

Who Votes? How and When Negative Campaign Advertisements Affect Voter Turnout

by

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For my grandfather, Khaim Kushkuley,
and for Adam Herman, who told me to stop worrying and just test my hypotheses.

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CHAPTER 1

Introduction and Overview

Do negative advertisements demobilize voters? The answer to this question is crucial. If negativity is systematically turning people away from the polls, then the very nature of American political legitimacy is in question. As Geer (2006) writes, “worries about negativity lie at the very center of concerns about the health of our electoral system and whether that system promotes a process that can be thought of as democratic” (2). In this dissertation I specify conditions under which negativity will affect voter turnout. I argue that the effect of negativity on turnout depends on the timing of exposure to negative advertising.

Most scholars of political communication agree that the relationship between negativity and turnout is not only of great importance, but something that political scientists should understand (Franz et al. 2008; Lau and Pomper 2004). This is where the agreement ends. Existing research on campaign negativity has produced a series of conflicting results. Some scholars show that negativity decreases the likelihood of turnout (Ansolabehere and Iyengar 1995). Others find that negativity actually mobilizes the electorate (Goldstein and Freedman 2002). Still others show that the overall effect of negativity is null (Finkel and Geer 1008).

The consequences of this empirical disagreement are serious. In the 2008 campaign more than 70% of all advertisements aired by John McCain were negative, while more

than 60% of all advertisements aired by Barack Obama were negative.¹ What's more, the 2008 campaign was more negative than the 2004 campaign and political analysts predict that future national campaigns will only grow more negative (Segal 2007).² In short, so long as we do not have a firm understanding of the relationship between negativity and turnout, we may draw erroneous conclusions about whether negative advertisements are merely an unpleasant part of campaigns or a systematic force of demobilization.

I argue that we have reached this empirical stalemate because we have not adequately addressed the conditions under which individuals are exposed to negativity during a campaign. In particular, scholars have not considered how the timing of negativity (i.e. when in a campaign negative advertisements are aired) can affect how advertisements change an individual's likelihood of voting. I examine the effect of timing directly.

To understand why negativity may be demobilizing, I offer a new psychological theory of the effect negative advertising has on individual decision processes during a political campaign. This theory emphasizes that two factors shape the effect of negativity on turnout: (1) most individuals select which candidate they prefer long before Election Day and (2) negativity has a different effect before and after an individual has made this selection. Combining these factors, I show that negative advertising is most likely to demobilize at a certain time: *after an individual has selected which candidate he prefers, but before an individual has had the chance to implement this selection with a vote*. Even more precisely, after selection, it is negativity about the individual's selected candidate that is most likely to be demobilizing.

I support this conclusion through five empirical tests. First, I combine data from the American National Election Study (ANES) with John Geer's dataset of campaign

¹Wisconsin Advertising Project, Press Release, October 8, 2008, www.wiscadproject.wisc.edu/wiscads_release_100808.pdf

²Wisconsin Advertising Project, Press Release, October 8, 2008, www.wiscadproject.wisc.edu/wiscads_release_100808.pdf

advertisements to analyze presidential elections 1976 to 2000. Second, I combine panel data from the National Annenberg Election Study (NAES) with advertising data compiled by the Wisconsin Advertising Project. Third, I create a new dataset of county-level turnout in the 2000 election; I combine this dataset with the Wisconsin advertising data. Finally, I use two experiments to test the mechanisms underlying my theory. The first experiment (completed in October 2007) tests the effect of negativity in a laboratory setting; the second experiment (completed in April 2008) relies on a national sample of 535 people.³ All five empirical tests reinforce my key conclusion that negativity is most likely to demobilize only at a certain time: *after an individual has made a selection, but before he has had the chance to act on this selection with a vote.*

In this chapter I set the foundation for the rest of the dissertation. First, I define negativity and explain what is usually meant by this term. Second, I discuss three contrasting sets of empirical findings within the existing literature on negativity and turnout. Finally, I provide an overview of my dissertation and my empirical findings.

1.1 What is Negative Campaigning?

Despite the broad base of literature, negativity in campaigning has often been given the “I’ll know it when I see it” treatment. West (2001) notes that “critics have widely condemned the advertising style of recent elections, but few have defined what they mean by *negativity*” (64). In fact, even Ansolabehere and Iyengar’s (1995) pivotal book on negative campaigning only defines the term by examples of experimental stimuli: “in the positive versions of these advertisements the sponsor was depicted as ‘tough’ on crime and a friend of the environment. In the negative versions, the opponent was depicted as ‘soft’ on crime and a foe of environmental protection” (49).

³In addition to these experiments, I also present the results of experiments designed to test alternative explanations. All experiments are detailed in Chapter 5.

Over the years, however, the definition of “negativity” has become clearer. These definitions of negativity can be divided into two groups: substantive and tonal. By a substantive definition I mean one that considers both the tone and the substance of the advertisement. By a tonal definition, I mean one that focuses only on the advertisement’s tone.

On the substantive side, some definitions have distinguished between “personal attacks” or “issue” appeals (Freedman, Wood and Lawton 1999), as well as between “fair” and “unfair” advertisements (Freedman and Lawton 2001). Other types of substantive definitions offer a distinction between “attack” and “contrast” appeals (Jamieson and Waldman 1997; Jamieson, Waldman and Sherr 2000), while others have focused on “mudslinging,” defined as advertisements that are “inappropriate” or “presented harshly” (Kahn and Kenney 1999, 878).

On the tonal side, Lau and Pomper (2004) define negative campaigning as “talking about the opponent – criticizing his or her programs, accomplishments, qualifications and so on”(4). Finally, in one of the most parsimonious tonal definitions, Geer (2006) argues that

It is simple and straightforward: *negativity is any criticism leveled by one candidate against another during a campaign*. Under this definition there is no gray area. An appeal in a campaign either raises doubts about the opposition (i.e., negative) or states why the candidate is worthy of your vote (i.e., positive). There is no middle category. Note that this definition does not speak to whether the criticism is about policy or about traits. Any type of criticism counts as negativity (23).

In this dissertation I rely on Geer’s definition. This definition has a number of merits, among them its breadth and relative simplicity. As Geer’s definition (as well as Lau and Pomper’s) is tonal, negativity rests on the linguistic framing of the advertisement, rather than its substantive content. In other words, an advertisement is classified as negative when one candidate uses even part of his or her air time to

critique another candidate; the substantive nature of this critique does not matter.

I make a number of tradeoffs by opting for a tonal definition. I will be unable to discern, for example, what substantive elements of negativity have the greatest effect on turnout. I will also not be able to distinguish if personal attacks have a stronger effect than issue attacks, or if mudslinging has a different effect than other forms of negativity. Nonetheless, there are a number of benefits to using the tonal definition that Geer proposes. A tonal definition that encompasses the different substantive categories of negativity will help to answer the general question posed in this dissertation: Does negativity affect turnout? As Geer argues, while the substance of negativity is undoubtedly multi-faceted, the tonal aspect of negative advertising “is a key part of the discourse used to assess political campaigns. If we introduce new terms and concepts, we may only be muddying the waters” (24). As my goal here is to speak to the broad literature on negativity and turnout, I follow Geer’s approach and rely on a tonal definition.

1.2 Negativity and Turnout: Existing Literature

After years of rigorous empirical work, studies of negativity and turnout have produced a series of conflicting results. In fact, the dominant work on negative advertising and turnout can be categorized into three basic groups: (1) negativity *decreases* turnout, (2) negativity *increases* turnout and (3) negativity does not have any unique effect on turnout. Below, I consider the dominant empirical results for each of these three groups. Other than the general disagreement about the direction of the relationship, I want to stress that this work does not account for timing of exposure to negativity. My theory – which focuses on timing – brings a new approach to the study of this topic.

1.2.1 Negative Campaigning Decreases Turnout

In one of the most pivotal (and debated) analyses of negative campaigning, Ansolabehere, Iyengar, Simon and Valentino (1994) argue that negative campaigning does not sway voters toward one candidate or another, but demobilizes individuals, turning them away from politics. Using both, aggregate data from Senate elections and experimentation, Ansolabehere et al. (1994) showed that negative advertisements led to a decrease in turnout. Ansolabehere and Iyengar(1995) replicated this effect experimentally.

It is important to consider the Ansolabehere and Iyengar experiments in some detail, as this work forms the backbone of the demobilization argument and creates a foil for more recent work. In a series of experiments Ansolabehere and Iyengar (1995) exposed subjects to negative advertisements and positive advertisements.⁴ The experiments focused on real politicians running in various ongoing political races. Subjects were exposed to negative and positive advertisements in either single-advertisement conditions or double-advertisement conditions. Moreover, while the authors kept the tonal framing constant between advertisement types, they varied the focus of the advertisements, comparing advertisements that discussed character traits to advertisements that discussed political issues.⁵

What Ansolabehere and Iyengar (1995) found was that subjects exposed to negative advertisements were less likely to report that they intended to vote in the upcoming election.⁶ In fact, the authors reported that “[i]n our experiments the effect of seeing a negative as opposed to a positive advertisement is to drop intentions

⁴Some advertisements were specifically created for these experiments; others were actual advertisements used by candidates. The use of real vs. created advertisements depended on the study, meaning some experiments used only created advertisements, others used only actual advertisements.

⁵Ansolabehere and Iyengar report no significant differences between experimental groups based on these substantive differences in advertisements.

⁶The authors did find different results for different experimental conditions. For example the authors observed different results for subjects exposed to single-ad and double-ad conditions. Nevertheless, overall tone has a distinct demobilizing effect.

to vote by nearly 5 percentage points” (112).⁷ Ansolabehere and Iyengar attributed this lower likelihood of turnout to a decline in political efficacy among individuals exposed to negative advertisements. In their experiments, exposure to positive advertisements increased confidence in the political process by 1.4 percentage points compared to the control group, and increased internal efficacy by 2.6 points (105). In short, Ansolabehere and Iyengar argued that their results “are likely to dishearten even the most optimistic observers of American politics” (112).

Despite the careful control and rigorous design of the experiments, Ansolabehere and Iyengar’s results still leave many questions unanswered. One concern is Ansolabehere and Iyengar’s mechanisms. The authors argued that negative advertisements affect turnout by decreasing efficacy, and the less efficacious a person feels, the less likely he is to turn out and vote. Although this is an intuitive argument – in fact, efficacy has been shown to be a factor in participation (Finkel 1987) – Ansolabehere and Iyengar did not directly test whether efficacy mediates the relationship between negativity and lower turnout likelihood. While both turnout and efficacy are lower for the experimental groups exposed to negative advertisements, it is unclear if it was the drop in efficacy that contributed to the drop in likelihood of turnout, or if an unknown mechanism led to the drop in *both* variables.

Other studies have attempted to replicate the findings in Ansolabehere, Iyengar, Simon and Valentino (1994) and Ansolabehere and Iyengar (1995) under varying conditions. In a 3×3 experiment that varied advertising tone, television program and ad sponsor, Kaid, Chanslor and Hovind (1992) found that positive advertisements led to a higher likelihood of intended turnout than negative advertisements. Relying on survey data, Brooks (2000) found that negative advertising was associated with lower levels of turnout in the aggregate – although she found no relationship between negativity and turnout at the individual level. In a study of the 1996 Clinton/Dole

⁷The 5 percent is a change in relation to the other treatment group – the positives – the drop-off is about 2.3 percentage points when the negatives are compared to the control group.

race, Crigler, Just and Belt (2002) found that subjects who viewed two negative campaign videos were less likely to report an intention to vote than subjects who viewed two positive campaign videos. In a final example, Houston and Doan (1999) used a laboratory experiment to show that negativity decreased the intent to vote for a hypothetical candidate.

What is striking about these examples is not the direction of their findings – after all, they confirm Ansolabehere and Iyengar’s conclusions. Rather, what is notable here is the methodology. An inventory of studies about negativity created by Lau, Sigelman and Rovner (2007) shows that most of the studies where negativity produced a lower likelihood of turnout have been experiments where individuals were exposed to negativity in a controlled setting.⁸ While experimental settings are useful for observing direct effects of campaign communication, the external validity of these studies is questionable. In a campaign, individuals are regularly exposed to streams of conflicting information, rather than a single carefully-created manipulation. Although the fact that numerous experiments replicate the demobilizing effect of negativity suggests that this relationship is plausible, the fact that so few studies using observational data show the same result gives us some reason for concern.

1.2.2 Negative Campaigning Increases Turnout

While some scholars used Ansolabehere and Iyengar’s (1994) book as a foundation for an argument about the dangers of negative campaigning, other scholars proposed a different relationship between negativity and turnout. In perhaps the most direct critique of Ansolabehere and Iyengar, Wattenberg and Briens (1999) argued that negative advertisements may actually be quite mobilizing for demographic groups that

⁸A meta-analytic analysis by Lau, Sigelman and Rovner (2007) confirms this argument. The authors create a full list of over 100 empirical studies on the effects of negativity. Of studies that show that negativity leads to a lower likelihood of turnout all but 3 are experimental (this includes the pivotal Ansolabehere, Iyengar, Simon and Valentino 1994). We can add an additional study to this set of non-experimental results if we adjust the definition of negativity to mean “mudslinging.”

traditionally exhibit low turnout levels. Among respondents who did not have a high school education, Wattenberg and Briens found that people who recalled a negative campaign advertisement in the 1992 campaign had turnout rates that were 22 percentage points higher than those who did not recall any political advertisements; they found a similar difference for individuals who recall a positive advertisement (893). Additionally, Wattenberg and Briens find no evidence that negative advertisements lead to lower levels of efficacy.

Although Wattenberg and Briens cast doubt on Ansolabehere and Iyengar's results, their analysis leaves room for questions. First, Wattenberg and Briens' independent variable – recall of advertisements – is questionable. Does this variable tap into the effect of negativity, or is it merely a measure of attention? Next, Wattenberg and Briens only find a mobilizing effect for negative advertisements in the 1992 election, but do not see one in 1996. This discrepancy is difficult to explain, and points at what is perhaps the most important weakness in this work: the authors offer no theoretical reason as to why we may expect negative advertisements to be mobilizing in the first place.

Garramone, Atkin, Pinkleton and Cole (1990) offered a stronger theoretical grounding to explain why negativity may be mobilizing. Garramone et al. (1990) argued that negative advertisements are more informative than positive advertisements. This argument stems from the correspondent inference theory (Jones and Davis 1965); by this theory, behavior that is positive and “normal” does not tell much about a person performing the behavior, while unexpected, “non-normal” behavior – the likely content of a negative advertisement – provides a better basis for judgment. “Information that a candidate fought to protect the environment (a normative behavior) may be perceived as less information than information that a candidate acted to exploit environmentally sensitive areas (a non-normative behavior)” (Garramone et al. 1990,

301).⁹ This greater “informativeness” of negative advertisements, the authors argued, may help individuals distinguish between candidates and form stronger preferences, leaving them more confident about their decisions. As a result, individuals may be more likely to participate in the political process and cast votes.

This is an intuitive theory. Geer (2006), for example, also argued that negative advertisements hold a superior level of “informativeness” to positive advertisements. Moreover, Garramone et al experimentally showed that individuals exposed to negative advertisements did form stronger candidate preferences. Still, in their analyses of turnout likelihood and other forms of participation Garramone et al. (1990) found no differences between experimental subjects exposed to negative advertisements and experimental subjects exposed to positive advertisements. In short, although Garramone et al. (1990) provided an intuitive and psychologically plausible theory of negative advertisements as mobilizers, they did not show that negativity is mobilizing.

Yet more studies do show that negativity can be mobilizing. Goldstein and Freedman (2002) used survey data combined with detailed advertising data to show that negativity increased turnout in the 1996 election. In a field experiment, Arceneaus and Nickerson (2005) found that negativity increased turnout (albeit very slightly) in Minnesota and Los Angeles.

What is notable about this group of studies is the variability in the size of the effects. While some scholars do see substantively large mobilizing effects (for example, Goldstein and Freedman 2002), others report effects that are significant but substantively small, effects that Lau et al. (2007) call “trivial.” In short, if negative advertisements are, indeed, mobilizing the electorate, the substantive impact of this mobilization is unclear.

⁹Garramone et al. 1990 use normative differently than the term is commonly used in political science work. When they use this term they simply mean “normal”, or better put, behavior that is expected within the norms of our society.

1.2.3 Negative Campaigning Has No Effect On Turnout

What if negative campaign advertisements have no effect on turnout at all? Relying on individual-level survey data from 1960-1992, for example, Finkel and Geer (1998) found that an increase in negative advertisements did not have any effect on turnout for the general population. In fact, the authors concluded that “we dispute ... that the decline in (voter) turnout can be attributed in any way to the negativity of recent campaign advertisements” (590). While Finkel and Geer found this result by considering presidential campaigns, Lau and Pomper (2004) showed the same result for Senate campaigns.

Although both Finkel and Geer and Lau and Pomper noted in their conclusions that their results disprove Ansolabehere and Iyengar’s critique of negative campaigning, their results suggest a more muddy story. While Finkel and Geer conclude that negative campaigning has no effect on turnout, their results show that negative campaigning has a mobilizing effect on respondents who classify themselves as independents. Moreover, Finkel and Geer show that negative advertisements that focused on political issues – rather than character traits – did lead to lower levels of turnout among the electorate. Lau and Pomper echo the same idea: although they do present some results that suggest negative campaigning has no effect on turnout, they also present results that suggest that negative campaigning can have a demobilizing *and* a mobilizing effect.

In a still more recent study, Krasno and Green (2008) used a natural experiment to analyze the effect of campaign advertisements by state. Relying on an innovative design which considered differences by media markets within states, the authors find little evidence that negativity affects turnout, noting “it appears that presidential ads have minimal effects on turnout regardless of tone” (257).

Finally, in two thorough meta-analytic studies of negative campaign research Lau, Sigelman, Heldman and Babbitt (1999) and Lau, Sigelman and Rovner (2007) found

equally complex results. While the authors did not find that negativity has any statistically significant effect on turnout, they did find that negativity could lead to decreasing trust in government and efficacy – factors that do generally contribute to a lower likelihood of turnout (Lau et al. 2007).

In the end, after years of empirical research, the effect of negative campaigning can be summarized as follows: it may be demobilizing the electorate, it may be mobilizing the electorate or it may be doing nothing one way or the other. Moreover, the literature is unclear about when, why and how we should expect any of these three results. My dissertation works to deal with this empirical stalemate.

1.3 Overview

In the next chapters, I build a story about the relationship between negativity and turnout. In Chapter 2, I follow Bartels’ (1996) suggestion for more “philosophical and psychological elaboration”¹⁰ on this topic and present a theory that describes why and how negativity affects a voter’s likelihood of turning out on Election Day. This theory is in part based on empirical research in psychology, consumer behavior and behavioral decision theory. I merge the findings from these disciplines with our existing knowledge of political behavior to produce the following argument: the demobilizing effect of negativity is based on the *timing* of exposure. More precisely, I argue that negativity is most likely to demobilize at a certain time: *after an individual has selected which candidate he prefers, but before he has the chance to act on that selection with a vote*. Furthermore, at this crucial time, it is negativity about the individual’s *selected candidate* that will have the most profound effect on his likelihood of turnout.

My theory leads to different predictions than the existing literature. Since I argue

¹⁰Bartels (1996) makes this comment when writing his review of Ansolabehere and Iyengar’s (1995) book.

that negativity is most likely to demobilize after a selection but before an action, it matters *when* in the campaign an individual is exposed to negativity. Timing is something that the existing literature does not consider. More than simply being a missing variable or adding an additional mechanism, however, considering timing leads to predictions about the relationship between negativity and turnout that are different from those currently made in the literature. Current literature assumes that the effect of negativity is the same at any point in a campaign. I break with this assumption and argue that negativity will not be equally demobilizing (or mobilizing) at any randomly selected point for the duration of a campaign; in fact, negativity will have distinct effects at different points in time.

My dissertation rests on five independent empirical tests. I rely on five different tests for a number of reasons. First, capturing the effect of campaign communications is a difficult task. To this extent, each methodological approach I use will inevitably have certain strengths and certain limitations. The tests I use, however, each have *different* strengths. Therefore, in relying on different empirical approaches I not only hope to deal with these various limitations, but also conduct a thorough and rigorous test of my hypotheses. A second reason to use five different empirical tests is the current state of the literature. As I discussed earlier, findings that show negativity to have a demobilizing effect come largely from experimental studies. Few studies have replicated these results using observational data. Therefore, in order to ensure that the theory I propose here functions as well during an actual campaign setting as it does in the laboratory, I rely on both, observational and experimental data.

Chapter 3 sets the empirical foundation for the dissertation with a broad analysis of seven presidential elections. This analysis combines ANES data from 1976 to 2000 with presidential advertising data from the same years.¹¹ I use this data to test a simple proposition. Many surveys show that individuals decide which presidential

¹¹Data: Geer, John. Presidential TV Ads 1960-2000, www.vanderbilt.edu/psci/geer/downloads.html

candidate they prefer by October of an election year. If that is the case, then negative advertisements aired in October should be hitting individuals at what my theory predicts to be the most demobilizing point: after they have selected the candidate they prefer, but before they have had a chance to implement this selection with a vote. Here, I show that *negativity aired in the month of October* has a significant and substantively strong demobilizing effect on turnout. In contrast, I show, much like Finkel and Geer (1998) that the percentage of negativity aired *over the entire course of the campaign* has no significant effect on turnout. These contrasting results give initial support to my key argument that timing of negativity is crucial to its effect on turnout and shows that we can observe this pattern over a series of elections.

While Chapter 3 offers broad tests of my hypotheses, in Chapter 4 I delve more deeply into the details by focusing on the 2000 presidential campaign. In this chapter I rely on two independent tests. First, I use panel data from National Annenberg Election Study (NAES).¹² I combine this data with the rich advertisement dataset compiled by the Wisconsin Advertising Project (Goldstein, Franz and Ridout 2002).¹³ This advertisement dataset accounts for the precise geographic airing of the advertisements and the number of times an advertisement was aired during a campaign. The combination of these datasets allows me to match the number of aired negative advertisements to a respondent's specific geographic location and interview date. I rely on this data to document whether or not a respondent has made a selected which candidate he prefers ("selection state"), and to show that negativity has a very different effect on respondents who have made selections ("selecteds") than on respondents who have not yet made such selections ("unselecteds"). I use interac-

¹²I use Multiple Reinterview Panel B.

¹³Use of the Wisconsin Advertising Project data requires the following acknowledgment: "The data were obtained from a joint project of the Brennan Center for Justice at New York University School of Law and Professor Kenneth Goldstein of the University of Wisconsin-Madison, and includes media tracking data from the Campaign Media Analysis Group in Washington, D.C. The Brennan Center-Wisconsin project was sponsored by a grant from The Pew Charitable Trusts. The opinions expressed in this article are those of the author(s) and do not necessarily reflect the views of the Brennan Center, Professor Goldstein, or The Pew Charitable Trusts."

tions between a respondent's selection state and percentage of aired negativity to show that negativity has a significant *demobilizing* effect only on the selecteds. The results reinforce my main prediction: *negativity is most likely to demobilize respondents after they have selected which candidate they prefer, but before they implement this selection with a vote.*

A second empirical test in Chapter 4 considers the relationship between negativity, selection and turnout from a different perspective. Since the geographic targeting of campaigns is not random, I consider the effect of negativity on turnout while accounting for the strategic distribution of advertising. Here, I use a dataset of county-level turnout in the 2000 election. This is a new dataset I compiled specifically for this project and it relies on measures of voter eligible populations (VEP). I again merge this data with advertising data from the Wisconsin Advertising Project. I use this data to produce a two-stage model. In the first stage I predict the likelihood that a county received negative advertisements. In the second stage I show that increases in negative advertisements aired during October 2000 produce a significant demobilizing effect. In contrast, increases in negative advertisements aired over the entire course of the campaign have no significant effect on turnout.

While Chapters 3 and 4 present results that show negativity is most demobilizing after individuals select which candidates they prefer, Chapter 5 offers two experiments that test *why* we observe this relationship. In this chapter, I first present the results of a laboratory experiment conducted in the fall of 2007. This experiment considers how negativity interacts with the costs and benefits structure of an election. Here I set up a voting scenario in a laboratory to show that negativity after selection makes an individual significantly less likely to pay the costs of casting a ballot.

My next experiment relies on a geographically-representative sample of 535 adults. This experiment, conducted in April 2008, asked individuals to choose between two candidates for a Senate seat. While some individuals were randomly exposed to a

negative appeal after making their selections, others were randomly exposed negatively before making their selections. In this experiment, I also employ new measures of intended turnout in order to prevent the social desirability problems that plague experiments on voter turnout. Moreover, in this study not only do I test for intended turnout, but I also included measures of my mechanisms. My results show that individuals exposed to negativity after making a selection were significantly *less likely* to report that they will vote in the upcoming election. I conclude my experimental chapter by testing for alternative explanations of my results. These tests speak to the unique nature of negativity and reinforce the key conclusions I reach in my experiments.

In sum, each of my empirical tests leads to the same conclusion – the timing of negativity matters. No matter the breadth, data or methodological approach, these tests point in the same direction and each tells a piece of the same story. Negativity is demobilizing at a certain time, after individuals select which candidates they prefer and before they have a chance to implement this selection with a vote. Thus, to observe the demobilizing effect of negativity, we need to know *when* to look.

In the final chapter of this dissertation I offer conclusions. First, I consider how my results shed light on the current empirical stalemate in the literature. Second, I discuss what my work means for political practitioners by considering candidate strategy. Finally, I apply a normative lense to the question of negativity and turnout, and consider how early voting policies may restructure this relationship.

CHAPTER 2

Theoretical Premises on Negativity and Turnout

2.1 Introduction

Over the past 40 years, as campaigns have grown in length, Americans have been bombarded with more and more negative campaign advertisements (Mark 2007). This focus on negativity as a campaign tactic has led to concerns that negative campaigning disrupts the legitimacy of the political process by depressing voter turnout (Broder 2002). Is this critique of negativity empirically justifiable?

Empirical research on negativity and turnout offers inconclusive evidence. While some scholars argue that negativity is demobilizing (Ansolabehere and Iyengar 1995), others suggest just the opposite, that negativity is actually mobilizing voters (Goldstein and Freedman 2002). Still other scholars argue that negativity does not have any independent effect on voter turnout (Finkel and Geer 1998). I contend that we have reached this empirical stalemate because existing research ignores a key factor: the *timing* of an individual's exposure to negativity.

In this chapter I propose a theory of negativity and turnout that explains why the timing of exposure is critical to the effect negativity has on likelihood of turnout. I do so as follows. In Section 2 I lay out eight premises about individual decision processes. For each premise I offer supporting evidence. I then work through the joint logical implications of these premises to derive a set of theoretical conclusions. These

conclusions are the foundation of the empirical hypothesis that I evaluate in the subsequent chapters of the dissertation. I present this hypothesis in Section 3. In Section 3 I also discuss the implications of my hypothesis using concrete examples. Section 4 considers the relationship between my predictions and the existing literature. Finally, in Section 5 I present the empirical strategy for testing my hypothesis.

2.2 Negativity and Turnout: Eight Premises

The relationship between negativity and turnout is a product of two processes: (1) the campaign and (2) an individual's decision. The first process – the campaign – is familiar to political science. In recent years, as data availability has improved, more scholars have analyzed the campaign as a process. Holbrook (1996), for example, writes “A political campaign must be understood as a process that generates a product...”(153). Hillygus and Jackman (2003) reinforce this point, writing “Much of [the past research] relies on data that measures presidential campaigns as monolithic, time-invariant events that have the same average effect for all people during all points of a campaign. Clearly the campaign is not so simplistic. Presidential campaigns consist of a series of events, activities and efforts, and the candidates may do better or worse on each of these efforts”(584). Following this logic, information about the candidates is doled out over time. The information available to voters in July of a campaign year, for example, will be different than the information available in September.

The second process – the individual's decision – is less familiar to political science. In order to understand the relationship between negativity and turnout we must understand how an individual goes about making a decision. Therefore, in this section I present a set of eight premises about decision making and the way negativity affects various parts of the decision process. Later, I will consider the joint logic of these premises in order to form the main hypothesis of this dissertation.

Premise 1: An individual's decision process has two parts.

In the first phase of a decision process the individual considers the alternatives available to him and determines which one is best (Svenson 1992). This phase is termed the pre-selection phase. In the second phase of the process the individual translates the selection he made in the first phase into an action (Dholakia and Bagozzi 2002; Svenson 1996). This second phase of the decision process is called the post-selection phase.¹ In a voting scenario, the first phase is when an individual selects which candidate he likes best. The second phase is when the individual acts on this selection by casting a vote. This paradigm of a decision as a process is a long-standing tradition; as early as 1979, Svenson wrote, “it is gradually becoming clear that human decision making cannot be understood simply by studying the final decision” (86).

In the rest of this chapter I follow the terminology presented above. I call the alternative selected at the end of the first phase of the decision process the *selection*. I call enacting that selection after the post-selection phase the *implementation* or the *action*. I refer to the combination of the pre- and post-selection phases as *the decision process*. Thus, if an individual selected the Republican but did not vote, we can say that he made a selection and engaged in parts of the decision process but did not implement his selection.

Premise 2: During the pre-selection phase, an individual differentiates among alternatives.

The purpose of this first phase of the decision process is to compare the available alternatives with the goal of determining which alternative is most attractive.² The

¹Some scholars (see, for example, Gollwitzer 1996) call the second phase of a decision process the “post-decision, pre-implementation phase.” For the sake of brevity, I will refer to this phase as the post-selection phase.

²As my focus here is on the voting decision, the concrete examples I provide deal with decisions that have only two alternatives. However, as the language suggests, the theory of decision-making presented here extends to decision contexts with multiple alternatives as well.

mechanism for determining the most attractive alternative is differentiation.³ Differentiation is an abstract concept that defines the distance between the alternatives (Svenson 1996). This idea of differentiation is not new to political science, hewing closely to Downs' party differential (1957, Ch 3). Downs defines the party differential as the difference in the utility an individual would obtain from each of the two candidates so long as there is some difference between the two utilities an individual has reason to vote. Much like Svenson, Downs argues that individuals need to have differentiation in order to have any reason to act.

Individuals can reach different levels of differentiation (Svenson 1992). Following Svenson's logic, I extrapolate that individuals who find one alternative significantly better than the others will reach wider levels of differentiation than those who find one alternative only somewhat better than another. Using this logic, we would expect strong partisans, for example, to have wider levels of differentiation between candidates than weaker partisans.

Premise 3: During differentiation negativity can be helpful.

During the pre-selection phase, negativity can help individuals differentiate between alternatives. First, negativity "may be especially useful to voters in developing their images (i.e., mental pictures) of candidates, and in differentiating or discriminating between those candidate images" (Garramone et al. 1990, 301).

Moreover, negativity can be more diagnostic than positivity and as a result can ease the differentiation process (Riskey and Birnbaum 1974; Skworonski and Carlston 1989; Yzerbyt and Leyens 1991).⁴ Yzerbyt and Leyens(1991), for example, asked subjects to select an actor suitable for a specified character role. At the start of the

³How individuals come to arrive at appropriate differentiation is a separate question. Svenson (1996) distinguishes between holistic and alternative based models. The precise mode of differentiation is important for many decision questions, but it is of little consequence here, as what is important is that individuals produce some differentiation level.

⁴Baumeister et al. 2001 extend this diagnosticity argument to numerous decision contexts, but upon a review of the literature it appears to be especially pronounced when individuals make selections between other people.

experiment, subjects were told how much information they would receive in order to make this selection, but they were instructed to make the selection at any time they felt confident enough to do so – even if they had not received the full set of information. The results of the study showed that subjects made speedier selections and relied on less information when they were given negativity rather than positivity. Put broadly, it is the presence of negativity – rather than the presence of positivity – that is more likely to signal to an individual that he has sufficient information to complete differentiation and make a selection (see Skowronski and Carlson 1989 for a discussion of this sufficiency argument).

Premise 4: Differentiation is complete once an individual passes his differentiation threshold.

An individual differentiates to pass his own selection threshold, and different individuals have varying thresholds for differentiation (Svenson 1979, 1992, 1996). Svenson (1992) defines a threshold as “ a level sufficient for a specific type of preference indication”(153). This threshold is exogenous to the selection, therefore the specific alternatives do not determine the threshold.⁵ While one individual may complete the selection phase if he prefers the Democrat only slightly more than the Republican, another may only complete the selection phase if she considers the Republican to be far better than the Democrat. Differentiation can only be complete and a selection can only be made once the individual has passed this differentiation threshold (Svenson 1992).

Premise 5: Once the differentiation threshold is passed, the unselected alternatives are transformed.

⁵However, different thresholds may be applied to different decision categories. So, an individual may have a similar differentiation threshold every time he purchases a car, even if the particular alternatives change from decision to decision. The car differentiation threshold may be different from his threshold in political decisions, which may be different from his threshold in a medical decision.

Svenson (1992) writes “the goal of [pre-selection] is not only to select the best alternative, but an alternative that is sufficiently differentiated from its closest competitor” (150). Once an individual has selected such an alternative, meaning that the differentiation threshold is passed, the unselected alternatives are then transformed from viable decision options into reference points for the superiority of the selected alternative.⁶

I arrive at this proposition about the transformation of the unselected alternatives in two steps. First, at the end of a selection phase, the unselected alternatives are perceived to be a poor choice and a bad course of action (Holyoak and Simon 1999; Simon et al. 2001). Over numerous experiments, Holyoak and Simon (1999) as well as Simon et al. (2001) find that not only do subjects maintain belief that their selected alternative was the best selection they could have made, but they were also unlikely to recall any good points about the unselected alternatives. Simon et al.(2001) explain that as individuals made selections, “the inferences that supported the chosen [alternative] became stronger, and the inferences that supported the rejected [alternative] decreased in their level of acceptance”(1257). This result is also borne out in a series of experiments analyzing the pre-selection phase of the decision process. Here Simon et al. (2001) find that over the course of four separate studies, only 5% of subjects changed their minds about which alternative was more attractive in the face of new information.

Second, individuals continue to compare their available alternatives even after a selection is made (Svenson 1992). The campaign context further reinforces these comparisons. First, the campaign constructs the decision context such that until Election Day candidates are judged relative to each other (Houston and Doan 1997). Second, in a campaign information about a candidate is almost always transmitted

⁶Following Svenson (1992), in a decision context where individuals have two alternatives – such as an American election – the second, unselected alternative automatically serves as this reference point. In a decision context where there are more than two alternatives, the “next closest competitor” to the selected alternative will act as the reference point.

with some reference to his opponent (Gulati et al 2004). Thus, an individual will use the unselected candidate as a reference point after he has selected which candidate he prefers.

I combine these two sets of arguments discussed above to propose that at the end of the selection phase the unselected alternatives transform from a possible decision option into a “non-viable reference point.” In this way, the unselected alternatives serve as a comparison point to the selected alternative; this way, the individual compares his chosen alternative to a poor option the individual would not wish to select. For the rest of the chapter I refer to the unselected alternatives as the “transformed alternatives.”

Premise 6: In the post-selection phase, individuals consider how to enact selections.

Once a selection is made, the pre-selection phase ends and the individual enters the post-selection phase. The post-selection phase focuses on action, the purpose of this second phase of the decision process is different than that of the first phase. Recall from Premise 2 that the purpose of the first phase was to consider the available alternatives and differentiate to select the most attractive one. The purpose of the post-selection phase is to enact the selection (Gollwitzer 1996).

In this phase individuals think about the basic means of enacting their selections (Bagozzi and Dholakia 1999; Sheeran 2002). In a voting scenario this may mean learning the location and hours of the polls on Election Day. Also, along with the means of action, individuals must also consider the costs and benefits of action (Busemeyer and Towensend 1993). As Busemeyer and Towensend (1993) write, “as the possibility of action increases, the attention to its consequences increases” (442).

The timing of the selection relative to action is also important in the post-selection phase. When individuals cannot immediately implement selections, they often fail to implement them at all. Evidence from various contexts supports this claim. In

particular, when action is not immediate individuals were significantly less likely to act on their intention to purchase selected consumer goods (Young, DeSarbo and Morwitz 1998), cars (Juster 1966; Pickering and Isherwood 1974) and services (Jamieson and Bass 1989). This is because during a temporal gap between the selection and the opportunity for action, events may occur to decrease likelihood of action. As Bagozzi and Dholakia (1999) note “in many cases, especially when there is a time gap between intention formation and action initiation, impediments to the enactment of the actions necessary...may occur” (29).

Premise 7: Negativity during the post-selection phase diminishes levels of differentiation.

Svenson (1992) argues that “in the [post-selection] phase potential threats can appear that may decrease ... the differential advantage for the chosen alternative” (150). I propose that one such threat is campaign negativity. While during the pre-selection phase negativity was helpful for differentiation (Premise 3), negativity has a different effect during the post-selection phase. This is because in the post-selection phase of the decision process, an individual will pay more attention to what the negativity says about his selected alternative than he did in the pre-selection phase. This increased focus on the content of negativity results from the fact that the individual is faced with the costs of action (Busmeyer and Towensend 1993), as well as the irreversibility of acting out his selection (Svenson 1996).

As a result, during the post-selection phase, exposure to negativity about his selected alternative can diminish the individual’s level of differentiation. Since an individual selected one alternative over another because he considered it superior to the unselected alternative, negativity can place that perception of superiority in doubt (Svenson 1992). Recall that according to Premise 5, the unselected alternatives have been transformed from a viable option in to a poor choice that now serves as a stationary reference point for the good qualities of the selected alternative. As

negativity leads the individual to believe that his selected alternative is no better than the transformed alternatives, differentiation levels between the selected alternative and the transformed alternatives will shrink.

What this premise also assumes is that is is negativity about the selected candidate that will have more effect on the individual's differentiation, while information about the unselected, transformed alternatives will have little effect on differentiation. This follows from Brehm's (1956) findings on post-selection exposure to new information, as well as from Premise 5 itself: if an individual has already determined that the other alternatives are unviable, he has little reason to consider new information about those alternatives.⁷

Premise 8: When given the chance to enact a selection, an individual only has reason to act if his differentiation remains above his necessary differentiation threshold.

An individual only has reason to act on a selection if there is enough differentiation between the selected alternative and the transformed alternatives (Svenson 1996). If the selected alternative is no better than the transformed alternative, the individual has no reason to pay the cost of action. Following this logic, an individual will not act on his selection if the differentiation level between the selected and the transformed alternatives falls below the individual's threshold (Anderson 2003; Svenson 1996).

2.3 Hypotheses and Examples

The joint logic of these eight premises leads me to the following main hypothesis:

Campaign negativity is more likely to be demobilizing if an individual is exposed to it after selection than if he is exposed to it before selection.

⁷The precise mechanism underlying this assumption may be attention (an individual simply does not pay attention to information about the unselected alternative) or it may be that the individual hears the information but does not internalize it into his thought-process.

In addition to this main hypothesis, my premises also point to a second, clarifying hypothesis:

After selection, it is negativity about the individual's selected candidate that is most likely to demobilize.

The implication of this hypothesis is that negativity is most likely to undermine action when individuals are exposed to it in the post-selection phase. Experimental evidence helps to illustrate this phenomenon. Weinberger (1986) randomly exposed subjects to negative information about a preferred brand of beer. He then asked subjects if they intended to purchase the particular brand of beer, and, more broadly, if they intended to purchase any beer at all. He found that subjects exposed to negative information about the preferred brand of beer were not only less likely to report that they intended to purchase that brand, but also less likely to report that they intended to purchase any beer at all. Negative information about a preferred brand led individuals to be less likely to enact their beer purchasing selections.⁸

This phenomenon will play out similarly in a voting scenario. Let us assume that a voter differentiates between the two candidates for President. He works through differentiation such that the Republican emerges as the dominant alternative. The voter selects the Republican – and following Premise 5, the Democrat transforms into a “non-viable reference point.” Selecting the Republican, however, does not mean that the individual will turn out to vote. If the voter makes a selection in early September of an election year, he must still navigate nearly two months of campaigning. During this time the voter will likely be exposed to negativity about his preferred candidate,

⁸This finding was almost incidental, as Weinberger was searching to find how individuals deal with negative information about well known versus unfamiliar brands. Preference was measured as heavy versus light drinking of Budweiser brand beer. This effect of negativity affecting not only intent to purchase Budweiser, but also other brands, for those who prefer Budweiser, was observed in analyses that covaried experimental effects with preference for the Budweiser brand. In this experiment, Weinberger found effects for most subjects, but he did not find effects for subjects who were heavy beer drinkers. For subjects classified as heavy drinkers, negative information had not effect on either their likelihood of purchasing their preferred beer or any other type of beer.

the Republican. As Premise 7 states, negativity may diminish an individual's level of differentiation. Enough regression in his level of differentiation may lead our voter to believe that the Republican is not much better than the Democrat (his unselected, transformed alternative). Following Premise 8, if the differentiation level between the selected and the transformed alternative shrinks to below his differentiation threshold, the voter will disengage from the decision and not vote at all.

How likely are we to come to a situation where a voter faces a lengthy, negativity-filled post-selection phase? Survey data shows that individuals *do* select preferred candidates early on in the campaign season. Holbrook (1996) shows that on average more than 60% of voters make their selections by October of an election year. The 2000 National Annenberg Election Survey (NAES) confirms Holbrook's finding. During a round of interviews conducted in late September 2000, around 67% of respondents indicated that they had selected a candidate. During a different interview wave, conducted in early October 2000, the percentage of respondents who made selections grew to nearly 75%.⁹ While some individuals wait until the very last moment to make candidate selections, many make selections well before they can act on them.¹⁰ This leaves a gap between selection and action during which negativity can work to diminish differentiation levels and lead individuals not to vote.

⁹Selecteds and unselecteds were determined through a two-question process. First, individuals were asked if they had decided which candidate they supported; then, they were asked if there was still any chance they would vote for the other candidate. Those who said there is still a chance were coded as unselecteds; those who said there is no chance were coded as selecteds.

¹⁰Currently 23 states allow no-excuse early voting. These states are Alaska, Arizona, Arkansas, California, Colorado, Florida, Georgia, Hawaii, Indiana, Iowa, Kansas, Maine, Nebraska, Nevada, New Mexico, North Carolina, North Dakota (only in certain counties), Oklahoma, Tennessee, Texas, Utah, Vermont and West Virginia (information from the National Conference of State Legislatures). Early voting is different from absentee voting in that a voter does not need an excuse to cast an early ballot. In states where early voting is available, however, such ballots are only cast 10-14 days prior to Election Day, thus even if a voter opts for the early voting approach, he may still need to wait before casting a ballot. Moreover, studies of voting patterns show that most early voting happens close to Election Day, rather than far in advance (Gronke 2004). Studies of early voting, however, often conflate early and absentee ballots. As a result it is unclear how many ballots are cast early because individuals want to implement their selections. In addition, it is unclear (outside of states like Oregon which have mainstream early voting policies) how many voters know that they can vote early even if they do not have an excuse for doing so.

2.4 Predictions and Existing Literature

In developing this theory of decision-making and negativity, my eight premises all converge on one key variable: the timing of exposure to negativity. Since I predict that negativity is more likely to demobilize voters after a selection but before an action, it matters *when* in the campaign an individual is exposed to negativity. Timing is something that the existing literature does not consider. More than simply being a missing variable or an additional mechanism, considering timing leads to predictions about the relationship between negativity and turnout that are different from those currently made in the literature. Current literature predicts that the effect of negativity – whether mobilizing, demobilizing or null – will be the same at any point in a campaign. The theory presented here suggests that this is not the case: negativity will only be demobilizing if an individual is exposed to it at a certain time.

The prediction that negativity is most demobilizing *after* individuals have selected a preferred candidate breaks with existing literature in another way. Many scholars of campaign effects argue that campaigns have little effect on individuals once they have selected which candidate they prefer. In *The American Voter*, for example, Campbell et al. (1960) note that nearly 63% of respondents selected their preferred candidate for president at the end of the nominating conventions, leading the scholars to the following conclusion “the psychological forces guiding behavior arise before the campaign opens”(78). Responding to this argument, Holbrook (1996) writes “the fact that 63% of the electorate has already decided how they will vote by the end of the convention was previously presented to suggest minimal campaign effects. The flip side of this, however, is that the remaining 37% constitute a significant portion of the electorate that, if mobilized by a campaign, can play an important role in the outcome”(12). I do not disagree with Holbrook that mobilization of the “undecided” is an important campaign effect. I want to suggest, however, that existing literature discounts the importance of the campaign for individuals who have already chosen a

candidate, but whose behavior may still be influenced. Unlike conventional wisdom, my theory suggests that campaigns may be having the most profound effect on the 63% who have already made selections, except that this effect is not in their vote choice – it is in their likelihood of voter turnout.¹¹

2.5 Conclusions and Empirical Approach

In this chapter I presented eight premises about the decision process. These premises point to a key hypothesis: *negativity is most likely to demobilize voters after they select which candidate they prefer but before they have a chance to act on this selection with a vote.* Following this prediction, I considered how this phenomenon may play out in an election context and, finally, I discussed how the predictions I make differ from the existing literature.

In the remaining chapters of the dissertation, I take the following approach to testing my hypothesis. In each of the next three chapters I test the main hypothesis using a series of independent empirical analyses. Each of these empirical tests offers a different way of observing the effect negativity has after selection but before action.

In addition to my main hypothesis, I also test the mechanism I specify in my premises: differentiation. I test for this mechanism in two ways. First, my premises state that differentiation mediates the relationship between post-selection negativity and turnout. Negativity is more likely to diminish levels of differentiation after selection. In turn, diminishing levels of differentiation decrease the likelihood of turnout. I focus on this mediational effect in my experimental analysis.

Second, I also test for the effect of differentiation by considering the effect of partisanship. In Premise 2 I note that strong partisans are likely to have wider levels of differentiation. If strong partisans do have wider levels of differentiation and if differentiation does mediate the relationship between negativity and turnout, then

¹¹Some of this literature speaks to switching votes, but it speaks to propensity to vote as well

we should observe that negativity has a different effect on likelihood of turnout for stronger partisans. I test for the partisanship effect in all three empirical chapters.

In sum, the next three chapters will first evaluate the post-selection effect of negativity on turnout, and second, consider the mechanism driving this effect.

CHAPTER 3

Negativity and Turnout 1976 to 2000

3.1 Background

Existing work on negative advertising leads to conflicting predictions about the effect of campaign negativity on turnout. Some empirical findings show that negativity is demobilizing (Ansolabehere, Iyengar, Simon and Valentino 1994; Ansolabehere and Iyengar 1995). Other sets of findings show that negativity is mobilizing (Wattenberg and Briens 1999; Goldstein and Freedman 2002). Still another group of findings show that negativity has no effect on turnout (Finkel and Geer 1998; Brooks 2006). Although these scholars reach different conclusions, the work is unified by a common theoretical thread: negativity is perceived to function in the same way at any point in a campaign. I break with existing literature and argue that timing matters. To make this argument, in Chapter 2 I presented a set of eight premises that underlie an individual's decision process. The joint logic of these premises forms the focal hypothesis of this dissertation: *negativity is most likely to demobilize after an individual has made a selection but before he has the chance to implement this selection with a vote*. Moreover, at this crucial time, it is negativity about the individual's selected candidate that will have the strongest effect on his likelihood of turnout.

In this chapter I present an initial test of this hypothesis and consider the effect of negativity over seven presidential elections, 1976 to 2000. In this chapter I will first

replicate a key study of negativity and turnout – Finkel and Geer’s (1998) analysis of the relationship between negativity and turnout over a series of presidential elections. Next, I will extend Finkel and Geer’s work by considering the timing of negativity. I show that once we account for timing, we reach different conclusions about the way negative campaign advertisements affect voter turnout. Finally, I account not only for timing, but candidate selection, and show that it is a precise combination of target and timing that has a significant effect on the likelihood of turnout.

3.2 Finkel and Geer (1998)

As I mention above, I begin my analysis by replicating Finkel and Geer (1998). Finkel and Geer set out to reconsidering findings that show that negativity has a strong demobilizing effect on voter turnout (e.g. Ansolabehere et al. 1994 and Ansolabehere and Iyengar 1995), arguing that “there is little theoretical reason to expect a powerful, systematic relationship between advertising tone and voter turnout” (575). Merging data from the American National Election Study (ANES) with Geer’s database of presidential advertisements, the authors find that over elections 1960 to 1992 negative advertisements have no demobilizing effect on voter turnout. In the next section I will rely on similar data to reconsider Finkel and Geer’s findings and account for the timing of negative advertisements.

3.3 Presidential Elections 1976 to 2000

To analyze the effect of negativity over a series of presidential elections, I follow Finkel and Geer and construct a dataset by merging John Geer’s data with the ANES. Geer’s advertising data is a collection of advertisements aired over the course of a presidential campaign.¹ These advertisements are coded for various characteristics,

¹Geer, John. Presidential TV Ads 1960-2000, www.vanderbilt.edu/psci/geer/downloads.html

including negativity.² While Finkel and Geer look at elections 1960 to 1992, I focus on years 1976 to 2000 as Geer’s dataset has dates³ for advertisements aired during these years.⁴ I merge these advertisements with the ANES by year of survey.

Operationalizing my key hypotheses for a test with survey data offers considerable challenges. First, the ANES does not allow me to obtain a precise measure of selection time.⁵ Second, it is difficult to measure exposure to advertisements in a survey context as it is impossible to account for an individual’s whereabouts at the time of an advertisement’s airing. I deal with the first limitation – inability to measure selection-time – by focusing on the timing of the *negative advertisement* rather than the timing of the individual’s selection. I use additional data sources as well as existing research on electoral choice to consider if an advertisement aired when most individuals in the electorate had not yet made a selection or after most individuals had already made a selection. In the next section I discuss how I created this measure.

The second limitation is the measurement of exposure (Freedman and Goldstein 1999). Though I do account for individuals’ media habits, measurement of exposure remains a limitation in this first test of my hypothesis. To address the limitation of this first analysis I will consider exposure more directly in Chapter 4 using data better suited to measuring exposure. Despite this limitation, however, this first test will provide a useful means of considering the relationship between the timing of

²For a full description of Geer’s data see Appendix 3A.

³Geer’s dates are dates of the creation of the advertisement, rather than its airing. However, according to discussions by Mark (2007) dates of creation and airing are highly similar.

⁴I have not found any evidence that suggests Geer’s coding of the advertisements is systematically different from other scholars who have coded advertisements for negativity. In a comparison of negativity codings over ads aired from 1960 to 1992, Geer shows that his coding is substantively similar to that of Jamieson, Kaid and Benoit (Geer 2006, 37, Figure 2.2). Geer’s coding, however, shows substantively less negativity over these elections than West’s, although that is likely due to sampling differences: West creates a sample of “prominent” ads, and therefore excludes certain ads from his full dataset (38). Nevertheless, in Chapter 4 I also test my hypotheses using data from the Wisconsin Advertising Project. The results in Chapter 4 suggest that it is unlikely that the results presented in this chapter are simply a function of Geer’s coding.

⁵The ANES changed its mode of asking respondents whether or not they have made selections during the years in this analysis. Although the ANES does ask a question about selection time in the post-interview, only respondents who report that they voted in the election were asked this question.

negativity and turnout over a series of elections.

3.3.1 Data and Methods

3.3.1.1 Negative Advertising

In order to consider negativity, I rely on Geer’s characterization of the advertisements in his dataset. Using this data, I code as “negative” all advertisements that contain a negative appeal, even if that appeal is contrasted with a positive appeal. This follows from both my theoretical argument and Geer’s definition: even if an advertisement contains some promotion of the sponsoring candidate, so long as the advertisement also contains a direct criticism of the opposing candidate it is still offering negativity.

Using this measure of negativity, I create two variables. The first I call **Overall Negativity**. This variable measures the total percentage of negative advertisements in a particular campaign.⁶

My second measure is **Late Negativity**. Following my theoretical premises, a late advertisement is aired after an individual has made a selection. Since I do not have a direct measure of selection time, I rely on previous research to estimate when in the campaign process individuals may make their selections. In *The American Voter*, Campbell et al (1960) find that the majority of individuals make selections soon after the party conventions. More recently, in his discussion of campaign effects, Holbrook (1996) writes that over the years 1952 to 1992 by October most voters have already selected which candidate they prefer. Data from the 2000 National Annenberg Election Study (NAES) reinforces this point, showing that by October nearly 80% of respondents have already made selections. Also strengthening this argument are

⁶Finkel and Geer measure this differently. They obtain the percentage of positive advertisements and subtract the percentage of negative advertisements. To test the robustness of my results, I ran an alternate model with this specification; I obtain similar results. Therefore, I use simpler percentage for ease of interpretability and further likelihood estimation.

Hillygus and Jackman's (2003) results which show that in the 2000 election, there was little change in candidate preference after the first presidential debate – which took place on October 3 of that campaign.⁷ Relying on these results, I operationalize a late advertisement as one that is aired in October and November of an election year.

I also use this measure of late negativity to consider if the effect differs by the target (i.e. the candidate criticized) of the ad. Here I use an ANES measure of candidate preference to determine the percentage of late negative ads about the individual's preferred candidate and the percentage of late negative ads about the other candidate. I call this measure **Targeted Negativity**. Here, I again face serious limitations. Nonetheless, while question wording issues prevent me from obtaining a measure of selection time, here I am able to reach a measure of candidate preference – though this measure is far from precise. Specifically, there are numerous differences in question wordings and interviewer instructions over the various ANES surveys. For example, in some years interviewers were instructed to probe respondents who stated that they had not yet selected a preferred candidate, in other years interviewers were instructed to allow respondents to report that they had not yet made a selection. In some years, respondents who stated they were not sure if they would turn out to vote were not asked a selection question. In other years, the selection question is phrased as a preference question. As a result of these differences, I do not have a measure of the selected candidate for every respondent interviewed between 1976 to 2000; moreover, in some cases where I do have a measure it is not precisely a measure of selection (this is particularly true of cases where interviewers were instructed to probe respondents). In short, in a number of cases my measure is more of a “predicted leaning” than a selection, though for ease of discussion I refer to negativity about the selected candidate. As a result, I have a lower N for my estimate of the effect of targeted negativity and less precision in my measure of whether the negativity is about the

⁷The ANES also reinforces the October date. In years where turnout is validated the “selection-time” question shows that most respondents do make selections by October.

respondent’s selected candidate.

3.3.1.2 Controls

To account for other individual-level factors that influence likelihood of turnout, I follow existing literature in specifying a model. I combine the controls used by Finkel and Geer (1998) and Rosenstone and Hansen (1993). I use this combined model for two reasons. While Rosenstone and Hansen specify a number of controls for a model of turnout, they do not account for political interest and media use; on the other hand, while Finkel and Geer do not control for the full range of factors specified in the Rosenstone and Hansen model, they do control for interest and media exposure. Taken together, these two specifications form a useful model with which to consider the effect of negativity (full list of controls is shown in Appendix 3B). To ensure that my findings follow over multiple specifications, I run these analyses with the “pure” Rosenstone and Hansen model, and the “pure” Finkel and Geer model; I obtain similar results each time. These results are shown in Appendix 3C.

3.3.2 Estimation

I estimate three models. The first uses Overall Negativity as an independent variable. The second model breaks negativity into two different variables: the focal Late Negativity variable and a control for early negativity (i.e. negative advertisements aired *before* October 1). The final model considers late negativity by target of the advertisement. In total, I will estimate the following models:

(1) Finkel and Geer 1998: $Turnout = \alpha + \beta(OverallNegativity) + [Controls] + \epsilon$

(2) General Late Negativity: $Turnout = \alpha + \beta(LateNegativity) + [Controls] + \epsilon$

(3) Targeted Late Negativity: $Turnout = \alpha + \beta_1(LateNegativity, PreferredCandidate) + \beta_2(LateNegativity, OtherCandidate) + [Controls] + \epsilon$

3.3.2.1 Broad Effects of Negativity

As I only look at seven elections, I do not have high variation in my measures of negativity. Therefore, I use robust standard errors.⁸

I use the Overall Negativity measure to estimate a model that is similar to Finkel and Geer's (1998) study of campaign negativity. The key differences between my model and Finkel and Geer's are (1) my new model contains a different time period, (2) I include the full set of independent variables from Rosenstone and Hansen's (1993) canonical model of turnout and (3) I use robust standard errors. The substantive results follow Finkel and Geer's original findings: the coefficient on Overall Negativity is positive, meaning that an increase in negativity in an election leads to an *increase* in the likelihood of turnout. This coefficient is not significant. The results are reported in Table 3.1.⁹

Next, I consider the effect of Late Negativity on turnout. Since advertisements coded as "late" aired after most individuals have made selections, I predict that an increase in late negativity should lead to a *decrease* in the likelihood of turnout. This means that the coefficient on Late Negativity should be negative. In this model, I also include a control for early negativity, though I do not make any explicit predictions as to the effect of this form of negativity. I make no prediction as, following my theoretical premises, early negativity had an important effect on selection and my dependent variable here is not selection, but rather implementation (turnout). As shown in Table 3.1, the coefficient on Late Negativity is negative and significant supporting my predictions. The coefficient on early negativity is positive, although it does not reach significance.

⁸The potential problem here is heteroskedasticity. My mode of correcting for this concern is limited as I only have seven elections. For this reason, a common approach like clustering by year could be detrimental for inferences made from estimation (Leoni 2005).

⁹I control for the years 1988 and 2000 because in those years the ANES did not measure particular factors in media exposure; to control for differences in this variable over the years, I include the dummies.

Table 3.1: Full Results, Campaigns 1976 to 2000

Dependent Variable: Turnout				
Variable	Coefficient	(Std. Err.)	Coefficient	(Std. Err.)
Negativity				
Overall Neg.	0.70	(1.09)	—	—
Late Neg.	—	—	-1.866†	(1.05)
Early Neg. Control	—	—	0.163	(0.266)
Resources				
Income	0.738**	(0.140)	0.746**	(0.141)
Education	1.843**	(0.138)	1.800**	(0.140)
Age	0.074**	(0.010)	0.073**	(0.010)
Age Squared*01	-0.005**	(0.001)	-0.005**	(0.001)
External Efficacy	0.480**	(0.080)	0.506**	(0.081)
Internal Efficacy	0.163*	(0.080)	0.164*	(0.080)
Unemployed	-0.101	(0.139)	-0.096	(0.139)
Evaluation of Parties and Candidates				
PID Strength	0.755**	(0.101)	0.762**	(0.101)
Affect For Party	0.315†	(0.190)	0.311†	(0.190)
Care about Outcome	0.476**	(0.069)	0.462**	(0.069)
Affect for Pres.	0.686**	(0.155)	0.686**	(0.155)
Social Involvement				
Years Logged	0.110**	(0.027)	0.110**	(0.027)
Church	0.838**	(0.080)	0.855**	(0.081)
Homeowners	0.512**	(0.071)	0.516**	(0.071)
Working	0.157†	(0.083)	0.156†	(0.084)
Mobilization				
Contacted	0.725**	(0.084)	0.726**	(0.085)
Perceived Closeness	0.188*	(0.073)	0.177*	(0.072)
Actual Closeness	0.260	(0.635)	0.584	(0.846)
Governor's Race	-0.099	(0.089)	-0.098	(0.089)
Compet. of Primary	-0.116*	(0.073)	-0.136†	(0.074)
Other Demographic Variables				
Black	0.101	(0.103)	0.097	(0.103)
Southern	-0.419**	(0.069)	-0.422**	(0.069)
Gender	0.010	(0.065)	0.015	(0.065)
Border States	-0.283*	(0.111)	-0.275*	(0.112)
Hispanic	0.049	(0.147)	0.030	(0.148)
Interest and Exposure				
Media Exposure	0.415**	(0.068)	0.455**	(0.071)
Interest	0.520**	(0.050)	0.513**	(0.050)
Dummy 1988	0.096	(0.096)	0.178†	(0.105)
Dummy 2000	0.080	(0.147)	0.431†	(0.244)
Intercept	-7.125**	(0.620)	-5.933**	(0.608)

† ≤ 0.1 , * ≤ 0.05 , ** ≤ 0.01 , N=8,508, Robust Standard Errors, logit

In both the Overall Negativity and the Late Negativity models the rest of the control variables function much as they do in Rosenstone and Hansen’s (1993) model. We observe that education and income – the two most consistent determinants of turnout – are significant here just as they have been in models past. Moreover, just as Rosenstone and Hansen show, mobilization (i.e. contact from the party) is also significant.

3.3.2.2 Substantive Implications for Turnout Likelihood

While raw coefficients are helpful in determining the general direction of the relationship between negativity and turnout, it is important to consider the substantive effect that Overall Negativity and Late Negativity have on likelihood of turnout. To do so I use *Clarify*.¹⁰ Approaching this next analysis, then, we have the following expectations for the substantive effect of negativity on likelihood of turnout. First, we would expect that increases in Overall Negativity will not have a demobilizing effect on turnout likelihood, while increases in Late Negativity – negativity transmitted at the most crucial time – will cause a significant decrease in likelihood of turnout.

Given the non-linear form of the model, the marginal effect of negativity will differ depending on the values of the independent variables. As a result, here I highlight predicted values at two different partisanship points: strong partisans and weak partisans.

Turning first to Overall Negativity, the substantive change in turnout likelihood as a function of negativity speaks to our prediction. Shifting from a campaign environment where there are no negative campaign advertisements to one where 40% of all advertisements are negative, leads to a 6.6% increase in turnout likelihood at

¹⁰Michael Tomz, Jason Wittenberg, and Gary King (2001). CLARIFY: Software for Interpreting and Presenting Statistical Results. Version 2.0 Cambridge, MA: Harvard University, June 1. <http://gking.harvard.edu>. Clarify draws simulations of estimated parameters and uses these in estimation of predicted values, changes in likelihood, and first differences.

strong partisanship and an increase of 6.7% at weaker partisanship.¹¹ Neither of these changes in likelihood reach statistical significance.¹² These results further reinforce my initial conclusion that negativity over the duration of the entire campaign is not producing substantive changes in turnout likelihood.

Late Negativity, however, does have a demobilizing effect. At weak partisanship, a 40% increase in late negativity – which means that now 40% of all late advertisements shown are negative – leads to a 11.1 % *decrease* in likelihood of turnout.¹³ Moreover, this change in likelihood is statistically significant.¹⁴ At strong partisanship a 40% increase in negativity leads to a 6.7% decrease in turnout likelihood for strong partisans. These results reinforce my key prediction: while the tone of campaigning is important, it is the timing of the message that is crucial. All changes in likelihood are shown in Table 3.2.

These results also hint that there is some difference between strong and weak partisans. These differences follow from my theoretical argument. Recall that in Chapter 2 I theorized that likelihood of turnout decreases due to declining differentiation after selection. Moreover, I also argued that strong partisans likely have wider levels of differentiation than weaker partisans. Putting these two ideas together, identical increases in negativity *after selection* are likely to have a stronger demobilizing effect on weaker partisans, who have lower levels of differentiation to begin with. However, it is not yet clear if these observed differences in likelihood of turnout between strong and weak partisans are due to the functional form of the model, or are signals of the theoretical mechanism I propose. Therefore I will test for the effect of partisanship more explicitly using experiments in Chapter 5.

¹¹Rest of variables set at median or mode for dummies.

¹²With a 90% confidence interval.

¹³Rest of variables set at median or mode for dummies.

¹⁴Significance based on a 90% confidence interval.

Table 3.2: Changes in Turnout Likelihood by Increases in Negativity

	20% Increase in Neg.		40% Increase in Neg.	
	<i>Overall Neg.</i>	<i>Late Neg.</i>	<i>Overall Neg.</i>	<i>Late Neg.</i>
Weak PID	3.2%	-4.7%†	6.6%	-11.1%†
Strong PID	3.5%	-2.7%†	6.7%	-6.5%†

† represents significance at 90% confidence interval(calculated with *Clarify*).

3.3.2.3 Candidate-Specific Effects

We can consider this effect of negativity with even more precision. In particular, as the premises presented in Chapter 2 show, after a selection it is negativity about one’s preferred candidate that will have the most profound effect on likelihood of turnout. To consider this effect, I estimated a model with separate measures of negativity targeting a respondent’s preferred candidate and negativity targeting the other candidate.¹⁵ The results are shown in Table 3.3, and show that, as predicted, only late negativity about the individual’s selected candidate has a statistically significant demobilizing effect. Turning to negativity about the other candidate, while the coefficient is negative, it is not significant.¹⁶

3.3.2.4 Substantive Implications for Turnout Likelihood

I next consider the substantive effects of targeted negativity on likelihood of turnout. Here I follow the same approach as above – using *Clarify* to calculate the predicted likelihood of turnout. I predict that only changes in late negativity about the individual’s preferred candidate will lead to a statistically significant change in his likelihood of turnout. Again, I consider effects for weak and strong partisans separately. To calculate the effect changes in one type of negativity have on likelihood

¹⁵This model also includes two additional dummies to control for the years 1992 and 1996 when the candidate selection question was asked differently from the rest of the years in the data.

¹⁶To rule out alternative explanations, I also consider this test using overall negativity – although such a specification does not at all follow from my theory. Here, the results, much like in the first test shown in Table 3.1, are null: both coefficients are not significant. The coefficient on overall negativity about the preferred candidate is 0.077 and the coefficient on overall negativity about the other candidate is 0.013.

Table 3.3: Targeted Negativity, Campaigns 1976 to 2000

Dependent Variable: Turnout		
Variable	Coefficient	(Std. Err.)
Negativity		
Late Neg.: Preferred Candidate	-1.31 [†]	(0.79)
Late Neg.: Other Candidate	-1.1	(0.78)
Early Neg. Control	-0.241	(0.381)
Resources		
Income	0.821**	(0.199)
Education	1.644**	(0.200)
Age	0.095**	(0.014)
Age Squared*01	-0.008**	(0.001)
External Efficacy	0.256*	(0.115)
Internal Efficacy	0.303**	(0.116)
Unemployed	-0.290	(0.185)
Evaluation of Parties and Candidates		
PIDStrength	0.476**	(0.143)
Affect for Party	0.416	(0.256)
Care about Outcome	0.114	(0.104)
Affect for Pres.	0.586**	(0.205)
Social Involvement		
Years Logged	0.088*	(0.038)
Church	0.911**	(0.117)
Homeowners	0.487**	(0.101)
Working	-0.006	(0.119)
Mobilization		
Contacted	0.774**	(0.122)
Perceived Closeness	0.197*	(0.100)
Governor's Race	-0.296*	(0.120)
Compet. of Primary	-0.072	(0.105)
Other Demographic Variables		
Black	0.063	(0.138)
Southern	-0.445**	(0.097)
Gender	0.042	(0.091)
Border	-0.261 [†]	(0.150)
Hispanic	-0.048	(0.195)
Interest and Exposure		
Media Exposure	0.385**	(0.102)
Interest	0.324**	(0.070)
Dummy 1988	0.048	(0.157)
Dummy 2000	0.257	(0.249)
Additional Dummies		
Dummy 1992	0.792 [†]	(0.406)
Dummy 1996	0.234	(0.246)
Intercept	-4.130**	(0.960)

[†] ≤ 0.1, * ≤ 0.05, ** ≤ 0.01, N=6,222, Robust Standard Errors, logit

of turnout, I set the other type of negativity to 40% (i.e. when I calculate the effect of increases in negativity about the preferred candidate, I set the level of negativity about the other candidate at 40%, and vice versa).¹⁷

In order to show the full effect of different types of negativity, I present the results in Figure 3.1 as changes in likelihood of turnout as a function of an increase in the different types of negativity. In this figure the point represents the total decrease in likelihood of turnout as a function of an increase in negativity (either about your preferred candidate or the other candidate). A point below the 0 line means a decrease in the likelihood of turnout (i.e. the change is negative); conversely, a point above the 0 line would mean an increase in the likelihood of turnout (i.e. the change is positive). The lines around the point represent the confidence interval around this change in likelihood. Where the confidence interval crosses the 0 line, the change in likelihood of turnout is not statistically significant. Figure 3.1(a) and 3.1(b) represents the decrease in the likelihood of turnout as a function of negativity about the selected candidate, while Figure 3.1(c) and 3.1(d) represents the same for negativity about the other candidate. Figures 3.1(a) and 3.1(c) show an increase in from 0% negative ads to 40% negative ads; Figures 3.1(b) and 3.1(d) show an increase from 0% negative ads to 60% negative ads.

As Figure 3.1 shows, more negativity about the selected candidate leads to a decrease in likelihood of turnout (in both, Figures 3.1(a) and 3.1(b) the change in likelihood falls in the negative range). Moreover, these decreases are statistically significant, as the confidence intervals around the change never cross the 0 line. Even more importantly, as predicted, only when there are increases in negativity about the respondent's *selected candidate* are these changes in likelihood statistically significant. While we do observe decreases in likelihood of turnout when there are increases in negativity about the other candidate, these decreases never reach statistical signif-

¹⁷I calculate the change in likelihood of turnout at various other set levels of negativity as well – the results remain substantively similar.

icance. In Figures 3.1(c) and 3.1(d), which depict changes in likelihood of turnout as a function of negativity about the other, unselected, candidate, the confidence intervals always cross the 0 line. In short, as predicted only late negativity about the selected candidate has a significant effect on likelihood of turnout. This finding further reinforces my key argument: negativity is most likely to be demobilizing after an individual has made a selection. This is the case because negativity after selection suggests to the individual that his preferred candidate is no better than the candidate he already does not like.

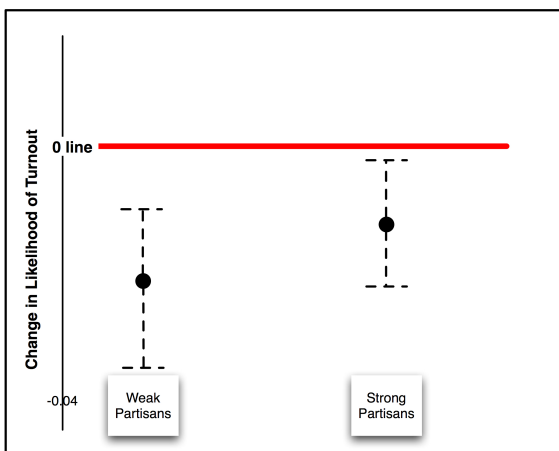
3.3.3 Robustness

The claims above rest on the correct specification of two key points: (1) timing of advertisements and (2) negativity. Although my specification of “late” negativity as negative advertisements shown in October or November of an election year rests on empirical evidence, it is still important to consider the sensitivity of this analysis. To do so, I reconsidered “lateness” as one week before and one week after my cut-off point. The result remains robust even when this time window shifts. The result also holds for changes in measures of negativity volume and I obtain a significant, substantively similar result when I use Finkel and Geer’s difference measure, rather than the basic percent negativity measure.

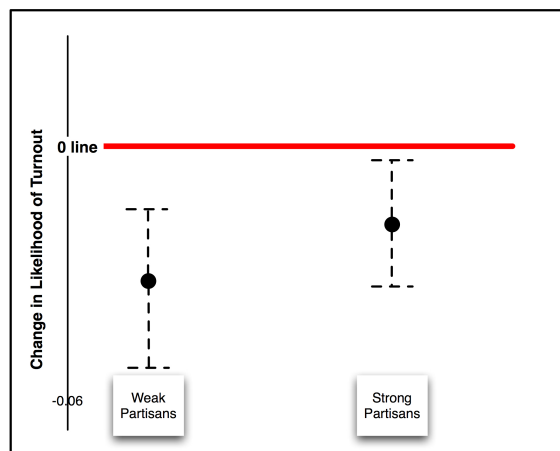
3.4 Conclusions

The results of this first analysis are promising. We see that over elections 1976 to 2000, only negativity aired late in the campaign – after most individuals have made their selections – is demobilizing. This result already hints that timing matters because negativity is most likely to demobilize at a certain time – after selection, but before implementation. Yet this is a blunt test. Therefore, in the next chapter I build on these findings and I expand on my analysis in two ways. First, using panel data

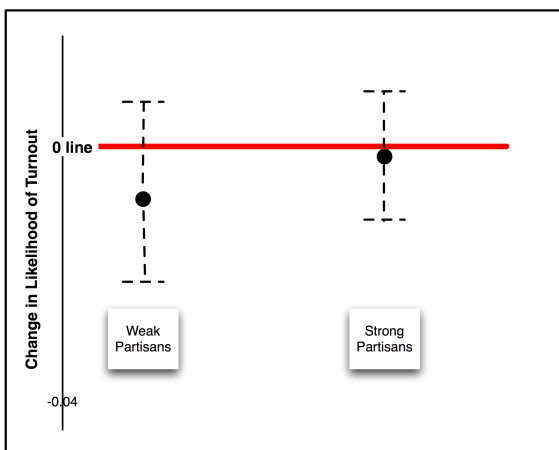
Figure 3.1: Effect of Negativity on Turnout: Selected Candidate Versus Other Candidate



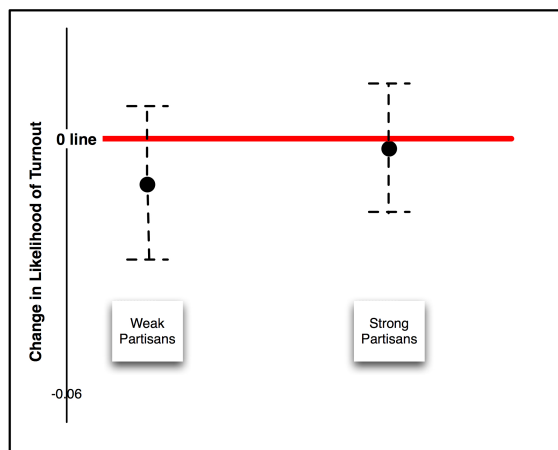
(a) Increase In Late Negativity About Selected Candidate, (increase from 0% to 40%)



(b) Increase In Late Negativity About Selected Candidate, (increase from 0% to 60%)



(c) Increase In Late Negativity About Other Candidate, (increase from 0% to 40%)



(d) Increase In Late Negativity About Other Candidate, (increase from 0% to 60%)

I will directly account for an individual's selection time when estimating the effects of negativity. Second, I turn to a media market- level analysis to consider the role of strategic targeting in the relationship between negativity and turnout.

Appendix 3A

The advertisements in Geer’s dataset come from the Julian P. Kanter Political Commercial Archive at the University of Oklahoma. As Finkel and Geer (1998) write “The archive does not claim to have every political advertisement run since 1960, but the founder of the archive, Julian Kanter, believes to have close to a complete set from most elections and all the ads in some of them” (579, Footnote 1). In addition to Geer, Kaid and Johnston (1998) and West (1993, 1997) also make use of this archive.

A full appendix describing Geer’s dataset is available in Geer (1998) or here: <http://www.vanderbilt.edu/psci/johngeer/Downloads>. Below I include some notable points. Geer’s dataset includes a content analysis of all advertisements in the archive that are 60 seconds long or shorter. Geer (1998) explains his decision as follows:

In the content analysis, I limited attention to ads that were 60 seconds or shorter. I chose to focus on these shorter ads, since they constitute the bulk of the contenders’ advertising campaign. At the Archive, usually 90 percent or more of all ads are one minute or less in duration. This focus should allow me to capture the major campaign themes of the nominees. Moreover, in recent years, there has been a greater emphasis on the shorter spot, making this choice even more reasonable (Appendix).

In addition, Geer’s dataset only includes advertisements that were aired (versus advertisements that were created, but were not broadcast). Geer’s dataset also excludes advertisements that are what he calls “near-duplicates” of previous ads. Finally, Geer’s dataset excludes all advertisements that target small, specific sections of the country, but does include advertisements that have specific targets but that target large geographic swaths. Finally, Geer codes only advertisements aired by the campaigns, a decision he explains as follows:

Finally, I limited my attention to spots authorized by the candidate's campaign committee. Occasionally, a group aired an ad either attacking or supporting a particular candidates. But since I am concerned with how candidates compete with each other, these ads fell outside of the bounds of this project. Given this rule, perhaps the most famous ad of recent times—the Willie Horton spot—is not included in the analysis. One might worry about the absence of such ads. But for the most part, this was not a common occurrence. And in the particular case of Bush's ads, the themes of the Willie Horton ad appeared in other spots authorized by the Vice President's campaign, making this omission not as troubling as one might first think (Appendix).

Geer's content analysis is focused on the language used in the ads; the ads are not coded with respect to visual images and sounds. Although this approach is somewhat limiting, it follows from the boundaries of the definition of negativity used in this dissertation.

Appendix 3B

Rosenstone and Hansen (1993) variables:

Data is the American National Election Study Cumulative File, 1948 to 2004 (ICPSR study #8475). All variables that are included in the Rosenstone and Hansen model of voter turnout are coded in accordance with the directions provided in Rosenstone and Hansen (1993) Appendix B (257-265):

- Income: Individual income taken directly from ANES, categorical
- Education: Level of education taken directly from ANES, categorical
- Age: Age taken from ANES, continuous
- Age Squared*01: Age squared and multiplied by 0.1, continuous
- External Efficacy: Measured using ANES questions on government responsiveness, categorical
- Internal Efficacy: Measured using ANES questions on individual political competence, categorical
- Strength of PID: Seven point partisanship scale, categorical
- Affect for Partisanship: Created using thermometer scores for parties, continuous
- Care about Outcome: Taken from ANES question asking whether a respondent cares about the election outcome, binary
- Affect for President: Difference of thermometer scores, continuous
- Years logged: Natural log of years a respondent has lived in a community, continuous

- Church: How often does the respondent attend church, categorical
- Homeowners: Does the respondent own a home, binary, 1 if homeowner, 0 otherwise.
- Contacted: Was the respondent contacted by any campaign, binary, 1 if contacted, 0 otherwise.
- Perceived Closeness: Does the respondent perceive the election to be close, categorical
- Black: Is the respondent black, binary
- Southern: Does the respondent live in a state classified as the “Solid South” by the ANES, binary, 1 if a state can be classified as south; 0 otherwise.
- Working: Is the respondent currently employed, binary, 1 if working, 0 otherwise.
- Unemployed: Is the respondent currently unemployed, binary, 1 if unemployed, 0 otherwise.
- Gender: Respondent’s gender, binary, 1 if male, 0 if female.
- Governor Race: Is there a governor’s race in the respondent’s state, binary, 1 if governor’s race, 0 otherwise.
- Border States: Does the respondent live in a state classified as a “Border State” by the ANES, binary
- Hispanic: Is the respondent hispanic, binary
- Competitive Primary: Was there a competitive primary in the state, taken from the Almanac of States, binary, 1 if both major parties had at least two candidates on the ballot in a primary, 0 otherwise.

- Actual Closeness: How close was the race according to the last Gallup Poll before the election, continuous

Variables included in Finkel and Geer (1998), but not included in Rosenstone and Hansen (1993)

- Media: Index of respondent's media use including television (except 1988), radio, magazines (except 2000) and newspapers, categorical
- Interest: How interested is the respondent in the election, categorical
- Year Control: Because the media use index was constructed differently for 1988 and 2000 due to a difference in the questions included by the ANES during those two years, I include a dummy for 1988 and a dummy for 2000.

Appendix 3C

Table B1: “pure” Rosenstone and Hansen model, *excludes the media and interest variables*. Table B2: “pure” Finkel and Geer model, *excludes the governor, competitiveness of primaries, hispanic, border states, south, perceived and actual closeness, and affect variables*.

B1: Rosenstone and Hansen Model with No Media or Interest Controls

Variable	Coefficient	(Std. Err.)
Late Negativity	-1.220*	(0.511)
Income	0.884**	(0.136)
Education	1.991**	(0.129)
Age	0.079**	(0.010)
Age Squared*01	-0.006**	(0.001)
External Efficacy	0.629**	(0.077)
Internal Efficacy	0.269**	(0.079)
PID Strength	0.786**	(0.099)
Affect for PID	0.486**	(0.182)
Care	0.667**	(0.065)
Affect for PRES	1.044**	(0.149)
lnYears	0.100**	(0.026)
Church	0.918**	(0.079)
Homeowners	0.489**	(0.069)
Contacted	0.778**	(0.083)
Perceive Closeness	0.198**	(0.071)
Black	0.113	(0.097)
Southern	-0.400**	(0.067)
Working	0.127	(0.082)
Unemployed	-0.072	(0.135)
Gender	-0.062	(0.064)
Actual Closeness	-0.261	(0.593)
Governor Race	-0.101	(0.085)
Competitiveness of Primaries	-0.042	(0.066)
Border States	-0.296**	(0.108)
Hispanic	-0.017	(0.210)
Intercept	-4.633**	(0.396)

Table B2: Finkel and Geer Model, State-based Controls Excluded

Variable	Coefficient	(Std. Err.)
Late Negativity	-1.371 [†]	(0.793)
Income	1.113**	(0.115)
Education	1.778**	(0.115)
Age	0.073**	(0.009)
Age Squared*01	-0.005**	(0.001)
External	0.517**	(0.071)
PID Strength	0.869**	(0.088)
Care	0.533**	(0.060)
Church	0.877**	(0.071)
Contacted	0.797**	(0.076)
Perceived Closeness	0.169**	(0.063)
Black	0.003	(0.086)
Southern	-0.383**	(0.060)
Gender	-0.057	(0.057)
Interest	0.568**	(0.043)
Media	0.428**	(0.058)
Dummy 1988	0.155	(0.096)
Dummy 2000	0.318*	(0.160)
Intercept	-5.598**	(0.482)

CHAPTER 4

Negativity and Selection in 2000

4.1 Introduction

The results presented in Chapter 3 provide an overview of the relationship between negativity and turnout and, most importantly, suggest that the timing of negativity matters. This first analysis paints the effect of negativity in broad strokes; therefore in this chapter my goal is to look more closely at the details. I will do so through a focus on a single election: the 2000 presidential race. I consider the 2000 election through a series of analyses. First, I will use individual-level data to directly engage an individual's decision process to show that negativity is most likely to demobilize those who have already made selections. Second, I will use aggregate data to account for strategic targeting of negative advertisements to particular media markets.

The rest of this chapter is organized as follows. First, I focus on individual-level data. In this section, I present the results of an analysis that accounts for the general effect of negativity after selection, as well as the results of analyses that consider the effect of post-selection negativity about specific candidates. Second, I turn to my aggregate analysis. Here I discuss how I constructed the dataset used in this analysis and focus on the estimation of strategic targeting of negativity. Finally, I offer some conclusions and provide an overview of the next chapter.

4.2 The 2000 Campaign: Individual-level Analyses

4.2.1 Data

To consider the relationship between selection, negativity and voter turnout at the individual level, I turn to two sources of data. Data about individual characteristics come from the National Annenberg Election Study (NAES). The NAES is a study commissioned by the Annenberg School for Communication and the Annenberg Public Policy Center of the University of Pennsylvania (Romer et al. 2004). Here I use the NAES Multiple Reinterview Panel B. This is a small NAES panel where respondents were first interviewed during an interview period starting in September 21, 2000 and then subsequently reinterviewed after the election.¹

Data about advertisements aired during the 2000 campaign come from the Wisconsin Advertising Project (Goldstein, Franz and Ridout 2002).² This dataset was built using advertising tracking data provided by the Campaign Media Advertising Group (CMAG) and coded for content at the University of Wisconsin. In total, CMAG tracked the top 75 media markets in America in 2000.³ The data provide information about the date the advertisement aired, the media market in which the advertisement aired, as well as the number of times each advertisement aired. These advertising data, to which I refer as “Wisconsin data,” provide a helpful means of considering exposure, as they track each time an advertisement was aired – something

¹A subset of these respondents were also interviewed before and after the October debates. To ensure that these respondents are not systematically different from those who were not interviewed for the debate sample, I estimate the model with a control included for the debate sample; the results do not show any significant difference.

²Use of the Wisconsin Advertising Project data requires the following acknowledgement: “The data were obtained from a joint project of the Brennan Center for Justice at New York University School of Law and Professor Kenneth Goldstein of the University of Wisconsin-Madison, and includes media tracking data from the Campaign Media Analysis Group in Washington, D.C. The Brennan Center-Wisconsin project was sponsored by a grant from The Pew Charitable Trusts. The opinions expressed in this article are those of the author(s) and do not necessarily reflect the views of the Brennan Center, Professor Goldstein, or The Pew Charitable Trusts.”

³About 78% of the population lives in the top 75 media markets (Krasno and Green 2008). This includes only the 48 continental states.

the Geer data I used Chapter 3 does not do. I merge the NAES data with the Wisconsin data using the respondent's media market and date of interview; this allows me to pinpoint just how many negative advertisements were aired after a respondent's interview in *the respondent's media market*.

Relying on this data allows me to consider two factors that I could not consider in Chapter 3. First, I can establish whether or not a respondent had made a selection at the time of interview. Second, due to the geographic information in the Wisconsin data I can estimate a respondent's potential exposure to negativity with greater precision.

4.2.2 Models

I use the combination of NAES and Wisconsin data to test my general prediction that negativity is most likely to be demobilizing after an individual has made a selection but before he has implemented that selection with a vote. This prediction means that the power of negativity to demobilize depends on whether or not a respondent has made a selection. I specify this relationship as an interaction between an individual's selection state and the percentage of negative advertisements aired after the initial NAES interview as follows:

(Model 1) Turnout = $f(\alpha + \beta_1\text{Selection} + \beta_2\text{Negativity} + \beta_3\text{Selection} \times \text{Negativity} + \text{Controls})$

In estimating this model it is important to consider that candidates may rely on different campaigning techniques in different states. Although I do try account for these differences using control variables (as I will discuss in greater detail later in this chapter), there may be other state-level differences that I cannot account for with controls alone. To address the possibility that errors may be correlated by state, I cluster by state. As there is some debate about the costs and benefits of using

clustering (Graubard and Korn 1994), in addition to the clustered model, I also use a random effects probit. Both models lead to the same substantive conclusions.

I also use a similar approach to consider whether the effects of post-selection negativity differ by candidate. Specifically, I consider whether negativity about the candidate the individual selected is more likely to be demobilizing than negativity about the candidate the individual did not select. This leads to two additional models. In the first model, I leverage the measure of selection in the NAES (which I discuss in the next section) to analyse the differing effect of negativity by target using my entire sample:

(Model 2) $\text{Turnout} = f(\alpha + \beta_1 \text{Selection} + \beta_2 \text{Neg.: Preferred Candidate} + \beta_3 \text{Selection} \times \text{Neg.: Preferred Candidate} + \beta_4 \text{Neg.: Non-Preferred Candidate} + \beta_5 \text{Selection} \times \text{Neg.: Non-Preferred Candidate} + \text{Controls})$

In the third model, I take a simpler approach and consider the effects of targeted negativity only for those individuals who have already made selections:

(Model 3) $\text{Turnout} = f(\alpha + \beta_1 \text{Neg.: Selected Candidate} + \beta_2 \text{Neg.: Unselected Candidate} + \text{Controls})$

Having established the three basic models and estimation method, I next turn to my key variables. In the next sections I discuss my main independent variables and the controls used in this model. Later, I will also discuss the limitations of this data and how these limitations affect my estimation.

4.2.3 Independent Variables

4.2.3.1 Selection

I measure selection using two questions that are ideal to my purposes. The first question asks respondents which presidential candidate they intend to vote for in the general election, Bush or Gore. Then, respondents were asked a follow-up question: is any chance that they will vote for the other candidate? In other words, those who said they intend to vote for Bush were asked if there is any chance they may still vote for Gore, and vice versa. I code respondents who say there is *no chance* that they will vote for the other candidate as having made a selection and I code those who say there is still a chance that they may vote for the other candidate as having not yet made a selection. For ease of discussion, from this point on I will refer to respondents who have made a selection as the “selecteds” and those who have not yet made a selection as “unselecteds.”⁴

4.2.3.2 Advertising

In this analysis I once again follow Geer’s definition of negativity that classifies any critique by a candidate of his opponent as negativity. Relying on this definition, I create a measure of general post-selection negativity by calculating what percentage of ads aired after a respondent’s first interview were negative.⁵ I also calculate the percentage of negative ads aired before the respondent’s interview, to use as a control.⁶

To create a measure of targeted post-selection negativity, I use the first stage of the selection question detailed in the section above. For respondents who stated they preferred Gore, negativity about Gore is used as negativity about the selected candidate; for respondents who stated they preferred Bush, negativity about Bush is

⁴Individuals who did not provide an answer to the first or second question or reported selecting a third-party candidate were excluded from the analysis.

⁵Percentage based on total number of ads aired after the first interview, rather than total number of ads aired over the entire course of the campaign

⁶Respondents whose media markets were not tracked by CMAG were excluded from the analysis.

the measure of negativity about the selected candidate. Conversely, for respondents who preferred Gore, negativity about Bush serves as the measure of negativity about the unselected candidate; for respondents who prefer Bush, negativity about Gore is negativity about the unselected candidate. As I use only the first stage of the two-stage selection question to calculate this measure, I have a measure of targeting for both selecteds and unselecteds; I leverage this information in model 2.

4.2.3.3 Additional Controls

In addition to selection, negativity and the interaction term (selection \times negativity), I also include the traditional determinants of turnout included in Rosenstone and Hansen's (1993) model of turnout. These variables include a set of demographic characteristics (age, race, education, income, gender, employment status, church attendance and years at current residence), political characteristics (strength of partisanship, care about electoral outcome, affect for presidential candidates and efficacy), mobilization variables (direct contact from campaigns and indirect contact from campaigns) and measures of interest (political interest, media habits).⁷

In addition to these traditional control variables, I also control for three sets of factors that are unique to the particular task at hand: (1) state competitiveness, (2) exposure to advertising and (3) volume of advertising.⁸

My measure of competitiveness comes from Shaw (2006). Shaw's algorithm of

⁷These final variables do not come from Rosenstone and Hansen's model, but they are used by Finkel and Geer (1998), as well as numerous other scholars who have considered turnout and other forms of participation. All control variables are detailed in full in Appendix 4A.

⁸Another control is methodological. A clustered approach assumes that the predictors included in the model and the group-level variation is not correlated. This is not an assumption I can make in my model. I use random effects to deal with excluded state-level variation, and it is possible (and, indeed, likely) that these excluded state-level factors do correlate with other predictors in this model. This is a concern, as Bafumi and Gelman (2006) show that using a clustered or random effects approach in cases where predictors correlate with excluded group-level factors may lead to biased estimates that seem more precise than they actually are. Bafumi and Gelman (2006) provide a simple fix for this problem: including a group-level mean for predictors that correlate with group-level effects. Therefore, in my model I will include means of predictor that correlates most strongly with my state-level units: volume of advertising.

competitiveness is built using interviews with operatives from both the Gore and Bush campaigns and details whether each of the campaigns classified a particular state as a battleground, a “possible leaner,” solidly Republican or Solidly Democrat. I adapt this algorithm as follows: states that both campaigns classified as battleground are coded as 1; states that one of the campaigns classifies as battleground are coded as 0.75; states that both campaigns classified “possible leaners” are coded as 0.5; states that one of the campaigns classified as “possible leaners” are coded as 0.25; states that both campaigns agree are solidly one party or the other are coded as 0.⁹ Here, we would assume that states that are coded as 1 are receiving the most campaign attention and states that are coded as 0 are receiving the least. Shaw (2006, Table 4.4) suggests that such a distribution of campaign attention is indeed the case.

I also include measures of exposure. One of the most precise measures of exposure to campaign advertising comes from Freedman and Goldstein (1999) who write that exposure is best estimated using two factors: (1) the frequency of advertisement airings and (2) an individual’s television viewing habits. While I can implement the first part of this approach – the frequency of advertisement airings – I cannot measure the individual’s viewing habits with as much precision as Freedman and Goldstein (1999). Instead, I attempt to account for individual viewing habits in two ways. First, I include a measure of television habits separate from the general media exposure measure. While the media exposure measure in this case speaks to general interest in politics (and the specific elections), the measure of television habits plays a more direct role in exposure. Here, I create a measure of whether or not the respondent watched cable television, network television or local television within the last week.

A second exposure measure is the individual’s level of political knowledge. I use this measure following Zaller’s (1992) argument that a measure of political knowledge (which he terms “political awareness”) “is assumed to capture propensity for reception

⁹My results are robust to different specifications of this variable, including a dummy specification where 1 = solidly battleground and 0 = otherwise.

of political communications generally, regardless of their point of origin” (44). While I agree with Freedman and Goldstein (1999) that this type of awareness measure is “less valid when applied to the specific case of television advertising,” this measure does offer some additional value by considering a different dimension of exposure (1196). Following Zaller, individuals who are more politically knowledgeable may be more likely to be exposed to television news and other political information. As a result, negativity may have a different effect on these types of individuals. Therefore, in estimating my model it is important to control not only for basic exposure to television, but also the more deliberate political exposure that is measured through a respondent’s knowledge.

An additional set of controls included here is the volume of negativity. While the percentage of negativity speaks to the relative proportion of advertisements classified as negative, this measure does not account for the total number of advertisements that an individual could be exposed to during a particular set of dates. To control for the volume of advertising, I follow Brader and Corrigan (2006) and include the total number of advertisements shown. I include this measure in the same manner as I include the percentage of negativity: the total number of advertisements is matched to a particular respondent’s media market and particular dates of interview.

4.2.4 Limitations

Although the NAES provide me with a means of measuring selection time, there is a limitation to relying on this survey. This limitation is the measure of turnout. In particular, the majority of the respondents interviewed through the NAES report that they will turn out and vote, producing a turnout rate that is significantly higher than the turnout rate of any recent American presidential election. Although it is possible that nearly 80% of NAES respondents turn out to vote, it is also likely that the current turnout measure does not do enough to control the social desirability

issues involved in measuring turnout. Given these measurement issues, it is likely that it will be more difficult for me to observe the effect negativity has on turnout.

4.2.5 Model 1: General Negativity

In estimating my first, and most general, model I will follow an approach similar to one presented in the previous chapter. First, I will estimate the model and present raw coefficients. Second, I will focus on the substantive results. Here, I follow the predictions made in the Chapter 2 and consider how and why negativity may work differently depending on an individual's strength of partisanship.

Before turning to my results, I briefly consider what we would expect to observe in estimating this model. Following my main hypothesis, I predict that negativity will have a demobilizing effect at a specific time: after an individual has made a selection, but before he has implemented that selection with a vote. Translating this to the analysis at hand, we should observe negativity have a more demobilizing effect on selecteds than on unselecteds. In my model we would observe this effect through the interaction between negativity and selection. In particular, we should observe that selection has a negative marginal effect on likelihood of turnout. Furthermore, if my prediction holds we should observe that for selecteds the likelihood of turnout decreases as negativity increases.

I do not make a specific prediction for unselecteds, though it is possible that here we may observe a null effect. I make no predictions due to the way my theoretical premises interact with the structure of my data. My theory predicts that negativity in the pre-selection stage may help individuals make selections. Since making a selection makes an individual much more likely to act, negativity for unselecteds may increase their likelihood of turnout. However, my data only captures individuals at a single point in time – their interview. As a result, it is possible that some unselecteds will make selections after their NAES interviews but prior to Election

Day, meaning that these unselected respondents will still receive negativity after selection. Depending on the timing of selection, then, and the amount of negativity, the potentially mobilizing pre-selection effect of negativity and the demobilizing post-selection effect of negativity could balance each other out, leaving a null effect. As I cannot predict or capture what happens to an unselected respondent's selection state after the interview, I make no direct prediction here.

4.2.5.1 Results

The results of estimation are presented in Table 4.1. Estimating this model shows that the coefficient on the interaction between selection and negativity is negative and significant. This result already offers some support for my prediction that the effect of negativity depends on the timing of exposure – or, more directly, whether or not an individual has made a selection. Next, the coefficient on selection is positive and significant – showing that when there is no negativity at all, making a selection increases likelihood of turnout. This follows theoretically. If there is no incoming negativity to deter an individual, selecting one of the alternatives makes him more likely to act than someone who has not made a selection at all. The coefficient on negativity, on the other hand, does not reach significance, meaning that when an individual has not yet made a selection, negativity does not have any significant effect on turnout. This is also to be expected given that respondents in the unselected group likely made selections at various points after the initial interview, thus muting the overall effect of negativity.

The rest of the controls act in a manner similar to previous studies of turnout. In particular, education and income – two variables systematically shown to be key determinants of turnout – are significant. Also significant is whether or not an individual cares about the outcome of the election and how many years an individual has lived in his home. This follows from previous studies of turnout (see, for example,

Table 4.1: Effect of Negativity and Selection on Turnout in the 2000 Campaign

Variable	Coefficient	(Std. Err.)
Negativity and Selection		
Selection × Negativity	-2.27 *	(0.925)
Selection	0.423 [†]	(0.248)
Negativity After Interview	-1.702	(1.596)
Early Negativity	-0.003	(0.004)
Resources		
Income	1.180**	(0.423)
Education	1.642**	(0.607)
Age	-0.002	(0.009)
Internal Efficacy	-0.211	(0.129)
External Efficacy	0.013	(0.100)
Employed	0.054	(0.258)
Evaluation of Parties and Candidates		
Strength of Partisanship	0.540	(0.364)
Affect for Candidate	0.003	(0.004)
Care about outcome	-0.725**	(0.256)
Social Involvement		
Years	0.050**	(0.011)
Church	0.079	(0.110)
Mobilization		
Contact	-0.060	(0.288)
Other Mobilization	-0.195	(0.348)
State Competitiveness	0.272	(0.321)
Interest and Exposure		
Interest	-0.177	(0.137)
All Media	0.107	(0.196)
Television Habits	0.042	(0.043)
Knowledge	-0.150	(0.129)
Ad Volume	0.014**	(0.004)
Other Demographics		
Black	0.137	(0.420)
Hispanic	0.051	(0.866)
Gender	0.161	(0.219)
Cluster Control and Intercept		
Cluster Control	-0.003	(0.004)
Intercept	-0.116	(0.890)

[†] ≤ 0.1, * ≤ 0.05, ** ≤ 0.01, N=285, Robust Standard Errors, logit

Rosenstone and Hansen 1993). Unlike in previous work, mobilization variables fail to reach significance, although this may be in part due to differences in question wording between previous studies and the NAES.

In the end, however, the results in Table 4.1, however, do not yet offer a complete portrait of the relationship between selection, negativity and turnout. First, the fact that the coefficient on the interaction term is significant is not enough to justify its inclusion in the model. As Brambor et al. (2006) write:

It is perfectly possible for the marginal effect of X on Y to be significant for substantively relevant values of the modifying variable Z even if the coefficient on the interaction term is insignificant. [This means] that one cannot determine whether a model should include an interaction term simply by looking at the significance of the coefficient on the interaction term. (47)

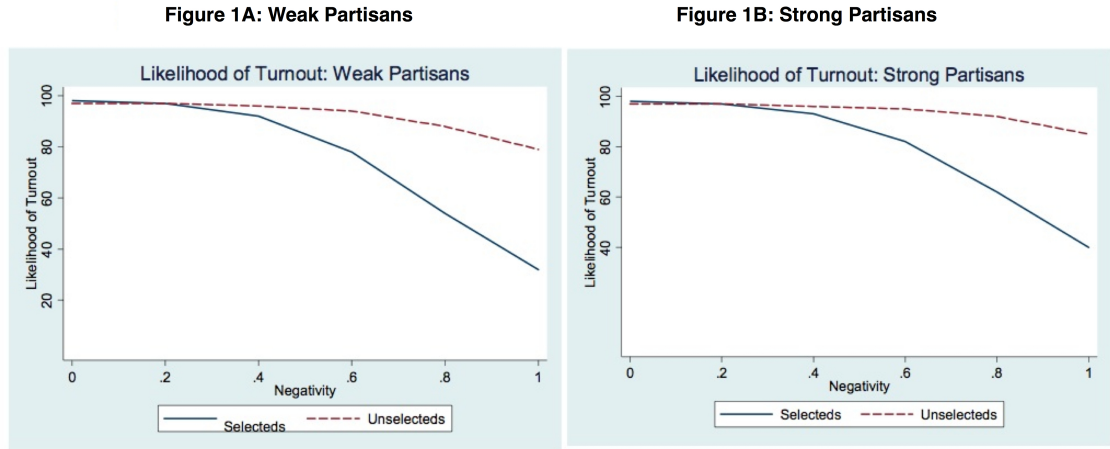
Second, these results do not present the substantive effect making a selection has on the relationship between negativity and turnout. Therefore, I re-consider my results as follows: first, I will plot the predicted likelihood of turnout, and second I will do the same with the marginal effect of selection at varying levels of negativity.

4.2.5.2 Predicted Probabilities

Plotting predicted probabilities will provide a simple way of observing how negativity affects voter turnout. If my predictions hold, as negativity increases we should observe a decreasing likelihood of turnout for selecteds. What's more, we would expect that these changes in likelihood of turnout will be significant. In a plot of predicted probabilities we would observe this as a negatively sloped line. Finally, we would expect that the selecteds are consistently different from the unselecteds.

The plotted predicted probabilities are shown in Figures 4.1(a) (weak partisans) and 4.1(b) (strong partisans). Looking at these figures we can observe that as the level of negativity increases there is a steeper drop in likelihood of turnout for selecteds

Figure 4.1: Predicted Probabilities of Turnout



than for unselecteds. To the extent that the slope for unselecteds is still somewhat negative, the changes in likelihood of turnout that result from increases in negativity are substantively small and not significant. For example, for weaker partisans who have made selections, a shift from an environment where 40% of all advertisements are negative to one where 60% of all advertisements are negative will *decrease* likelihood of turnout by 14%. The same shift in negativity will decrease turnout likelihood by 11% for stronger partisans who have made selections. In each case, the change in likelihood is significant.¹⁰ On the other hand, the same change in negativity will lead to a 2% decrease in likelihood of turnout for unselecteds with weak partisanship and a 1% decreases in likelihood of turnout for unselecteds who are strong partisans. For unselecteds, these changes in likelihood are not significant.¹¹

4.2.5.3 Marginal Effect of Selection

In addition to being a necessary dimension of any interactive analysis (Brambor et al. 2006; Kam and Franzese 2007), calculating the marginal effect of making a

¹⁰The confidence interval here was 90%.

¹¹Predicted probabilities were calculated using Clarify.

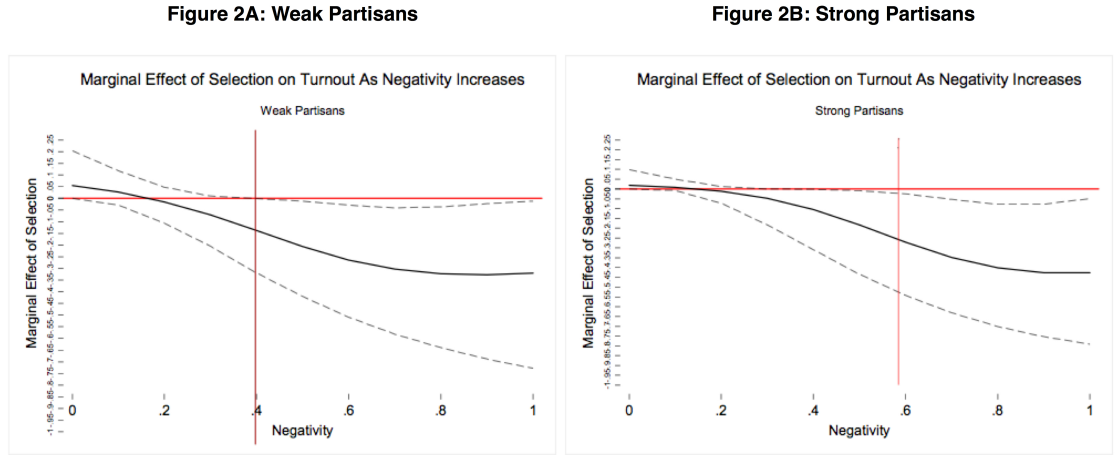
selection has an added benefit here. We already know that when there is no negativity at all, the act of making a selection will have a positive effect on likelihood of turnout. This leads to a question: how much negativity must be transmitted after a selection to demobilize a voter? This is something that can be observed through a plot which traces the relationship between selection and turnout at various levels of negativity. In particular, here we will likely see the effect of selection be distinguishable from zero at some levels of negativity and indistinguishable from zero at others. Moreover, earlier I theorized that individuals with wider levels of differentiation are better able to withstand negativity and that strong partisans likely have wider levels of differentiation than weaker partisans. If this is correct, we should observe differences between strong partisans (wider levels of differentiation) and weaker partisans (smaller levels of differentiation). Here it should take more negativity to demobilize a strong partisan, than a weak one.

Looking at the marginal effects plots – Figure 4.2(a) and 4.2(b) – we observe that, conditional on negativity, the effect of making a selection on turnout is negative. Put another way, as negativity increases, moving from being an “unselected” respondent to a “selected” respondent makes an individual less likely to vote. What is also interesting is how much negativity is necessary to dissuade a voter from acting out a selection. For weaker partisans (4.2(a)), the effect of making a selection was statistically distinguishable from zero when 40% or more of all advertisements shown were negative. For stronger partisans (4.2(b)), as expected, more negativity was necessary: here the effect of making a selection was statistically distinguishable from zero when 60% or more of all advertisements shown were negative.¹² Neither 40% negativity, nor 60% negativity, are unrealistic thresholds. In the 2000 election, advertising in a number of media markets – particularly those which included competitive states – crossed both the 40% and 60% marks.¹³

¹²The confidence interval here is set at 90%.

¹³Marginal effects plots were created following Brambor, Clark and Golder

Figure 4.2: Marginal Effect of Selection



In sum, these results show that negativity is demobilizing at a certain time: after selection but before implementation. While we saw traces of this effect in the analysis of presidential campaigns 1976 to 2000, focusing on the selecteds as a group allows for a clearer observation of the relationship between negativity, selection and turnout.

4.2.5.4 Alternative Explanations

Before moving any further, I look at qualitative differences between selecteds and unselecteds that may make one group more likely to be exposed to advertising or more likely to turn out and vote. My concern here is the alternative explanation that there are some differences between these two groups that makes one more susceptible to negativity, and that these differences have nothing to do with selection. Despite the fact that there are more selecteds than unselecteds¹⁴, there are few other differences between these two groups. The groups appear equally likely to follow national and local news, are equally likely to have been contacted by campaigns and have nearly

(2006); base-code for analysis is courtesy of Golder and is available here: <http://homepages.nyu.edu/~mrg217/interaction.html>

¹⁴This is likely a function of interview timing; at earlier waves of the NAES, these two groups are more equal in size.

equal levels of knowledge. The place where the two groups differ is in partisanship strength (the selecteds are more partisan) and in income (the selecteds have higher incomes). If anything, though, these differences should make the selecteds more likely to turn out and vote (partisanship strength, education), or at least more likely to say they have voted (higher income). The partisan difference in particular should lower the likelihood of obtaining a positive result on my prediction – existing literature suggests that strong partisans are less susceptible to campaigning in general.

Moreover, the distribution of respondents is generally equivalent among competitive and non-competitive states. So, it is *not* the case that decideds are living in battleground states and unselecteds are living in partisan base states (or vice versa). These comparisons are shown in Appendix 4B.

I also consider the alternative explanation that an individual's selection state at the first interview is a function of the negativity that happened *prior* to the interview. I estimated a model with the individual's decision state as a dependent variable; among the explanatory factors, I used the percentage of negativity aired prior to the NAES interview. The results show that negativity prior to the NAES interview is not a significant determinant of an individual's selection state.

4.2.6 Models 2 and 3: Negativity by Target

My next step is to consider whether the effect of post-selection negativity differs by candidate. Following the premises in Chapter 2, I predict that negativity about the individual's selected candidate is more likely to demobilize than negativity about the candidate the individual did not select. In this section I rely on two different approaches to consider how the effect of post-selection negativity differs by target.

4.2.6.1 Model 2: All Respondents

In the first of my targeted negativity analyses, I estimate negativity about the selected and unselected candidates using the first stage of the selection question. This allows me to include all respondents in this analysis – even respondents who have not made selections. Since this means that my sample includes individuals who have not made selections, in discussing Model 2 I refer to the candidate specified at the first stage of the selection question as the “Preferred” candidate; I call the candidate *not* specified in this first stage the “Non-Preferred” candidate. Before moving on to my results, I will briefly review model 2 and my expectations:

(Model 2) Turnout = $f(\alpha + \beta_1 \text{Selection} + \beta_2 \text{Neg.: Preferred Candidate} + \beta_3 \text{Selection} \times \text{Neg.: Preferred Candidate} + \beta_4 \text{Neg.: Non-Preferred Candidate} + \beta_5 \text{Selection} \times \text{Neg.: Non-Preferred Candidate} + \text{Controls})$

In the above model, the interaction between selection and negativity about the preferred candidate will show the way post-selection negativity about the selected candidate will affect turnout. Since I expect that negativity will be demobilizing after selection only when it is about the candidate the individual selected, we should observe a significant negative coefficient on β_3 . My theory does not generate direct predictions about the other coefficients.

The estimation results from model 2 are shown in Table 4.2. First, we see that as predicted of the two interactions only the interaction between selection state and *negativity about the selected candidate* is significant and negative. The interaction between selection and negativity about the other candidate is not only positive, but is also not significant. Of all the coefficients in this model that relate to negativity, this is the only one that does not reach significance and is not negative. This suggests an important conclusion: for individuals who have made selections, negativity about

the other, non-preferred candidate has little effect on likelihood of turnout.

The substantive effects of increases in negativity about an individual's selected candidate are also substantively large. An increase of 20% in negativity about the selected candidate transmitted after the individual has made a selection leads to an 11% decrease in the individual's likelihood of turnout.¹⁵ An increase of 40% in the same type of negativity leads to a 24% decrease in the likelihood of turnout.¹⁶

4.2.6.2 Model 3: Selecteds

Another way of considering the effect of negativity by target, is to focus only on those individuals who have made selections (therefore I can once again refer to candidates as the selected and unselected candidate):

(Model 3) Turnout = $f(\alpha + \beta_1 \text{Neg.: Selected Candidate} + \beta_2 \text{Neg.: Unselected Candidate} + \text{Controls})$

Here the predicted results are simpler: following my theoretical premises, we should observe that only negativity about the selected candidate (β_1) has a significant, negative effect on likelihood of turnout.

The results of estimating model 3 are shown in Table 4.3. Following predictions, the coefficient on negativity about the *selected candidate* is both negative and significant. Meanwhile, the coefficient on negativity about the other, unselected candidate

¹⁵The 20% change is moving from an environment where there are no ads about the selected candidate, to one where 20% of all negative ads are about the selected candidate.

¹⁶Both of these changes in likelihood are significant using a 95% confidence interval. A more complicated case are the coefficients on negativity alone (i.e. the non-interactions), which are both negative and significant. As I rely on interactions, the coefficients on negativity alone represent the effect of negativity when selection = 0, or an individual who reports that he might still change his mind and vote for a different candidate. As I mention above, it is very difficult to interpret what these coefficients mean for my theoretical premises. My data provide an individual's selection state at the time of their interview, meaning that it is possible that some unselecteds will make selections after their NAES interviews but prior to Election Day. In short, unselecteds at the time of interview, may become selecteds later, making the effect of negativity on unselecteds difficult to capture and interpret.

Table 4.2: Effect of Negativity and Selection on Turnout in the 2000 Campaign: Targeted Negativity

Variable	Coefficient	(Std. Err.)
Negativity and Selection		
Selection×Negativity:Liked	-0.806[†]	(0.418)
Selection×Negativity:Disliked	0.053	(0.679)
Selection	0.173	(0.281)
Negativity About Liked Candidate	-1.387*	(0.700)
Negativity About Disliked Candidate	-2.293**	(0.767)
Early Negativity	-1.882	(1.665)
Resources		
Income	1.232**	(0.446)
Education	0.974	(0.718)
Age	0.003	(0.009)
Internal Efficacy	-0.370**	(0.088)
External Efficacy	0.382**	(0.129)
Employed	0.061	(0.242)
Evaluation of Parties and Candidates		
Strength of Partisanship	0.588	(0.382)
Affect for Candidate	0.002	(0.005)
Care About Outcome	-0.704**	(0.269)
Social Involvement		
Years	0.054**	(0.011)
Church	0.050	(0.112)
Mobilization		
Contact	-0.032	(0.325)
Other Mobilization	-0.417	(0.312)
State Competitiveness	1.043**	(0.241)
Interest and Exposure		
Interest	-0.106	(0.150)
All Media	0.138	(0.191)
Television Habits	0.068	(0.044)
Knowledge	-0.185	(0.149)
Ad Volume	0.010**	(0.004)
No Ads Shown	-0.231	(0.427)
Other Demographics		
Black	0.014	(0.427)
Hispanic	-0.340	(0.873)
Gender	0.305	(0.238)
Cluster Control and Intercept		
Cluster Control	-0.004	(0.003)
Intercept	-0.054	(1.058)

[†] ≤ 0.1, * ≤ 0.05, ** ≤ 0.01, N=285, Robust Standard Errors, logit

does not reach significance. In short, these coefficients once again follow my theoretical premises: after selection, it is negativity about the selected candidate that is most demobilizing.

I present the substantive results of this estimation in a similar manner to that used in Chapter 3. Specifically, in Figure 4.3. I show the change in likelihood of turnout as a function of increases in negativity. Figures 4.3 (a) and (b) show the change in turnout as a function of negativity about the *selected* candidate for weak and strong partisans; Figures 4.3(c) and (d) do show the change in turnout as a function of negativity about the other, unselected candidate. In these figures, the point shows the change in likelihood, while the dashed lines show the confidence interval around this change. Where the confidence interval crosses the 0 line, changes in likelihood are not significant.

These figures show that the changes in likelihood of turnout are only significant for increases in negativity about the *selected* candidate. Changes in negativity about the *unselected* candidate, on the other hand, are substantively small and always cross the 0 line, meaning that they are also not significant. Moreover, these changes are substantively large. An increase in post-selection negativity about the selected candidate from 0% to 40% would make a weak partisan 8% less likely to turn out. An increase in the same type of negativity from 0 to 60% would make a weak partisan 13% less likely to vote. In short, after selection it is negativity about the selected candidate that has an important demobilizing effect.

4.2.7 Conclusions

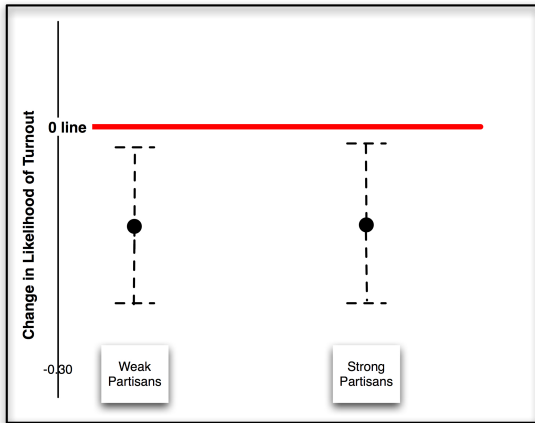
The results of this analysis fill out the story I began in Chapter 3. Relying on panel data and a rich advertising dataset, the first analysis of this chapter directly addresses the relationship between negativity and selection. Further individual-level analysis only reinforces my case, as we observe that negativity is most likely to de-

Table 4.3: Effect of Negativity and Selection on Turnout in the 2000 Campaign: Targeted Negativity, Only Selecteds

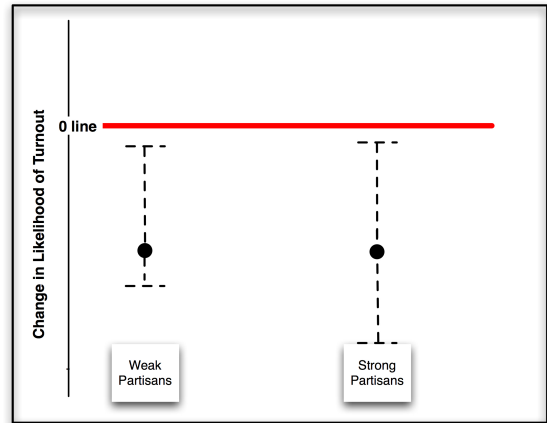
Variable	Coefficient	(Std. Err.)
Negativity and Selection		
Negativity About Selected Candidate	-1.359*	(0.675)
Negativity About Other Candidate	-0.634	(0.799)
Early Negativity	-1.280	(2.624)
Resources		
Income	1.911*	(0.838)
Education	0.974	(0.718)
Age	0.002	(0.015)
Internal Efficacy	-0.350*	(0.160)
External Efficacy	0.392*	(0.161)
Employed	-0.202	(0.318)
Evaluation of Parties and Candidates		
Strength of Partisanship	0.088	(0.541)
Affect for Candidate	-0.002	(0.004)
Care About Outcome	-1.105**	(0.348)
Social Involvement		
Years	0.049**	(0.012)
Church	0.204	(0.131)
Mobilization		
Contact	-0.457	(0.371)
Other Mobilization	-0.378	(0.414)
State Competitiveness	1.077*	(0.434)
Interest and Exposure		
Interest	-0.232	(0.182)
All Media	0.054	(0.223)
Television Habits	0.048	(0.053)
Knowledge	-0.032	(0.195)
Ad Volume	0.008*	(0.004)
Other Demographics		
Black	0.041	(0.601)
Gender	-0.350	(0.312)
Cluster Control and Intercept		
Cluster Control	-0.008*	(0.004)
Intercept	2.597	(1.751)

† ≤ 0.1 , * ≤ 0.05 , ** ≤ 0.01 , N=202, Robust Standard Errors, logit

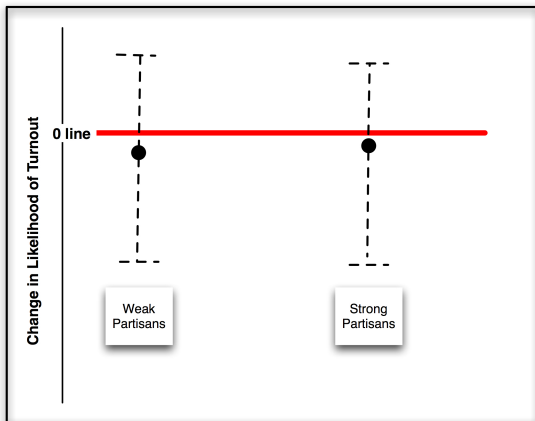
Figure 4.3: Effect of Negativity on Turnout: Selected Candidate Versus Unselected Candidate



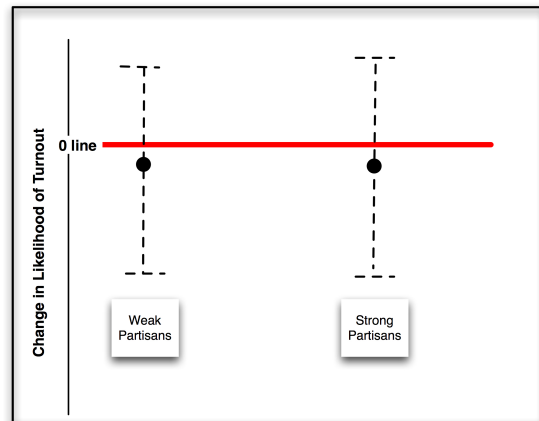
(a) Increase in Negativity About Selected Candidate From 0 to 40%



(b) Increase in Negativity About Selected Candidate From 0 to 60%



(c) Increase in Negativity About Unselected Candidate From 0 to 40%



(d) Increase in Negativity About Unselected Candidate From 0 to 60%

mobilize individuals who have made selections. In short, this result speaks to my key prediction: *negativity is most likely to demobilize after a selection, but before implementation*. The next analyses focus on the target of negativity and both reinforce a crucial point: after selection, it is negativity about the *selected* candidate that has a significant effect on likelihood of turnout.

While to this point all analyses have focused on individuals, in the next section I apply a different approach. Rather than relying on survey data, I will use aggregate data to consider the relationship between negativity, selection and turnout. More than merely changing datasets, in the next section I also consider the role of negativity from a different direction by accounting for campaign strategy.

4.3 The 2000 Campaign: Aggregate Analysis

Within a given election year, there is tremendous variation in the number of campaign advertisements between media markets. While some media markets receive numerous advertisements, other media markets see little if any campaign advertising. The targeting of these advertisements is not random, but rather the product of strategic decision-making on the part of presidential campaigns (Shaw 2006). As Shaw (2006) writes: “presidential campaigns see the world in terms of amassing 270 electoral votes, which requires identifying, persuading, and/or mobilizing a requisite number of voters in battleground states. In short, they make plans. Campaigns do not seek to talk to everyone, and if they can avoid ‘wasted effort,’ they will” (4). In this section I address this differential targeting of campaign advertising. I show that even once we account for the strategic distribution of activity, the timing of exposure still remains a crucial factor in the relationship between negative advertising and turnout.

In my second analysis, I will once again rely on advertising data from the Wisconsin Advertising Project, and will merge this data with county-level turnout data from

the 2000 election. I will then estimate the effect of negativity on turnout using an approach similar to the one used to analyze elections 1976 to 2000 in Chapter 3. Next, I first describe how my dataset county-level turnout, next I present my estimation of targeting and finally present the full results of this analysis.

4.3.1 Data and Methods

For this second analysis, I constructed a dataset of aggregate turnout in the 2000 election. To determine the turnout rate in 2000, I relied on three sets of data: (1) county-level vote totals for the 2000 election,¹⁷(2) the voting age population per county¹⁸ and (3) the population of voting age non-citizens per county.¹⁹ I calculated the turnout rate per county after first excluding the non-citizen population. As my unit of analysis is the media market, I aggregate my county-level data by media market.²⁰

As I mention above, data about advertisements once again come from the Wisconsin Advertising Project (Goldstein, Franz and Ridout 2002). Just as I did for the individual level analysis, I use presidential advertisements aired during the general election. Again, I rely on Geer’s definition of negativity and consider all advertisements that include a negative appeal as negative advertisements.²¹

Since my turnout data is aggregate, I cannot account for the timing of individual selection. Therefore, here I apply the same approach I do in Chapter 3: I rely on

¹⁷Obtained using Leip’s Atlas of U.S. Presidential Elections. <http://www.uselectionatlas.org> (February 25,2008).

¹⁸Obtained using U.S. Census Bureau; Census 2000, Summary File 1, generated by Yanna Krupnikov; using American FactFinder; <http://factfinder.census.gov>; (January-February 2008).

¹⁹I was not able to obtain data on felons per county, as it is nearly impossible to tie felons back to their county of origin, rather than county of incarceration.

²⁰Although this is an unusual approach, I aggregate to the media market level to ensure that all measurements at the same level, as the number of airings per advertisement is measured over a media market.

²¹Although I rely on the same combination of data as Krasno and Green (2008), I take a very different approach. Krasno and Green focus on what they call “media zones” – or different media markets within states. As a result their sample looks very different than the one used here. Moreover, the estimation approach and the model differ here as well.

the timing of advertising. Just as I do in the analysis of presidential election 1976 to 2000 in Chapter 3, I refer to the percentage of late advertisements aired in a media market in October 2000 or later as “Late Negativity.” Again, the logic is that these advertisements should be most likely to demobilize as they were aired after most individuals had already made their selections.²² I refer to the percentage of negativity aired in a media market over the entire duration of the campaign as “Overall Negativity.”

4.3.2 Estimation

I define “targeting” as a media market’s likelihood of receiving any advertising from either of two presidential candidates in the 2000 campaign. In order to consider how the strategic targeting of advertising affects the relationship between negativity and turnout, I rely on a two-stage approach to estimation. First, I estimate the likelihood of a media market receiving any presidential advertisements in 2000 and then I include this estimated likelihood in my model of turnout. Below I first discuss my approach to estimating the likelihood of targeting and second I discuss how this approach affects the controls used in my model of turnout.

4.3.2.1 Media Market Targeting

To estimate the likelihood that a media market received advertising, I rely on Shaw’s (2006) analysis of the 2000 presidential campaign.²³ Through interviews with campaign staff, Shaw pinpoints an algorithm by which the campaigns determined their targeting strategies. Using this algorithm, the campaigns identified a set of key battleground states; these states were targeted heavily during the general campaign. These were the states where the candidates spent the most money on advertising and

²²For more detail on timing of individual selections see Chapter 2.

²³I estimate the likelihood of receiving *any* advertising, rather than only negativity, as it would be very difficult to determine the strategy for the distribution of negativity.

other campaign activities.²⁴ These states were determined using the following characteristics: (1) past voting history, (2) polling numbers, (3) endorsements and other partisan organizational development, (4) other races in the state (5) focus on other issues and (5) “native son effects” (Shaw 2006: 55-56). I translate these key states to media markets. Since media markets cross state lines, I calculate the percentage of counties in each media market that belong to a battleground state.²⁵ In addition to this battleground variable, I also use the proportion of the population that is over 18 years of age. I rely on this variable as Shaw argues that campaigns seek to maximize their dollars by targeting advertisements to areas with higher concentrations of potentially eligible voters.

An analysis of the relationship between a media market’s exposure to advertising and these factors show that both, the percentage of counties in a battleground state and the proportion of population over 18 are significant and substantively strong explanatory factors. For example, moving from a media market where no counties are located in a battleground state to one where 50% of counties are located in a battleground states increases likelihood of that media market being targeted with advertisements by 38%. This change in likelihood is significant.²⁶

²⁴These states are Arizona, Kentucky, Louisiana, Ohio, Arkansas, Florida, Iowa, Maine, Michigan, Missouri, New Hampshire, New Mexico, Oregon, Pennsylvania, Tennessee, Washington, Wisconsin, Minnesota and West Virginia.

²⁵For example, the Pittsburgh media market has 15 counties, 1 is located in Maryland, 2 are located in West Virginia and the other 12 are in Pennsylvania. Of these counties, only Maryland was not a battleground state in 2000, meaning that 14 of the 15 counties in the Pittsburgh media market were battleground counties, giving this media market a score of 0.93.

²⁶Here I used a logit where the dependent variable was whether or not a media market received any advertising. The two factors were independent variables. The percent of counties in a battleground state was significant at 0.05; the proportion of population over 18 was significant at 0.1. I calculated predicted probabilities using *Clarify*. To calculate the predicted probability as a function of a change in the percent of counties in a battleground state I set the population variable at its mean. I also calculated the likelihood as a change from minimum to mean proportion of the population over 18. The change in likelihood here was 29.4% increase in receiving advertising. This increase was significant. See Appendix 4C for full estimates from this first stage model.

4.3.2.2 Controls

My reliance on Shaw's algorithm to estimate the likelihood that a media market received advertising is not without consequences for my model of turnout. Factors that explain turnout at the aggregate level may not be orthogonal to the process that determines targeting. I consider this possibility in specifying a model of turnout. While there are numerous factors that can be taken into account when considering turnout at the individual-level, I am limited by the aggregate-level analysis as well as the small number of observations in my media market-level analysis (N=75). As a result, I focus on factors that have repeatedly been shown to be important in explaining turnout, but which can also be approximated at the aggregate level. These factors are education (Wolfinger and Rosenstone 1980), average income (Verba et al. 1995) and the diversity of a media market – the percentage of individuals who identify as black (Hill and Leighley 1999).

Rather than simply including these variables as controls in my turnout model, I first consider the relationship between these variables and the likelihood of targeting. In other words, I consider if education, income and diversity are orthogonal to my estimation of targeting. A strong relationship between the factors that explain targeting and the control variables would be problematic for estimation. To consider this possibility, I analyze the relationship between my targeting factors and my control variables. The results show that there is a strong relationship between targeting factors and two of my three control variables: education and income. There is no statistically significant relationship between the diversity of a media market and the targeting variables. As a result, in estimating turnout, I rely on the diversity of the media market as a control variable.

4.3.2.3 Predictions

Following my key hypothesis that it is negativity *after selection* that is most likely to demobilize, I predict that Overall Negativity should have no significant effect on voter turnout. Conversely, I predict that Late Negativity should have a significant negative effect on turnout. This follows from the argument that negativity is most likely to demobilize individuals after they have made selections, but before they have implemented these selections with votes. Once again I also include a control for early negativity, although again, I make no explicit predictions about its effect on turnout.

4.3.2.4 Results

I first consider the effect of Overall Negativity. As I discussed earlier, this estimation was done in two stages; in the first stage, I estimated the likelihood that a media market received any presidential advertisements at all in the 2000 election, while in the second stage I considered the relationship between negativity and turnout. I present the result of the second stage in Table 4.4. Focusing on negativity, we see that the effect of Overall Negativity is negative, but does not reach significance.

Table 4.4: Effect of Overall Negativity on Turnout, Aggregate Results

Variable	Coefficient	Std. Err.
Overall Negativity	-0.018	0.045
Diversity	-0.237	0.093*
Intercept	0.605	0.020**

† ≤ 0.1 , * ≤ 0.05 , ** ≤ 0.01

Next, I considered the effects of Late Negativity – defined as advertisements aired after October 2000. The results, presented in Table 4.5, first show early negativity has a significant positive effect on turnout. This is similar to what we see in the analysis of presidential elections 1976 to 2000 in Chapter 3, though in the earlier analysis the coefficient on early negativity did not reach significance. Although I did not make any explicit predictions about the role of early negativity, this result is in

line with my theoretical premises that state that negativity before selection will help individuals reach selections. Turning to the focal Late Negativity variable, we see that the coefficient is both negative and significant, meaning that Late Negativity has a significant negative effect on turnout. These results reinforce those found in my individual-level analysis and reinforce the idea that negative advertisements can have a negative effect on turnout, but only at a certain time: *after individuals have made selections*. While these results are limited by the sparseness of the model, they nonetheless suggest that timing of negativity is important. Considered alongside my individual-level analysis, these results strengthen the case for the conditional effect of negativity.

Table 4.5: Effect of Late Negativity on Turnout, Aggregate Results

Variable	Coefficient	Std. Err.
Late Negativity	-0.116	0.049*
Early Negativity	0.144	0.069*
Diversity	-0.237	0.091**
Intercept	0.598	0.019**

† ≤ 0.1 , * ≤ 0.05 , ** ≤ 0.01

4.4 Conclusions

In this chapter I focused on the 2000 presidential election to test my main hypothesis through two independent analyses. In the first analysis, I used panel data to trace the relationship between an individual’s selection, negativity and turnout. In the second analysis, I accounted for the strategic targeting of advertising during the 2000 presidential campaign. Taken together, these analyses reinforce the importance of timing: negativity is most likely to demobilize after an individual has made a selection, but before he has implemented this selection with a vote. Considered alongside the analysis presented in Chapter 3, these tests reinforce the need to consider timing of exposure; it is notable that we can observe the same pattern over different elec-

tions and using independent data sources. The next step, then, is to consider the relationship between negativity, selection and turnout in greater depth. To do so, in Chapter 5 I turn to a series of experiments. Relying on these experiments I will not only test my main hypothesis, but I will also focus more directly on the mechanisms underlying the demobilizing effect of negativity after selection.

Appendix 4A

Data come from the National Annenberg Election Study, 2000, Multiple Reinterview Panel B.

- Selection: Coded based on a two-stage question. In the first stage respondents were asked if they intended to vote for Bush or Gore; in the follow-up question, they were asked if there is any likelihood they would still vote for the other candidate. Respondents who stated that there is no chance they would vote for the other candidate were coded as “selecteds”(1), those who stated they were undecided in the first round question *and* those who stated that there is still a chance they may vote for the other candidate were coded as “unselecteds” (0). Respondents who stated a third-party candidate in the first round, or who did not want to give a response to the second round question were excluded.
- Income: Individual income coded to match ANES categories used in Chapter 3.
- Education: Level of education coded to match ANES categories used in Chapter 3.
- Age: Taken from the NAES, continuous
- Internal Efficacy: Measured using NAES question: “People like me have no say over who is president”
- External Efficacy: Measured using NAES question: “Government run for few interests or benefits of all” (those stating that for few interests are coded as less efficacious)
- Unemployed: Is the respondent currently unemployed, binary, 1 if unemployed, 0 otherwise.

- Strength of PID: Seven point partisanship scale; constructed from three separate NAES questions; categorical
- Affect for Candidate: Difference in favorability scales for the two candidates
- Care about outcome: Taken from NAES question asking whether a respondent cares which party wins the election; binary
- Years: Number of years a respondent has lived in a community, continuous
- Church: How often does the respondent attend church, categorical
- Contact: Was the respondent contacted by either presidential campaign during the campaign process; binary
- Other Mobilization: Was the respondent contacted by any group other than the presidential campaign about the campaign process; binary
- State Competitiveness: Based on Shaw (2006). States that both campaigns classify as battleground are coded as 1; states that one of the campaigns classifies as battleground are coded as 0.75; states that both campaigns classifies “possible leaners” are coded as 0.5; states that one of the campaigns classify as “possible leaners” are coded as 0.25; states that both campaigns agree are solidly one party or the other are coded as 0.
- Interest: Based on measure of respondent’s interest in the 2000 campaign; categorical.
- All Media: Composite index of media usage including newspapers, radio and internet.
- Television Habits: How much attention the respondent paid to network or cable news about the 2000 campaign.

- Knowledge: Respondent's knowledge rated by NAES interviewer, categorical.
- Ad Volume: Number of advertisements shown in respondent's media market.
- Black: Is the respondent black, binary.
- Hispanic: Is the respondent hispanic, binary.
- Gender: Is the respondent male or female.

Appendix 4B

Table below compares those respondents coded as selected to those coded as unselected on various key characteristics. This is done to ensure that the results shown in Section 2 of Chapter 4 are more likely the result of exposure to advertising, than difference between the types of people who remain unselected for longer periods of time.

Characteristic	Selected	Unselected
Education, % with high education	38.51%	37.71%
Income, % with high income	45.96%	40.61%
Mobilization, % not contacted	85.68%	88.00%
Television, % not watching campaign news over last week	27.54%	26.29%
Knowledge, % with high knowledge	25.22%	21.14%
Partisanship, % with medium-high partisanship	49.88%	23.68%
State Competitiveness, % living in a competitive state	31.92%	32.00%
Efficacy, % efficacious	27.96%	29.48%

Appendix 4C

First stage of 2-stage model. Dependent variable is whether a media market received any advertisements at all (1 if yes, 0 otherwise).

Variable	Coefficient	(Std. Err.)
Percentage of Districts in Battleground States	4.863**	(1.981)
Percentage of Population Over 18	6.981*	(4.094)
Intercept	-4.806	(2.839)

* ≤ 0.1 , ** ≤ 0.05

CHAPTER 5

Experimental Results

In Chapters 3 and 4 I tested how the timing of exposure affects the relationship between negativity and turnout. In Chapter 3 I traced the effect of negativity over elections 1976 to 2000, while in Chapter 4 I used an interactive model to consider the effect of negativity in the 2000 election. Together, the analyses presented in the preceding chapters work to reinforce the key argument presented in Chapter 2: negativity is most likely to be demobilizing after an individual has made a selection, but before he has implemented this selection with a vote. While Chapters 3 and 4 begin to build a case for my theory, they do so without considering the mechanisms by which negativity comes to affect turnout: differentiation. Therefore, to examine the role of differentiation – as well as to analyze the relationship between negativity and turnout in a controlled setting – I turn to experimentation.

In this chapter I will present a series of experiments that test the main hypothesis of this dissertation: *negativity is most likely to be demobilizing at a certain time, after an individual has selected which candidate he prefers, but before he has had the chance to implement that selection with a vote.* In Chapter 2 I theorize that negativity affects turnout through differentiation. Exposure to negativity after a selection diminishes an individual's level of differentiation between the candidates. This diminishing level of differentiation leads the individual not to vote. Relying on

measures of both turnout likelihood and differentiation, I also test for this specified relationship between negativity, differentiation and turnout.

5.1 Why Experiments?

I turn to experimentation for a number of reasons. First, experiments give researchers the ability to isolate and focus on specific relationships between the political environment and individual behavior (Kinder and Palfrey 1993). Next, as Brader (2005) notes, the immediate effects of political communication are best analyzed in an experimental setting, where an individual's response can be measured immediately after exposure to strategic communication. Finally, in an experimental setting I have control over the timing of selection and the content of the communication transmitted, both of which will allow me to identify with more precision how selection moderates the effects of negativity.

In the experimental studies that follow, I consider different aspects of the relationship between negativity and turnout. The first experiment simulates the costs and benefits of an election in a controlled setting. The second experiment generalizes the results of the first by relying on a national adult sample and more realistic manipulations. Finally, the third and fourth studies act as checks on the results of the first two experiments.

5.2 Study 1: Costs and Benefits of Voting

My first study is a simple experiment focusing on the costs and benefits of voting. The goal of this first experiment is to test the effect of selection in the most controlled setting possible, while accounting for the costs usually associated with casting a vote. In this study I will test if negativity after a selection makes individuals less likely to pay the costs of voting.

5.2.1 Experimental Design

The first study simulated an American election in a laboratory environment. In every experimental session, subjects – undergraduate students at the University of Michigan – were told that at the end of the study they would be playing one of two games to earn points for entries into a lottery for \$100. However, the game that everyone in a given experimental session would play was to be determined by majority vote. Subjects also knew that a vote was costly.¹ If they voted for one of the two games, one point would be deducted from their eventual winnings. If they opted not to vote, they would still get to participate in the game “elected” by the majority and they would not have to pay the cost of voting. Though simple, my experiment calls to mind a controlled American election. In an election, voting is costly and an individual is never certain how much effect he will have on the electoral outcome. Moreover, just as a subject in my study could have paid the costs of voting and ended up with a game he did not like, so too a voter can pay the cost of voting and still end up with an elected official that he does not like. Finally, in an American election not voting is also an option; a non-voter does not pay any voting costs, does not directly contribute to the electoral outcome, yet he still has to live with whichever regime the voters elect.²

In this experiment the manipulation was informational. Subjects were randomly assigned to one of the following groups: (1)*pre group*: negativity before the selection,³ and (2)*post group*: negativity after the selection. All subjects first received neutral,

¹Checks were included in the experiment to ensure that subjects accurately understood the instructions, since misunderstanding the instructions could bias the results. In total, 6 subjects were excluded from the final result because they repeatedly answered check points incorrectly, despite screens giving them the correct answers.

²While it is true that selecting between two people is different than selecting between two games, there is no psychological research to suggest that the underlying decision process is different when individuals decide between two human alternatives versus between two “non-human” alternatives.

³Since all subjects were students enrolled in a particular major, I ran a pre-test of students majoring in this subject before conducting this experiment in order to ensure that subjects in this group would receive negative information about the alternative that they would have been most likely to select, in order to make the manipulation equivalent for both groups

descriptive information about the two games and only subjects in the pre group received negativity at this time. After this initial set of information, subjects were asked to state which game they liked better. This was equivalent to making a selection.⁴ Next, all subjects completed a filler task,⁵ and subjects in the post group were presented with negativity about the game they had selected. Finally, all subjects were given the chance to vote (or not vote).⁶ Subjects knew that the final vote was costly and if they cast the vote, one point would be deducted from any winnings in the game.⁷ The experimental task is shown in Figure 5.1. The statements used in this study are shown in Table 5.1.

Table 5.1: Study 1, Neutral and Negative Statements

Neutral Statement	Negative Statement
Game A is an analogies game where you must complete analogies. The most you can win in this game is 5 points, but you are not guaranteed to win anything.	Please note that many of the participants who played [Game A or B] found it to be very difficult; these participants could not win any points in this game even after repeated tries.
Game B is a math game where you must figure out solutions to math problems. The most you can win in this game is 5 points, but you are not guaranteed to win anything.	

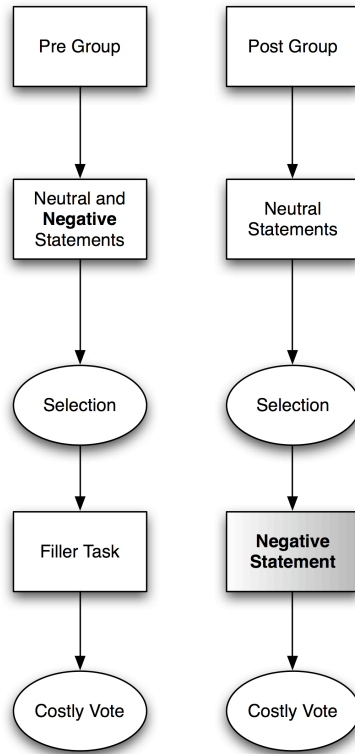
⁴Subjects had the option of saying that they liked both equally.

⁵This was done to create a longer selection time, as well as to ensure that subjects in the post group would not grow suspicious about the new information.

⁶Subjects completed the experiment by playing the game that won the majority of votes after this vote.

⁷I ensured that subjects knew this using check points that asked subjects about experimental tasks. Subjects who answered these check points incorrectly were subsequently excluded from the analysis.

Figure 5.1: Study 1, Order of Experimental Tasks



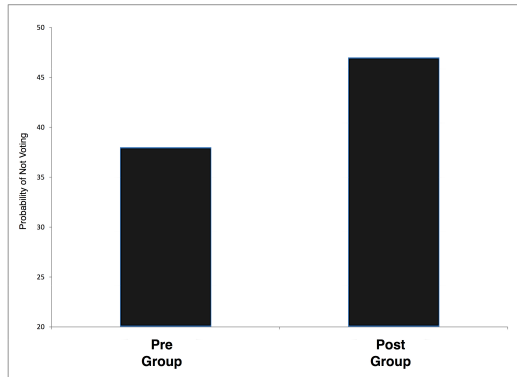
5.2.2 Results

To consider differences between groups, I calculated a subject's likelihood of paying the costs of voting as a function of both his experimental condition and of making the initial selection. Here I expect that subjects in the post group, who received negativity after selection, will be less likely to pay the costs of a vote than subjects in the pre group. The results, shown in Figure 5.2, are in line with my hypothesis: individuals who received negativity after making a selection were less likely to vote.

5.3 Study 2: Senatorial Election

The results of the first study support my prediction: subjects who received negativity after making a selection were less likely to pay the costs of voting than subjects who received negativity before making a selection. Nonetheless, this first study relied

Figure 5.2: Study 1, Percentage of Subjects Who Will *Not* Vote



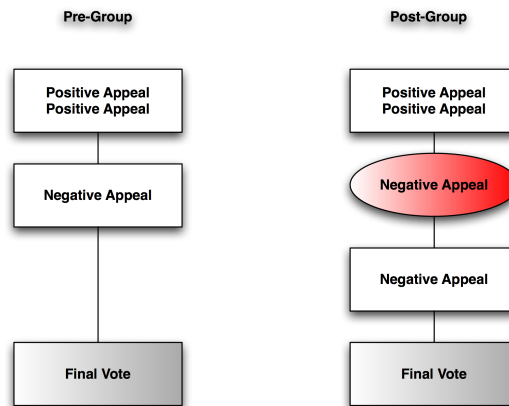
on an undergraduate sample and did not consider political appeals. Therefore, my next step is to conduct a second study testing my predictions with a more generalizable sample and more externally valid manipulations. In this next study I will also focus on the underlying mechanism by analyzing the mediational effect of differentiation.

While I cannot replicate an American political campaign in an experimental setting, I take a number of steps to increase the external validity of this second study. In particular, I field this study using Polimetrix, which provides me with a national, adult sample and a web-based study. In total, 535 adults participated in this study. In addition, because the experiment was web-based, subjects participated while sitting in front of their computers, surrounded by their ordinary, day-to-day distractions. While these conditions are still not entirely realistic, they come closer to mimicking the conditions under which individuals receive campaign information than an experimental laboratory which isolates individuals from all forms of distraction (Brooks and Geer 2007).

5.3.1 Experimental Design

The experiment began with all subjects specifying their partisanship. Next, subjects were told that they would be receiving campaign appeals from two candidates running for Senate in the 2008 election.⁸ In total, all subjects received three statements – a positive appeal about each candidate and one negative appeal. The manipulation in this experiment was *when* subjects received that final negative appeal. Some subjects were assigned to receive the final negative appeal after making a selection but before making the final implementation. Others were randomly assigned to receive all three appeals at once on a single screen and make the selection and the final implementation simultaneously. This meant that some subjects were exposed to negativity after making a selection, while others were not. All subjects were told that the implementation screen was their final chance to state a candidate preference. As I did before, I refer to the group exposed to negativity *after* selection as the post group and I refer to the group exposed to negativity *before* selection as the pre group. The experimental tasks are shown in Figure 5.3.

Figure 5.3: Study 2, Order of Experimental Tasks



For subjects in the post group (i.e. subjects who received the negative appeal

⁸The two candidates in question are Senate candidates as this study ran in April of 2008, during a heated presidential primary contest. As a result it would be nearly impossible to create a manipulation using presidential candidates.

after selection), the negative appeal focused on their selected candidate. For subjects in the pre group (i.e. subjects who received the negative appeal before selection), the negative appeal was about the candidate of the subject's partisanship. While partisanship does provide the best approximation of the candidate an individual would support absent any information, in order to ensure that the effect shown here is not a function of "mis-targeted" appeals in the pre group, I conduct additional analyses excluding subjects in the post group who identified as members of one party, yet selected a candidate of a different party.⁹ In the end, this experiment remains an explicit test of the effect of a selection: the campaign appeals are identical, it is only the timing of the appeals relative to a subject's selection state that changes.

5.3.1.1 Campaign Statements

The campaign statements are appeals of the type people may encounter during an actual campaign. All statements follow Geer's (2006) definition of negativity and positivity and focus on issues present on the major party platforms during the 2008 national campaign. Prior to the study, I conducted a series of cognitive interviews to ensure that individuals considered the statements to be both negative and realistic.¹⁰ Each statement is about a candidate; the positive statements praise a candidate and negative statements critique the candidate. The appeals identified the candidates'

⁹It is true that sometimes individuals opt to vote for candidates who do not share their partisanship. Nevertheless, partisanship does provide a fair approximation. In cognitive interviews conducted prior to the experiment with a different undergraduate sample, subjects were asked to discuss their thoughts on the campaign statements. All but 1 of the 15 subjects selected the candidate that matched his or her partisanship, even if they had reservations about the candidate's qualities. In separate cognitive interviews with an adult sample conducted throughout western Michigan, all of the subjects who identified themselves as partisans (n=20) selected candidates that matched their own parties. Finally, using the ANES, over presidential elections 1992-2000 only 6.8% of respondents (N=5196) reported intending to vote for a candidate who was not of their party. In short, this approximation is not ideal, but it is not unreasonable to assume that without any knowledge of the candidates, subjects will enter the experiment preferring a candidate of their own party.

¹⁰In total, I conducted 22 long-form interviews with adults. During these interviews statements were also calibrated to ensure that individuals did, in fact, perceive statements to be negative or positive.

partisanship. However, to increase the general applicability of these appeals, the two candidates were called “Candidate A” and “Candidate B” throughout both, the campaign statements and the rest of the study.¹¹ To ensure that the focus remained on the content of the message (i.e. its defined negativity or positivity), I follow Brooks and Geer (2007) and present the appeals as written statements. A full list of statements is shown in Table 5.2.

Table 5.2: Study 2, Experimental Campaign Statements

Positive Appeals	Negative Appeals
<p>Every year Americans give millions of their hard-earned dollars to the federal government. Candidate A, a Republican, supports eliminating unnecessary taxes like the death tax and the marriage penalty. Candidate A’s plan will help ordinary Americans like you.</p>	<p>In our country, millions of children go without medical treatment because their parents can’t afford health insurance. Yet [Candidate targeted by partisanship of subject], opposes subsidized health coverage for low-income children. It seems that [Candidate targeted by partisanship of subject] cares more about partisan politics than about our future – our children.</p>
<p>Candidate B, a Democrat, believes that in order to be safe from terrorist attacks, America must decrease its dependence on foreign oil. Candidate B will increase funding for research about alternative energy sources. We can trust Candidate B with our future.</p>	

¹¹I do this for two reasons. First, referring to the candidates as A and B eliminates any and all geographic cues from these campaign statements, ensuring that each member of my geographically-diverse sample has an equal chance of finding the statement compelling. Second, using A and B eliminates the possibility that subjects use a search engine during this web-based experiment as an attempt to learn more about the candidates discussed in these statements.

5.3.1.2 Dependent Variables

There are two types of dependent variables in this study. The first is a measure of turnout likelihood. Here, it was important to use a turnout measure that would speak to a subject's likelihood of turning out to vote for either of the two fictitious candidates, rather than his general turnout propensity. Moreover, this experiment uses hypothetical candidates for office, making a positive response to a "will you turn out to vote?" costless yet socially desirable. In short, asking individuals their turnout intent in an experimental setting may result in answers that reflect how subjects believe they *should* behave, rather than how they would behave were this a real-life scenario. As a result, I relied on turnout question designed to combat both of these problems. This question not only focused subjects on the two candidates mentioned in the campaign appeals, but also worked to combat social desirability by presenting subjects with a scenario in which they have already made it to the polls.¹²

This question was as follows:

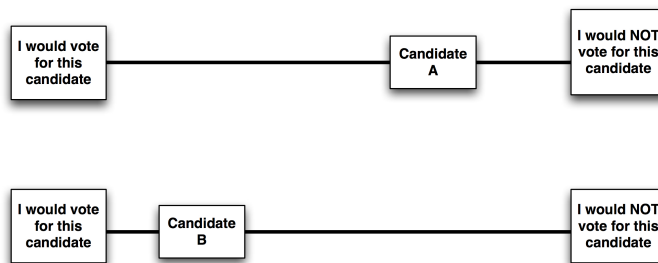
Suppose on the day of the election, you find out that your local polling place is closed due to an electrical outage. If Candidate A and Candidate B are the only candidates on the ballot, how much effort would you put into going to another polling location?

Here subjects had four response options, ranging from "I would put in a lot of effort" to "I would put in no effort."

A second type of dependent measure is differentiation. I measure differentiation using a relative placement scale (see Figure 5.4). Subjects were presented with a screen which asked them to position the candidates relative to each other on a 25-

¹²As this is an unusual turnout question, I conducted pre-testing using cognitive interviews. These interviews show that the question does work to combat social desirability and does measure an individual's intended turnout. Nevertheless, in order to ensure that this measure of turnout is comparable to more traditional measures, I also include a question in the main study that asks turnout intent in much the same way as it is asked on the ANES. I include the group differences for this measure of turnout as well just to reinforce the idea that my key effort question is, in fact, measuring turnout propensity.

Figure 5.4: Study 2, Differentiation Scale



point scale. At one end of the scale was a label reading “I would vote for this candidate,” and on the other side of the scale was a label reading “I would not vote for this candidate.” Subjects were told that the more likely they were to vote for one of the candidates, the closer that candidate should go to the “I would vote for this candidate” end of the scale. Subjects were instructed to place the candidates as they saw fit.¹³ The distance between the two candidates is my measure of differentiation.

These measures will allow me to test my main hypothesis, as well as the mechanisms underlying this hypothesis. First my theory predicts that individuals exposed to negativity after making selections should have a lower likelihood of voter turnout. To test for this effect, I will compare the reported likelihood of voting by group. Second, I consider weak and strong partisans separately. Recall that in Chapter 2 I argued that strong partisans likely have wider differentiation levels. If this is the case, then stronger partisans will be better able to withstand negativity and we should observe larger effects for weaker partisans. Third, I predict that differentiation is the mechanism by which negativity affects turnout. To test for this effect I will use my measure of differentiation as a mediator.

¹³The fact that I only use the relative placement of the two candidates will allow me to deal with issues of interpersonal incomparability that often arise when these types of placement scales are used to consider attitudes (Brady 1987).

5.3.2 Likelihood of Turnout

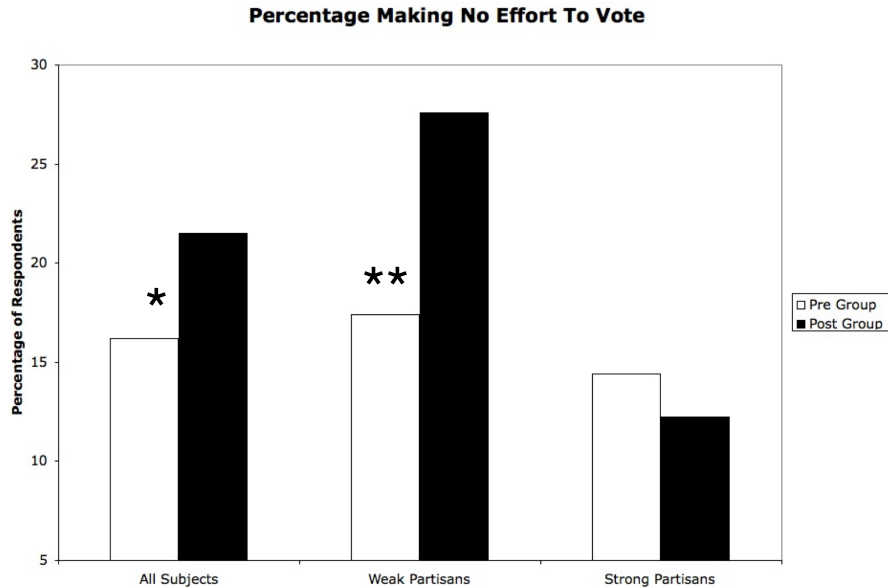
Before I consider group differences in likelihood of turnout, it is important to consider my results in context. As Lau et al. (1999) write, the median effect in a negativity/turnout experiment is 0.00. Moreover, unlike other studies, my experiment offers a stripped-down manipulation, devoid of images and sounds – factors that can work to increase effect size. Next, although the campaign statements are realistic, they do not offer real candidate names. Finally, this study was fielded in April 2008, during one of the most heated presidential primaries in recent American history. Thus, it is likely that all individuals expressed a higher propensity for turnout – no matter the experimental treatment. In short, in my experiment the deck was stacked against observing any substantial differences between groups.

To compare the two groups I measured the percentage of subjects who stated they would make very little or no effort to find a new polling place to those who stated that they would a lot or some effort to find a new polling place. Looking at the entire sample, 6% more subjects in the post group report that they will make no effort to vote. Focusing only on weak partisans, 11% subjects in the post group fall into the “no effort” category. This difference is significant at 0.05, in a two-tailed test.. These results are shown in Figure 6.1.¹⁴ These results already show that subjects in the post group – i.e. subjects exposed to negativity *after* selection – are less likely to report that they will turn out to vote in this election.¹⁵ This effect, as predicted, is larger

¹⁴Since those in the pre group received a negative statement about the candidate of their own partisanship, I conducted tests to ensure that these differences were not the result of mis-targeted information. These differences are robust to the exclusion of subjects who selected candidates of a different partisanship. In fact, differences only increase when these subjects are excluded from analysis.

¹⁵In order to ensure that this turnout question is comparable to more traditional measures of turnout, I also asked subjects how likely they would be to turn out and vote in an election where Candidates A and B were running for office. This second question was worded similarly to the standard ANES turnout question. We can observe group differences here as well, with 5% more subjects in the post group saying they will not vote in a full sample; this difference is 10% for weak partisans. This suggests that the effort question is measuring turnout propensity and potentially doing so with slightly greater precision.

Figure 5.5: Study 2, Percentage of Subjects Who Will *Not* Vote



for weaker partisans. In short, these results already show that timing matters. The next task, then, is to test the theoretical mechanism described in Chapter 2, namely the idea that the relationship between timing of negativity and likelihood of turnout is mediated by differentiation.

5.3.3 Negativity and Turnout: Mediation Analysis

As I explain in Chapter 2, I predict that negativity after a selection leads to lower levels of differentiation. Diminished levels of differentiation, in turn, make individuals less likely to vote. In order to consider the theory in full, then, I rely on a mediational analysis. More precisely, I will use the Sobel test of mediation (Baron and Kenny 1986) to consider how substantial of a role differentiation is playing in the effect of treatment on reported turnout likelihood. The Sobel test is a significance test of the indirect effect an independent variable has on a dependent variable via a mediator (Baron and Kenny 1986). While this test is often used in other disciplines, it is not often used in political science. Therefore, I will take some time here to

explain precisely how the Sobel test establishes mediation, and underscore some of this test's limitations. Below I rely on Shrout and Bolger's (2002) clarification of Baron and Kenny's (1986) discussion of mediation. I consider the mediational power of differentiation in three parts. First I show that differentiation levels differ by experimental group. Second, I explain why the Sobel test is the best approach by which to consider the mediational effect of differentiation and test for mediational effects. Finally, I consider how much of the relationship between negativity and turnout is being mediated by differentiation.

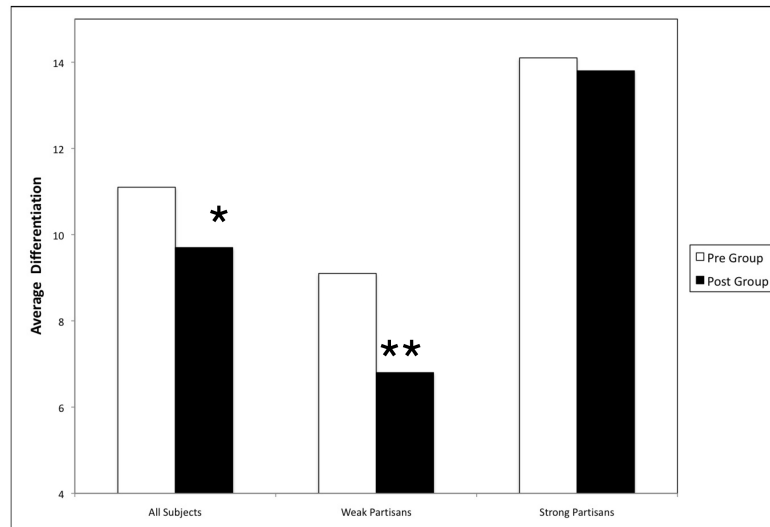
5.3.3.1 Differentiation Levels by Experimental Group

I theorize that the reason individuals exposed to negativity after selection are less likely to vote is because of diminishing differentiation levels. The first step to examining this idea is to consider differentiation levels by group. If my predictions hold, we should observe subjects in the post group have lower levels of differentiation. The results follow this pattern: subjects in the pre group reach average differentiation levels of 11.1, while subjects in the post group show lower average differentiation levels of 9.7. This difference is significant at 0.05. The distinction is even greater for weaker partisans. Weaker partisans in the pre group reach an average differentiation level of 9.1, while weaker partisans in the post group reach levels of only 6.8; this difference is significant at 0.01 (see Figure 5.6 for full results). This result speaks to my mechanism, showing that negativity after a selection does diminish levels of differentiation. The next step, then, is to systematically analyze if differentiation mediates the relationship between negativity and turnout through a Sobel test.

5.3.3.2 Defining Mediation

Although I discuss the mediational relationship between negativity, differentiation and turnout theoretically in Chapter 2, it is best expressed graphically. Figure 5.7a

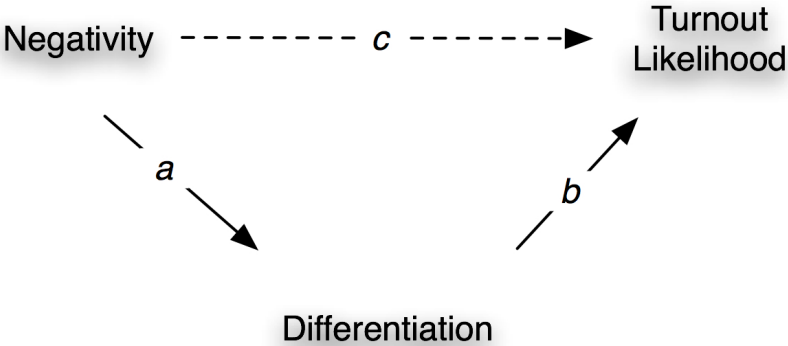
Figure 5.6: Study 2, Average Differentiation By Group and Partisanship



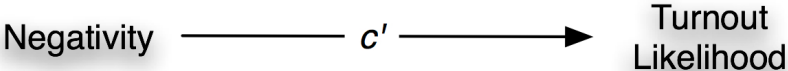
shows the theoretically predicted relationship between these three factors, while for comparison, Figure 5.7b shows a non-mediated relationship between negativity and turnout. Keeping Figure 5.7 in mind, the size of the indirect effect of negativity on turnout is $a \times b$, or the effect of negativity on differentiation (a) multiplied by the effect of differentiation on turnout (b). If $a \times b$ is statistically distinct from 0, we can say that some mediation has occurred. If $a \times b$ is completely equal to c' (the direct effect of negativity in the unmediated model), that means c (the direct effect of turnout in the mediated model) is 0. This means that relationship between negativity and turnout is fully mediated by differentiation. If $a \times b$ is smaller than c' , but still significant, then the relationship between negativity and turnout is partially mediated (Shrout and Bolger 2002).

Figure 5.7: Mediation and Non-mediated Processes

**Version A:
Mediation**



**Version B:
No Mediation**



5.3.3.3 Testing for Mediation

To consider whether differentiation mediates the relationship between negativity and turnout, I follow Greene's(2003) guidelines for a two-stage estimation of a non-linear model(184). Applying a two-stage approach I will consider the relationship between differentiation and negativity in the first stage, and I will consider the relationship between differentiation and turnout likelihood in the second stage. More precisely, I test for mediation as follows:

Stage 1: $\text{Diff} = f(\text{Treatment}, \text{Partisan Strength})$

Stage 2: $\text{Turnout} = f(\gamma)$

In the second stage, γ is the first round estimate of differentiation based on treatment and strength of partisanship. The first stage is a linear model, while the second stage is non-linear. I include strength of partisanship in the first stage of estimation (rather than the second) following the theoretical premises presented out in Chapter 2 which suggest that the stronger the partisanship the higher the level of differentiation.

The results of this two stage approach are presented in Table 5.3. The results follow my predictions. The first stage shows that receiving negativity after selection decreases levels of differentiation; the coefficient here is both significant and substantively strong. The second stage, which uses the first round estimate of differentiation, shows that differentiation has a significant effect on turnout. The coefficient here is positive, meaning that the lower the level of differentiation the lower the likelihood of turnout. In short, differentiation mediates the relationship between negativity and turnout. After selection, negativity leads to lower levels of differentiation and lower levels of differentiation in turn decrease the likelihood of turning out to vote.

In order to ensure that this result is robust, I also consider the mediational effect of differentiation is through a series of models. The first model specifies the effect of negativity on turnout without accounting for differentiation. In the second model

Table 5.3: Mediation Analysis, 2 Stage Approach

Variable	Coefficient (Std. Err.)		Coefficient (Std. Err.)	
	Stage 1: OLS		Stage 2: Logit	
	Dep Var: Diff.		Dep Var: Turnout	
Treatment	-1.492 [†]	(0.763)		
Strength of PID	-5.947**	(0.776)		
<i>First Round Estimate of Diff (γ)</i>			0.11 **	(0.041)
Intercept	14.680**	(0.710)	0.282	(0.408)

N=532, † ≤ 0.1 , * ≤ 0.05 , ** ≤ 0.01

specifies the effect of differentiation on turnout without considering negativity. The final model then considers turnout as a function of both, differentiation and negativity. These models are as follows:

$$\text{Model 1: Turnout} = f(\alpha + \beta_1 \text{Treatment} + \beta_2 \text{Partisan Strength} + \epsilon)$$

$$\text{Model 2: Turnout} = f(\alpha + \gamma_1 \text{Diff} + \gamma_2 \text{Partisan Strength} + \epsilon)$$

$$\text{Model 3: Turnout} = f(\alpha + \rho_1 \text{Treatment} + \rho_2 \text{Partisan Strength} + \rho_3 \text{Differentiation} + \epsilon)$$

If negativity after selection has a significant demobilizing effect on turnout and if differentiation is mediating this relationship between negativity and turnout, these three models would produce the following results. First, the coefficient on β_1 should be negative and significant. This is because the post-group is coded as 1, so if the effect of post-selection negativity is demobilizing we would observe a negative relationship with turnout. Next, model 2 would yield a positive, significant coefficient on γ_1 . If it is a shrinking level of differentiation that leads to demobilization, then differentiation should have a significant effect on turnout; the coefficient here will be positive because the higher the level of differentiation, the higher the likelihood of turnout. Finally, if it is differentiation that is responsible for the relationship between negativity and turnout, then in model 3 the coefficient on differentiation (ρ_3), rather than the negativity treatment (ρ_1), will be significant.

The results of this alternative approach to considering mediation are shown in

Table 5.4. These results follow predictions. In model 1, the coefficient on negativity treatment (β_1) is negative and significant. This means that being in the post group has a negative, significant relationship to likelihood of turnout, showing that receiving negativity after selection makes an individual less likely to turnout and vote. Next, in model 2, the coefficient on differentiation (γ_1) is, as predicted, positive and significant, showing that the higher the level of differentiation the higher the likelihood of turnout (conversely, the lower the differentiation, the lower the likelihood of turnout). Finally, model 3 presents the key result. When differentiation and negativity treatment are both included in a model, it is only the coefficient on differentiation (ρ_3) that is significant. This result shows that to the extent negativity affects turnout, it does so by diminishing levels of differentiation. In turn, lower levels of differentiation lead to a lower likelihood of turnout.

Table 5.4: Mediation Analysis, 3-Model Approach; Effect on Turnout

Var.	Model 1		Model 2		Model 3	
	Coeff.	(S.E.)	Coeff.	(S.E.)	Coeff.	(S.E.)
Treatment	-0.363 †	(0.227)	—	—	-0.312	(0.272)
Strength	-0.650**	(0.247)	-0.195	(0.299)	-0.197	(0.299)
Diff.	—	—	0.121 **	(0.022)	0.119 **	(0.022)
Intercept	2.076**	(0.246)	0.719	(0.293)	0.904**	(0.337)

N=532, † ≤ 0.1 , * ≤ 0.05 , ** ≤ 0.01

A final test of mediation is the Sobel test, which is a means of calculating the standard error of the mediational effect (Sobel 1982). If we follow the description of mediation shown in Figure 5.7, the standard error of the indirect effect ($a \times b$) is as follows:¹⁶

$$s_{\hat{a}\hat{b}} = \sqrt{\hat{a}^2 s_b^2 + \hat{b}^2 s_a^2 + s_a^2 s_b^2}$$

Conducting the Sobel yields a Sobel coefficient of -0.031, with a standard error of

¹⁶This is the specification that Baron and Kenny (1986) use, although there is another way of specifying the standard error of the indirect effect. The other specification excludes the $s_a^2 s_b^2$ term. Although some argue that the second specification is preferred (MacKinnon et al 1995), simulations show that for sample sizes larger than 200, both estimators work equally as well. As my sample size is over 400, I will simply rely on Baron and Kenny's specification.

0.009, meaning that the effect of mediation is significant at 0.001. Once again, this final test confirms the power of differentiation as a mediator. For a longer discussion of the Sobel test, as well as other tests that rely on the Sobel coefficient see Appendix 5A.

All three tests fall in line with my theoretic predictions: the mediational effect of differentiation is significant, suggesting that the relationship between negativity and likelihood of turnout hinges on an individual's level of differentiation. These results, then, underscore that timing matters and also take this idea a step further by showing that the effect of negativity on turnout is mediated by differentiation.¹⁷

5.4 Alternative Explanations

The results discussed above show that negativity after a selection has a demobilizing effect on turnout. Although they fall in line with my main predictions, these results do raise some questions. First, are the differences between groups a function of negativity, or will any type of campaign statement offered after a selection shift turnout intent? Second, are the results shown above specific to the content of the campaign statements used in the experimental manipulations? Will campaign statements that cover different issues lead to similar outcomes? To answer these questions I turn to two additional studies. Study 3 deals with the alternative explanation that *any* additional communication after a selection will lead to a lower likelihood of turnout in the experimental setting and Study 4 replicates the results of Study 2 with different campaign statements.

¹⁷How can we be certain that in this study the causal chain travels from negativity to differentiation to turnout, rather than from negativity to turnout likelihood to differentiation? In short, could likelihood of turnout mediate a subject's level of differentiation? Although this is not theoretically plausible, it is plausible in practice as a result of study structure. Conducting mediational tests that rely on turnout likelihood as the mediator, however, shows that this is not the case. When specified this way the indirect effect is not significant for either of the measures of turnout.

5.4.1 Study 3: Alternative Campaign Statements

To consider the possibility that any additional information after selection may decrease likelihood of turnout, I turn to a national experiment run parallel to Study 2. Like Study 2 this was a national, web-based study where the sample of adult subjects was $N=478$.¹⁸ This second study is identical to the first in all respects, except the tone of campaign statements presented to the subjects. While in Study 2 subjects received two positive appeals and one negative appeal, in this second study subjects received two negative appeals and a single positive appeal. The statements shown to subjects in Study 3 are shown in Table 5.5. The implication of this change in statements is that subjects assigned to the post group now saw a *positive* statement after making a selection (see Figure 3 for the full order of experimental tasks). As the issue content of the appeals differs between the two studies, we cannot make direct comparisons between Study 2 and Study 3. However, Study 3 serves as a useful foil for my earlier results.

What can we expect from this study? If the demobilizing effects observed in Study 2 are a function of negativity after a selection – rather than the result of offering subjects *any* sort of campaign appeal after they make a selection – we should *not* observe a lower likelihood of turnout in the post group. Rather, we should observe either little difference between groups or a slightly higher likelihood of voting among the subjects in the post group. These expectations follow logically from my theory. Negativity after a selection diminishes an individual’s choice and eats away at his differentiation level, leading an individual away from implementation. In the absence of negativity after a selection, however, an individual moves naturally toward implementation. Thus, since both the pre and the post group have no post-selection negativity, subjects in both groups should be equally likely to vote.

¹⁸The two studies ran simultaneously, serving as four different experimental conditions; the randomization worked such that subjects were randomly assigned to one of the four total conditions.

Table 5.5: Study 3, Experimental Campaign Statements

Positive Appeals	Negative Appeals
<p>[Targeted Candidate and Partisanship] believes that in order to be safe from terrorist attacks, America must decrease its dependence on foreign oil. [Candidate A/B] will increase funding for research about alternative energy sources. We can trust [Candidate A/B] with our future.</p>	<p>In our country, millions of children go without medical treatment because their parents can't afford health insurance. Yet Candidate A, a Republican, opposes subsidized health coverage for low-income children. It seems that Candidate A cares more about partisan politics than about our future – our children.</p>
	<p>Every year Americans give millions of their hard-earned dollars to the federal government. Yet Candidate B, a Democrat, opposes eliminating unnecessary taxes like the death tax and the marriage penalty. Candidate B's plan won't help ordinary Americans like you.</p>

First, I consider voter turnout. On two of the three measures of turnout, we do not observe any significant differences between groups. Even once we focus only on weaker partisans, there are no differences between the pre and post groups that reach significance. Only in the case of the effort question do we observe a slight differences between groups – here, subjects in the post group are less likely to report that they will make no effort to find a new polling place.

Turning next to differentiation, subjects in both the pre and post groups report nearly identical levels of differentiation (12.4 for the pre group and 12.9 for the post group). Moreover, the difference between the two groups does not increase when we divide the groups by partisanship; when we consider weaker partisans, the average level of differentiation for the pre group is 10 and the average level for the post group is 10.8. Though we see some directionality here – individuals in the post group have slightly higher levels of differentiation – these differences are not significant.

In short, this third study suggests that the effect we observe in Study 2 is not an experimental artifact. Subjects are not less likely to vote simply because they are exposed to *any* additional campaign appeal after a selection, it is *negativity* that has a unique demobilizing effect after selection.

5.4.2 Study 4: Replication of Results

While Study 2 suggests that negativity after a selection does lead to a lower likelihood of voting than negativity before a selection, could this result be a function of the issues used in the experimental manipulations? To consider this alternative explanation, I present the results of a study that replicates the experimental tasks of Study 2 using different campaign statements. If the results of Study 2 are a function of the power of negativity after a selection, then we should observe the same results even once the campaign statements focus on different issues.

To test how robust my results are to changing the content of the campaign appeals, I conducted a replication study. Study 4 is similar in almost every way to Study 2 with two key exceptions: (1) the campaign statements focus on different issues and (2) the subjects are undergraduate students at the University of Michigan. The full experimental process follows what is detailed in Figure 3. This study was conducted using the web and $N=46$.¹⁹ The statements used in this new study are shown in Table 5.6.

Even in this small sample, the results follow my predictions and replicate the patterns shown in Study 2. Subjects in the post group are less likely to report that they will turn out and vote. In fact, while 11.54% of subjects in the pre group say they will *not* make any effort to find a new polling place in the event of an electrical outage, 26.3% of subjects in the post group say they will make no such effort. This

¹⁹At two conditions, this sample size allows me to detect group difference of $d = 1$ or above with a power of 0.8 and an α of 0.05. It allows me to detect a $d = 1.25$ or above with a power of 0.95 and an α of 0.05 (Maxwell & Delaney, 2004).

Table 5.6: Study 4, Experimental Campaign Statements

Positive Appeals	Negative Appeals
<p>Congressman Bill Smith, a Democrat, cares about your future. He will propose legislation that will increase federal funding for public universities. With this funding, schools like the University of Michigan will be able to offer more merit-based and need-based scholarships.</p>	<p>As a Senator, [Targeted Name and Partisanship] will propose policies that prevent student newspapers at public universities, like the University of Michigan, from publishing politically controversial articles and editorials.</p>
<p>Congressman Sam Thomas, a Republican, will work to aid education. Thomas will propose widening the federal income bracket for need-based financial aid, so that more students can get the support they need to continue their education at schools like the University of Michigan.</p>	

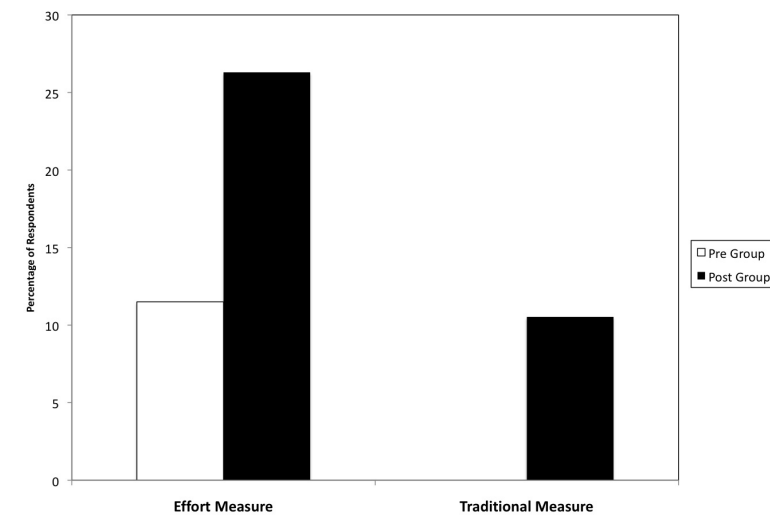
is a difference of nearly 15%.²⁰ These results suggest that what we observe in Study 2 is not a function of specific campaign statements, but rather an example of the fact that negativity has a more demobilizing effect after a selection. Full results from Study 4 are shown in Figure 5.8, where the effort measure of turnout is compared to the more traditional measure of turnout.

5.5 Conclusions

To recap, here I presented the results of four experimental studies. I relied on experimentation to bolster the results obtained using survey data. While survey data is useful, and allows for tests of my predictions in a “real world” setting, survey data does not allow me to fully consider the underlying mechanism I specify in my theory. Using experiments, I was able to isolate the selection, test how the effect of negativity

²⁰This difference is significant at 0.05

Figure 5.8: Study 4, Percentage of Subjects Who Will *Not* Vote



varies by selection state, and finally, test how differentiation mediates the relationship between negativity and likelihood of turnout.

Of the studies presented here, Studies 1 and 2 are direct tests of my main hypotheses, while Studies 3 and 4 serve as checks on these results. The first study focused on internal validity, and tested the effect of negativity in a laboratory setting with explicit costs of voting and a majority-determined outcome. The second study took a more direct and generalizable approach to test the effects of campaign negativity, providing a more externally valid test. Here subjects were exposed to realistic negative messages and asked about likelihood of turnout. In this study I was also able to directly test the mediational power of differentiation. The final two studies mimicked the design of Study 2 and attempted to speak to various alternative explanations of observed results.

In the end, these studies follow the empirical results shown in the preceding chapters: timing matters. Not only that, but studies were robust to alternative explana-

tions. What we observe is not an experimental artifact nor is it a function of specific types of campaign statements, but rather we observe the unique effect negativity has on likelihood of turnout. In short, the results presented here make one more step toward a key conclusion: *negativity is most likely to demobilize at a certain time, after selection but before implementation.*

Appendix 5A

The Sobel test (1982) provides a means of calculating the standard error of the indirect effect. Considering Figure 5.7 in Chapter 5, standard error of the indirect effect ($a \times b$) is as follows:²¹

$$s_{\hat{a}\hat{b}} = \sqrt{\hat{a}^2 s_b^2 + \hat{b}^2 s_a^2 + s_a^2 s_b^2}$$

It is important to acknowledge that Sobel's specification of the standard error of the indirect effect is most appropriate for large samples. Stone and Sobel (1990) show that the product of the a and b may have a positive skew if the sample size is below 400. This is actually most likely to happen if *mediation does exist* (Shrout and Bolger 2002). Sobel's test does not explicitly deal with the potential for positive skew in the distribution of the product, and this positive skew may reduce the power to observe mediation. As my sample passes the 400 mark, I will not spend much time discussing the nature of this bias here.²²

Once we establish the standard error of the effect, how can we establish its size? In other words, how much of the relationship between negativity and turnout is determined by differentiation changes? Shrout and Bolger (2002) suggest using a ratio of indirect to direct effects; they call this effect P_M , which is estimated as follows:

$$\hat{P}_M = (\hat{a} \times \hat{b}) / \hat{c}'$$

In cases of complete mediation, P_M will equal to 1, in cases of partial mediation, the

²¹This is the specification that Baron and Kenny (1986) use, although there is another way of specifying the standard error of the indirect effect. The other specification excludes the $s_a^2 s_b^2$ term. Although some argue that the second specification is preferred (MacKinnon et al 1995), simulations show that for sample sizes larger than 200, both estimators work equally as well. As my sample size is over 400, I will simply rely on Baron and Kenny's specification.

²²The precise concern here is that Sobel (1982) assumes a normal distribution in deriving the standard error of $a \times b$. Yet, if a is normally distributed and b is normally distributed, their product will skew positive (Shrout and Bolger 2002). In the end, this does not bias the estimate of the standard error, but the confidence interval: Stone and Sobel (1990) show that in the case of skew, confidence intervals produced asymmetric error rates – too wide intervals in the direction of the null hypothesis of no mediation and too narrow intervals in the direction of alternative hypothesis of mediation. Shrout and Bolger (2002) report that a number of scholars have shown the same effect using computer simulations.

ratio will be less than 1. In the end, if there are processes other than the specified mediator affecting the relationship between the independent and dependent variables, and we accounted for all these processes, the ratios would add up to 1.

What should we expect here? One expectation is clear: we should see a significant result on mediation. If my prediction that negativity affects turnout via differentiation holds, then the indirect effect here should be significant, suggesting that we are observing mediation. Less precise, however, is the size of this mediational effect. As I discuss in Chapter 2 I predict full mediation – meaning that negativity affects turnout solely through differentiation. To the extent that negativity stirs other effects – disinterest, declines in efficacy, I argue that these are products of declining differentiation as well. Nonetheless, the limitations of the data – and the tests – are such that statistically observing full mediation in this study may be unlikely, since it is difficult to measure turnout intention without error. As Bollen (1989) notes, measurement issues are a cause of observing partial mediation when full mediation is theoretically predicted. In short, my theory would predict full mediation here, but we are most likely to observe partial mediation.

Tests of mediation are crucial here, as these provide a more direct test of my theory than simple group differences. Therefore, my first step was to establish the significance of the indirect effect. This step relies on the Sobel test, which will help establish if there is any mediation here at all. In testing for mediation, I will also control for various covariates of turnout, including strength of partisanship.

Recall that my key measure of turnout is a question that asks subjects how much effort they would make to find a new polling location in the event of an electrical outage at their current polling location. This question is an attempt to deal with both, social desirability and focus on the 2008 election. The question does not mention 2008 and diminishes subject responsibility for voting by creating a situation where they have already made it to the polls. Here, the Sobel test shows that the effect of

mediation is significant at 0.01. This means that negativity is affecting likelihood of turnout through differentiation. Moreover, P_M in this case equals to 1.²³ This means that differentiation explains the full relationship between negativity and turnout.²⁴

These tests fall in line with my theoretic predictions: the mediational effect of differentiation is significant, suggesting that the relationship between negativity and likelihood of turnout hinges on an individual's level of differentiation. Moreover, the mediational effect of differentiation is substantive and reaches full mediation. In turn, as my earlier discussion shows, levels of differentiation depend on when a subject received negativity before or after making a selection. In short, these results underscore that timing matters and also take this idea a step further by showing that the effect of negativity on turnout is mediated by differentiation.²⁵

²³Actually, the raw ratio here is slightly greater than 1, but following Shrout and Bolger (2002) I set the limit to 1 because both the direct and indirect effects are in the same direction and thus can be interpreted clearly.

²⁴I also tested the mediational effect of differentiation using the more traditional measure of turnout as a dependent variable. The result shows that the mediational effect of differentiation is significant at 0.01, and the P_M equal 0.83. This shows partial mediation and is consistent with my argument that this more traditional question offers a less precise measure of turnout intention.

²⁵How can we be certain that in this study the causal chain travels from negativity to differentiation to turnout, rather than from negativity to turnout likelihood to differentiation? In short, could likelihood of turnout mediate a subject's level of differentiation? Although this is not theoretically plausible, it is plausible in practice as a result of study structure. Conducting mediational tests that rely on turnout likelihood as the mediator, however, shows that this is not the case. When specified this way the indirect effect is not significant for either of the measures of turnout.

CHAPTER 6

Conclusions

Do negative advertisements demobilize voters? In this dissertation I rely on a number of different empirical tests to show that the effect of negativity depends on the timing of exposure: negativity is most likely to demobilize after selection but before implementation. In this final chapter I consider the broader implications of this finding. I do so in three parts. First, I consider how my results relate to the existing literature on negativity and turnout. Second, I discuss the implications of my theory for candidate strategy. Finally, I apply a normative lens to the relationship between negativity and turnout.

6.1 Existing Research

What do my results mean for existing research? As I show in Chapter 1, the literature on negativity and turnout has produced conflicting results. Some scholars find that negativity is demobilizing (Ansolabehere and Iyengar 1995). Others find that negativity is mobilizing (Goldstein and Freedman 2002). Still others find that negativity has no effect on turnout (Finkel and Geer 1998). My dissertation relies on eight premises about individual decision making to highlight a heretofore ignored variable: the timing of exposure to negativity. Considering timing in this context brings a new perspective to the study of negativity and turnout, and this perspective

works to explain the conflicting results. Below, I focus on three prominent pieces in the literature on negativity and turnout. Accounting for timing of exposure alters our inferences, reconciling these different findings.

6.1.1 Negativity is Demobilizing

The evidence I present shows that negativity is most likely to demobilize after selection but before implementation. If this is the case, why do Ansolabehere and Iyengar (1995) observe the same result when they do not account for the timing of selection in their experiments? One answer to this question may lie in the design and timing of the Ansolabehere and Iyengar experiments. First, Ansolabehere and Iyengar field their experiments during on-going campaigns, using real candidates. Included in their sample of ads and elections was the 1990 race for California governor between Dianne Feinstein and Pete Wilson. Moreover, the 1992 California primary for US Senate, the 1992 presidential election between Bill Clinton and George H.W. Bush as well as a number of other races. What is notable about these studies is that they were fielded over a period of several years, and within those years at varying months. In particular, experiments where individuals were exposed to the most negativity were fielded late in the campaign season.¹ How may this experimental design affect Ansolabehere and Iyengar’s results?

Consider the campaign space, as shown in Figure 6.1(a). In this space, a campaign occurs from August to November of an election year. Given the data on selection timing shown in Chapter 2, I make the assumption that advertising shown in August and September (“Early Advertising”) is most likely to reach individuals in the pre-selection stage, while advertisements shown in October (“Late Advertising”) are more likely to reach individuals in the post-selection stage. Following my theoretical premises, then, early advertising would make an individual more likely to make a se-

¹Experiments conducted in October of 1990 and 1992, containing double advertisements designs are included in the negativity analyses.

lection; when there is no exposure to negativity after that initial selection, the simple act of picking one alternative over another would make an individual more likely to turnout. On the other hand, as I have shown in various ways in this dissertation, late advertising will make individuals less likely to turn out and vote.

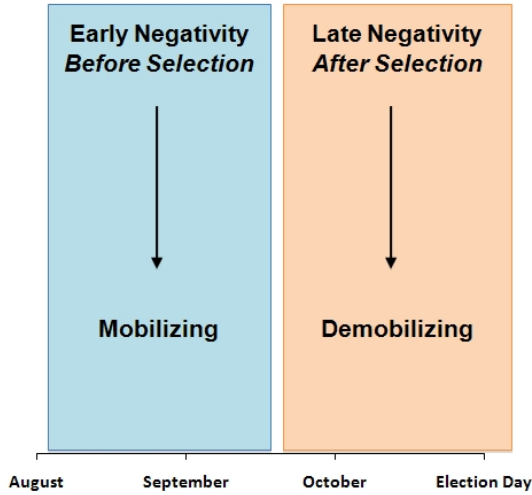
Keeping this in mind, I can plot the distribution of advertisements over time in Ansolabehere and Iyengar’s experiments. Aggregating across years, I simply consider *when* in a campaign a subject was exposed to negativity in Ansolabehere and Iyengar’s experiments. As Figure 6.1(b) shows, most of the subjects in these experiments who were exposed to negativity, were exposed in the *late* stages of the campaign. What this suggests is that many of the participating individuals were exposed to negativity after they had already made selections. In short, although Ansolabehere and Iyengar do not directly consider the effect of timing, they do, nonetheless, expose individuals to negativity at a point in time when it is most likely to demobilize – after selection. As a result, it is not surprising that they observe a demobilizing effect.²

6.1.2 Negativity is Mobilizing

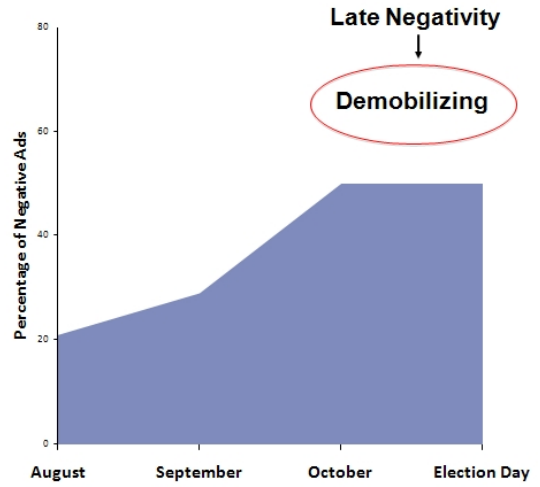
Goldstein and Freedman (2002) focus on differences in media markets during the 1996 election. Here, they measure negativity in a media market over the duration of

²I also reconsider Ansolobehere and Iyengar’s results using their own data. As I only have data from their 1992 primary experiment, this reconsideration is limited and I do not want to draw broad conclusions; moreover, I want to be cautious in considering these results. Nonetheless, as this experiment was conducted during an actual campaign, I can analyze the effect of negativity by selection state (though I do so in a crude manner). As subjects were asked whether they had selected which candidate they preferred, I use a measure of selection state to compare groups. First I compare the differences between those who had made selections and those who had not in the control group. Of those who had not made selections in the control condition, 28.6% said they would not vote; of those who had made selections, 33.33% said they would not vote. Using a one-tailed t-test, this is not a significant difference between the unselected and selected groups. Next, I compare those subjects exposed to negativity. Here, I find that of those who had not made a selection, 31.76% said they would not vote. Of those who had made selections, 37.32% said they would not vote. Using a one-tailed t-test this difference is significant at $p \leq 0.12$. In short, while those who have and have not selected appear largely similar in the control condition, they exhibit more difference in the negativity condition, with a higher number of selecteds reporting that they will not vote in response to negativity. Again, these are crude results, but they show a notable pattern in experimental results.

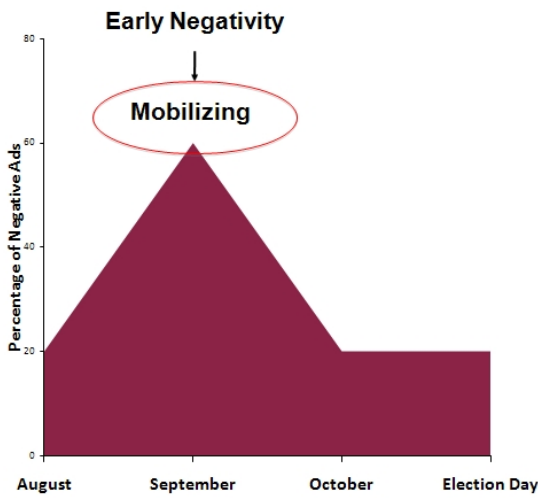
Figure 6.1: Distribution of Timing in Negativity in Existing Research



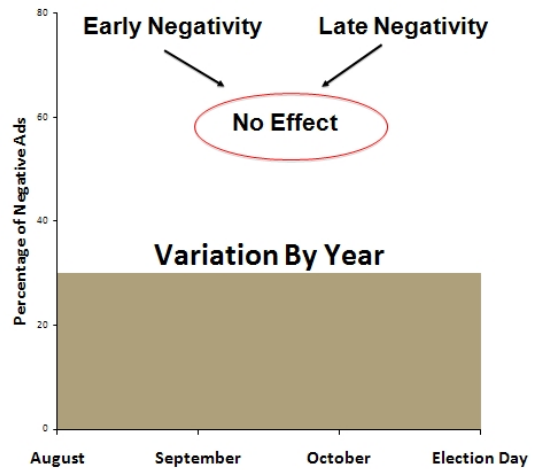
(a) Duration of Campaign and Expectations



(b) Ansolabehere and Iyengar(1995): Timing of Negativity



(c) Goldstein and Freedman(2002): Timing of Negativity



(d) Finkel and Geer (1998): Timing of Negativity

the campaign and find that more negativity leads to a higher likelihood of turnout. Can considering the timing of negativity change the inferences we can make from this result?

I reconsider the campaign advertisements shown in 1996 using John Geer’s data.³ The distribution of advertisements is shown in Figure 6.1(c) and shows that the 1996 election is “top-heavy” in its negativity. Of all negative advertisements in that election, only 20% were shown after October 1, 1996. In short, considering the campaign space in Figure 6.1(a), there was very little late negativity; most of the negativity was early, before individuals had made selections. This distribution of negativity makes the 1996 campaign different than, for example, the 1992 campaign where only 24% of all negative advertisements were shown *before* October 1, 1992. Thus, in 1996 most of the negativity was aired while individuals were making selections, and there was relatively little negativity was aired after individuals had made selections. Increases in early negativity helped individuals make selections, and with little negativity after selections to dissuade them, individuals exposed to negativity were more likely to vote.

6.1.3 Negativity Has No Effect on Turnout

Finally I turn to Finkel and Geer’s (1998) null result. The authors consider the effect of negativity over several presidential elections, measuring negativity over the entire duration of the campaign. I replicate this analysis in Chapter 3 to much the same effect. How can considering the effect of timing help explain Finkel and Geer’s null result?

We can approach this question in two ways. First, considering the effect of negativity over the duration of the campaign does not account for differences in the amount

³This is the same data I used to conduct the analyses in the first part of Chapter 3; this data is not the same data that Goldstein and Freedman used for their study. I cannot conduct a similar analysis with the original Goldstein and Freedman data as it does not contain dates.

of negativity when they matter most – after selection. Second, measuring the effect of negativity over the duration of the campaign – rather than at specific points in time – conflates the different effects of negative campaigning. While my empirical analyses focus on the demobilizing effect of negativity after selection, in Chapter 2 I argue that prior to selection negativity may help an individual differentiate. In turn, successful differentiation leads to selection, and making a selection increases the likelihood that a person will turn out to vote. Measuring negativity over the duration of the whole campaign, however, cancels out the positive effects of negativity prior to selection with the negative effects of negativity after selection, producing a null result. I show this effect against the campaign space in Figure 6.1(d).

6.1.4 Conclusions

In sum, accounting for the timing of exposure helps shed new light on the existing literature. While initially the three studies discussed above appear to reach conflicting results, incorporating timing suggest that there is more agreement than is first apparent. To the extent that these analyses lead to different interpretations of the relationship between negativity and turnout, they do so because they inadvertently consider negativity at different points in time. Ansolabehere and Iyengar (1995) conduct their experiments late in the campaign, when many of their subjects have already likely made selections. Goldstein and Freedman (2002), on the other hand, focus on an election when most of the negative advertisements were aired early, at a time when negativity can help individuals make selections. Finally, Finkel and Geer (1998) measure the effect of negativity over the duration of the campaign, leading the effects of pre- and post-selection negativity to cancel each other out.

6.2 Candidate Strategy

What do these results suggest for candidate strategy? Or, more precisely, how might candidates use negativity? Much of the campaign effects literature has focused on the idea that campaigns have very little role in voter behavior. In fact, as survey evidence has shown, many individuals select which candidate they prefer quite early in a campaign (Holbrook 1996) and by early October of an election year, most individuals have made selections.⁴ This leaves candidates with few options for changing election outcomes.

One approach is to focus on those voters who have yet to make selections (the “undecideds”).⁵ This is a reasonable strategy and one that is often touted as the best option in both political science research (Holbrook 1996) and the mainstream media (Rohter and Zeleny 2008). Given that low numbers of these types of individuals, are there other approaches a candidate could take?

An alternative strategy is to change the minds of voters who have made selections (the “decideds”). Existing research on decision making gives little impression that negativity can change minds. As I discuss in Chapter 2, Holyoak and Simon (1999) and Simon et al. (2001) find that once selections are made individuals rarely change their minds and return to the unselected alternative. This point is borne out in research on campaigns as well, where voters who have made selections are unlikely to shift their preferences to the other alternative (Holbrook 1996).⁶ In short, a candidate is unlikely to convince those individuals who have selected his opponent to switch their selections.

My dissertation suggests that candidates may do better to use negativity in a

⁴Evidence from ANES and NAES. See Chapters 2 and 3 for longer discussion of these findings.

⁵Undecideds is a term used by the mainstream media to describe voters who have not made selections; see Rohter and Zeleny 2008 for example.

⁶In the extensive meta-analysis of negativity studies, Lau et al. (2007) find some evidence that negativity very slightly decreases affect for the target of the attack. This evidence, however, does not speak to vote switching behavior, as it is unclear if individuals had made selections prior to exposure to negativity.

different way. The evidence presented here shows that negativity is most likely to demobilize after selection. Following this logic, a candidate could affect the outcome of an election by discouraging the supporters of his opponent from turning out to vote. This approach would mean focusing the transmission of negative advertisements during the month of the October. As I show in Chapter 3, negativity at this point in the election has a substantive and significant demobilizing effect, decreasing the likelihood of turnout by 14.4 percentage points.

In short, while I do not disagree that pursuing voters who have not yet made selections (the elusive “undecided voters”) is a worthwhile strategy for candidates, I suggest that it would be unwise to ignore those who have made selections. To the extent that electoral outcomes are decided by those who turn out on Election Day, taking the opportunity to discourage individuals from acting on their candidate selections by relying on negativity is a viable path to victory.

6.3 Broad Implications

Taking a step away from the existing literature and candidate strategy, let us consider the findings of this dissertation in a different perspective. Let us assume that in the American representative democracy, voting is a useful behavior. Put another way, let us assume that we want to see higher levels of voter turnout (Hill 2006).⁷ Under this assumption, the fact that negativity can be demobilizing gives reason for concern. Does this mean we would be better off without negativity? Should negativity be heavily regulated or even eliminated from campaigns?⁸

Certainly negativity is not the most pleasant part of any campaign – a fact that many scholars, journalists and politicians will readily acknowledge. Kamber (1997)

⁷Not all would agree with this assumption, see for example Highton and Wolfinger (2001).

⁸See Ferguson (1997) for a thorough discussion of the constitutionality of regulating negative campaigning, with a focus on the Florida case. Also see Thompson (2004) for a discussion of why all campaign activity should be regulated.

notes that negativity brings the level of the political discourse “to the level of tabloid scandal” (xiii). Meanwhile, former Senate majority leader Tom Daschle calls negativity the “crack cocaine of politics.”⁹ As Geer (2006) writes, “we are, in effect, awash in a sea of negativity about negativity”(1). Yet, the elimination of negativity from the political campaign is less clear-cut than these views suggest.

When we consider the timing of exposure, we learn that negativity is not always harmful. As I theorize in Chapter 2, negativity can be helpful. Before a selection, exposure to negativity can help individuals differentiate between candidates. As a result, eliminating negativity may make selection more difficult. A similar logic is applied in a series of experiments by Garramone et al. (1990) which show that negativity does help individuals make selections. Geer (2006) argues that negative advertisements help voters learn more political information than positive advertisements. Moreover, as Joel Rivlin of the Wisconsin Advertising Project notes “negative ads are more likely to talk about policy than positive ads. How else do you find out about the flaws of a candidate besides a negative ad?”¹⁰ In short, negativity may be unpleasant, but it can be helpful.

While the above are worthwhile considerations, I want to take the role of timing in the relationship between negativity and turnout a step further. What the evidence presented in this dissertation shows is that negativity is not demobilizing simply because it is *negative*, but rather because at a certain point in an individual’s decision process it makes him less likely to act on his selection. What makes the voting decision process particularly susceptible to this sort of effect is its structure: in most cases there is a temporal gap between the selection and the potential for implementation.¹¹ As I discuss in Chapter 2, individuals are particularly likely not to implement their

⁹This was quoted in a 2002 *Washington Post* column by George Will.

¹⁰Joel Rivlin of the Wisconsin Advertising Project as quoted in a September 2006 *New York Times* article by Adam Nagourney.

¹¹This idea is particularly salient in the American context. While the psychological principles of decision making can translate, it is the American campaign with its length and structure that makes for this particular relationship between decision structure and negativity.

selections when just such a time gap exists (Bagozzi and Dholakia 1999). In short, it is unclear if negativity would have such a strong demobilizing effect were it not for the particular structure of the voting decision.

Following this logic, then, eliminating the demobilizing power of negativity does not simply mean eliminating negativity itself. Even if there were no negative campaign advertisements, the temporal gap between selection and implementation is such that other factors could discourage individuals from action. This suggests that rather than focusing on negativity, we should consider the very structure of the voting decision. In particular, one means of working against negativity would be to narrow the temporal gap between selection and opportunity for implementation. While it would be impossible to control the timing of selection, there is more flexibility with timing of implementation.

Currently, numerous states have policies that allow for early voting. In fact, more and more states have stopped requiring excuses to cast these votes.¹² Yet it is unclear how many individuals are aware that they have the opportunity to vote early.¹³ Rather than focusing on negativity, effort could be channeled toward educating individuals about these types of opportunities. Providing individuals with the tools to cast early ballots would give them the chance to implement their selections, thereby narrowing the temporal gap between selection and implementation. Narrowing this gap could, in turn, insulate individuals from the demobilizing effects of negativity.¹⁴

In short, when we understand that the demobilizing power of negativity lies in the context of exposure to this type of campaigning, we reach a simpler solution. Rather than regulating candidates' speech during campaigns, greater effort could be made to

¹²Currently 32 states allow no excuse early voting in person; only 4 states do not allow any kind of early voting.

¹³See Gronke and Toffey, forthcoming, for demographics on the types of individuals who vote early.

¹⁴Thompson (2004) would disagree with this argument, suggesting that there is something important in the fact that all individuals vote together on Election Day. While I understand his logic, I would suggest that the same "simultaneity" could be achieved if all the ballots are counted on Election Day, even if not all ballots are cast on the same day.

educate individuals of institutional opportunities that already exist in many states. Such a solution resolves the tension in the conditional theory of negativity suggested in this dissertation: we can maintain the benefits of negativity for differentiation, yet provide individuals with the tools necessary to avoid its demobilizing post-selection effects.

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