0.036) *
Actions x Habit-Automaticity	-0.006 (0.038)
Habit-Identity	0.097 (0.031) *
Habit-Automaticity	0.021 (0.027)
Baseline Voting Likelihood	0.894 (0.018) *
Residential stability	-0.005 (0.010)
Campaign contact	0.001 (0.006)
Interest in politics	0.030 (0.016) *
Strength candidate pref	-0.013 (0.009)
Intercept	-0.015 (0.020)
Model F	538.31 p<.01
Adj. R <sup>2</sup>	0.926
Root MSE	0.065

Notes: \* p<.1 using two-tailed tests. Standard errors in parentheses.

**App Table 7. Probit Estimates for Validated Voting, Among Action & Control Groups (N=432)**

Actions message	-0.973 (0.643)
Actions x Habit-Identity	1.091 (0.823)
Actions x Habit-Automaticity	0.092 (1.049)
Habit-Identity	1.466 (0.608) *
Habit-Automaticity	0.092 (0.757)
Residential stability	0.755 (0.279) *
Campaign contact	0.020 (0.183)
Interest in politics	0.923 (0.434) *
Strength candidate pref	0.556 (0.265) *
Intercept	-1.357 (0.422) *
Wald $\chi^2$	107.76 p<.01
Pseudo R <sup>2</sup>	0.321

Notes: \* p<.1 using two-tailed tests. Robust standard errors in parentheses.