POLITICAL CORRUPTION AND POLITICAL ENGAGMENT: A MULTILEVEL ANALYSIS INVESTIGATING THE EFFECT OF POLITICAL CORRUPTION PROSECUTIONS ON VOTING AND GOVERNMENT TRUST IN THE UNITED STATES

by

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MAJOR PROFESSOR: Dr. Rachel Whaley

Past research has confirmed the importance of structural and individual-level factors in predicting voter turnout and citizen trust in the government. In international research particularly, political corruption has been shown to negatively affect citizen trust, though the effect of corruption on voter turnout is mixed. To date, no research has examined the effect of corruption on voting and government trust in the United States over a relatively long period of time. In this dissertation, I aim to answer two primary research questions: how U.S. corruption affects voting and how it affects citizen trust in the government. Using many sources of data for state-level variables, and the American National Election Study (NES) for individual-level variables, I investigate these relationships using multilevel modeling (MLM) of forty-six states and approximately 22,000 individuals in my analysis of voting and forty-one states and about 7,000 individuals in my analysis of political trust. I find that corruption has a small, but significant, negative effect on voting. Surprisingly, I find no effect of corruption on a citizen's political trust, even after assessing the impact of corruption on four other specifications of trust. I also investigate cross-level interaction effects for each analysis, and find no significant results. I conclude with a discussion of possible explanations for these findings, make policy recommendations with the knowledge gained from this research, and offer suggestions for future investigations.

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INTRODUCTION

How do corrupt governments affect citizen political participation? Assuredly, corruption is a hot topic in the media, with television shows like *House of Cards* and *Scandal* surging in popularity, newspapers and 24-hour news channels quick to highlight the latest government malfeasance, and the importance political presidential candidates place in presenting themselves as outsiders there to shake up a broken system. Might political corruption play a tangible role in our lives and affect how we view the world around us to the degree that it influences our political activity? Does state-level corruption influence individual-level political behavior and trust?

To answer these questions, this dissertation investigates the effect of government corruption on political participation. Specifically, it assesses the impact of political corruption prosecutions on self-reported voting and political trust in the United States. Using official statistics from the Department of Justice's (DOJ) Public Integrity Section (PIN), and survey data from the American National Election Study (NES), I examine how corruption affects Americans voting habits and levels of political trust. I also explore how the effects of certain demographic characteristics on voting and trust might be influenced by the level of corruption in the state.

In this chapter, I introduce key concepts to keep in mind before diving into the dissertation proper. First, I justify the use of the United States as a particularly important and useful country to analyze. There are methodological and theoretical reasons to focus on corruption in the United States, beyond simple convenience. Next, I argue for the importance of understanding corruption as a potential predictor of political engagement and trust, laying the groundwork for the deeper review of the literature ahead. I then introduce the importance of using multilevel modeling (MLM) to understand the effect state-membership might have on

individual decisions to vote or trust. Next, I introduce the concept of cross-level interaction effects, which are useful in MLM to determine whether the effects of certain demographic characteristics on voting vary as a function of state-level corruption. Specifically, I present the idea that the effects of income, education, race, and religion are affected by state-level corruption. Finally, I close by presenting my research questions and outlining the rest of the dissertation.

CORRUPTION IN A U.S. CONTEXT

Why focus on the United States? It seems like more devastating corruption could be found internationally, where developing nations can be run by warlords or where corruption by government officials is so expected by the population that bribing schoolteachers is one of the only ways for your child to pass a class (della Porta and Vanucci 2012; Vogl 2012). While corruption is indeed more difficult to spot in the U.S., there are some sociologically interesting motivations for understanding corruption here.

First, the United States has a long history of a political body built around ideas of a government with as little corruption as possible. This historical context provides a unique location for researchers to study the impact of corruption in a country so adamantly built on anti-corruption policies. In *Corruption in America: From Benjamin Franklin's Snuff Box to Citizens United,* Zephyr Teachout (2014) outlines an American history where the country's forefathers focused a good deal of their legal acumen on developing laws that limited not only corruption by government officials, but also the perception of such corruption by citizens. Defining said corruption as "excessive private interests influencing the exercise of public power" (Teachout 2014:38), the founding fathers paid special attention to disallow any private gifts for political players in the country's early history. Over time, citizens became less concerned with corruption,

and, by the mid-1800s, corruption was relatively rampant in the U.S., particularly in cities: "[n]ineteenth-century American urban governments vastly overpaid for basic services such as street cleaning and construction in exchange for kickbacks given to elected officials, and these governments gave away public services for nominal official fees and healthy bribes" (Glaeser and Goldin 2004:1). After a series of piece-meal stopgaps and judicial rulings around the turn of the twentieth century, the government introduced the PIN in the mid-1970s, a division of the DOJ committed to enforcing anti-corruption laws and aiding State Attorneys in finding, prosecuting, and convicting corrupt government employees (Public Integrity Section 1978). As this brief account shows, there is a healthy amount of history in the U.S. to suggest that the country, at least on the books, does not tolerate corruption.

Second, the U.S. allows scholars to study the effects of corruption on individuals, while accounting for state-level differences in their geographic locations. Whereas a major difficulty of comparative international research on corruption is that corruption is defined differently by different countries, and measures of corruption primarily rely on asking experts in each country their perceptions, the DOJ and PIN enforce federal anti-corruption laws that are the same for each state. While states differ in the amount of resources state attorneys have in enforcing anti-corruption laws (Boylan and Long 2003), the fact that all corruption prosecutions documented by one agency, using the same laws for each state, allows for cross-state comparison without worry of laws differing by state. According to Alt and Lassen (2003:342),

> A value of using the states is that we can hold some legal institutions constant while also avoiding many unobservable differences in culture and institutions that exist across countries. However, there are enough cases and sufficient heterogeneity in institutions and socioeconomic conditions to allow tests of leading conjectures and explanations of corruption.

In other words, if people in high-corruption Louisiana trust the government significantly less than people in low-corruption Colorado, it's not due to differences in corruption's measurement.

I argue that the United States provides a particularly rich and well-situated area to analyze the outcomes of political corruption. While much corruption research examines its effect on lesser economically developed countries, which is an entirely reasonable and important area of examination, this leaves an opening in the literature for an intensive study of the United States. Not for nothing, with the relatively new creation of anti-corruption think tanks in New York, and a number of policies enacted in Washington D.C. that emphasize the importance of tamping down corruption, this research comes at a time when policy makers are particularly open to research on corruption. Most importantly, however, the U.S. is an ideal place to test certain theories about political corruption.

THE EFFECT OF CORRUPTION ON POLITICAL PARTICIPATION

Voter turnout and government trust are two of the best indicators for a citizen's overall political participation and their broader perceptions of the government used today. Governments and those interested in encouraging regular citizens to participate in politics have a vested interest in understanding the mechanisms by which citizens are more or less likely to vote or trust the government. While I go into detail about other factors associated with increased voter turnout and political corruption in Chapter 2, I briefly lay the groundwork for how corruption affects those phenomena here.

Political corruption is defined in many ways, with the majority of definitions suggesting that corruption is an action undertaken by an elected official or government representative for the private benefit of that official at the cost of the public good (Amundsen 1999; Treisman 2007). Others specify and nuance this baseline definition. For instance, Brooks (1909:17) states that corrupt officials "must know the better and choose the worse" saying that government inefficiency is not enough to define a government as corrupt; Maxwell and Winters (2005)

emphasize the *political* nature of such corruption, establishing that such acts are corrupt if one only could have committed those acts because of his or her public position; Treisman (2000) points out that corruption can also benefit a political players' family or friends. All told, political corruption entails hurting the many for the benefit of the politically connected few. In the United States, the best available measure of state-wide corruption is that established by the Public Integrity Section (PIN) of the Department of Justice (DOJ), who define corruption as bribery, conflict of interest, election fraud, or other offenses that are committed by employees or officers of the government at the local, state, or federal level.

When political players benefit themselves over those they are elected or hired to serve, citizens have ample opportunity to hear about this type of political activity – newspapers, investigative journalists, and watchdog groups have a longstanding history of reporting on these issues (Glaeser and Goldin 2004), and overall levels of corruption have been shown to be well understood by citizens (Vogl 2012). Thus, a more corrupt government could produce apathy and disengagement in its citizenry, which could lead to people not engaging in political activities like voting. On the other hand, these citizens might be more likely to vote to "throw the bums out," and vote more often, with a sense of moral outrage. Concerning political trust, political corruption could erode public confidence in politics more generally, and thus decrease citizen trust in the government – or there might be no effect of corruption on a citizen's political trust.

If corruption acts as an indicator of government malfeasance and potential malevolence, and information about that corruption is relatively well-understood by citizens, at least in a general sense, we have reason to believe that this affects citizens. However, it remains to be seen how U.S. citizens might adjust their political actions and attitudes in this context.

A MULTILEVEL PROBLEM

So far, I have argued that corruption is particularly salient and imminently researchable in a U.S. context, corruption might impact political trust and voter turnout. Next, I briefly introduce my primary analytic strategy: multilevel modeling.

As evidenced by the numerous studies on the effects of corruption, there are many ways to go about investigating this topic, both qualitatively and quantitatively. For this dissertation, I use multilevel modeling (MLM) (Bickel 2007; Rabe-Hesketh and Skrondal 2006, 2008; Snijders and Bosker 1999). While I go into more detail about the specifics of MLM in Chapter 3, what is important to understand at this point is that this method allows us to examine relationships between independent and dependent variables, while taking into account the effects of the larger group of which the individuals being examined are a part. For this dissertation, that larger group is the state. Unlike basic regression, MLM takes into account the nested nature of individuals in states and the fact that some of what citizens do depends on which state they are from. In other words, each individual nested within the same state is likely to be influenced similarly by processes and attributes that are characteristics of that state (Hayes 2006). MLM moves beyond presenting information about the relationships between level-1 factors (i.e., individual-level variables) and level-1 dependent variables, and does not incorrectly treat variables that influence whole swaths of people (level-2, e.g. state-level characteristics like average household income) as level-1 characteristics, because it considers the influence of state context when determining the impacts of level-1 variables (Hayes 2006). Using MLM is especially important for this dissertation because the state corruption rate is in fact a level-2 variable, that is, an attribute of states and not individuals, though previous research often does not distinguish it as such.

More specifically, I use multilevel logistic regression (in the analysis of voting) and multilevel linear regression (in the analysis of government trust) to show the impact of corruption on my dependent variables: self-reported voting and political trust. I also present the predicted probabilities of voting for the first analysis, to give a more interpretable measure of the effects of my independent variables on self-reported voting, controlling for other factors.

THE IMPACT OF CORRUPTION ON THE EFFECTS OF EDUCATION, INCOME, RACE, AND RELIGION

Another contribution of this research is that I examine how voting and trust for U.S. citizens might be affected by certain demographic characteristics differently in states that are more or less corrupt. Specifically, I test for cross-level interaction effects to determine the extent to which the state corruption rate affects the effects that income, education, race, and religion have on self-reported voting and trust (Aguinnis, Gottfredson and Culpepper 2013).

My desire to better understand how certain demographic characteristics influence political participation stems from the work of Leighley and Nagler (2014) who, in *Who Votes Now? Demographics, Issues*, *Inequality and Turnout in the United States*, discuss a core issue with one form of political participation: voter turnout. Specifically, while our society has taken great steps (in some places) to encourage voting, there is a problem. While we focus on encouraging voting as a whole, the measures that we have implemented may have fostered continued voter inequality in *who* turns out to vote. Specifically, the poor are less likely to vote compared to the wealthy, even when efforts such as no-fault absentee ballots are implemented at the state-level. Instead, those who are already likely to vote (i.e., the more fiscally conservative, wealthier, and more educated) are more likely to take advantage of the measures used to encourage everybody to vote (Leighley and Nagler 2014).

The central thesis Leighley and Nagler (2014) posit is that those who are more invested in the government and the economic system (and, thus, the status quo) are more likely to participate politically. In this dissertation, I take this idea and see whether the impacts of income, education, race, and religion on self-reported voting and political trust are impacted by the amount of corruption in the state, in cross-level interaction effects. From the literature, there are several reasons to expect that background characteristics might impact citizens differently based on state-level corruption. For instance, citizens with less education might already feel like they do not have access to the ways to enact meaningful political change, which could explain their low voter turnout rates (Leighley and Nagler 2014). In more corrupt states, having a high school education might lead to *more* disengagement from the political process than being a high school graduate would in less corrupt states. As another example, due to the structural racism of the U.S. (Omi and Winant 1994), and the fact that African Americans might already feel like the political system is not set up to benefit them (Musgrove 2012), the influence of race on self-reported voting and political trust might be different in more or less corrupt states. Finally, certain religious affiliations focus on what comes next in the world, and are less engaged with the political process (Bean 2014; Sherkat 2014). Might differential levels of state political corruption impact the effect of religious affiliation for these citizens as well?

RESEARCH QUESTIONS AND OUTLINE

I now turn to the questions I grapple with in this dissertation. Specifically, I ask the following:

- What is the effect of political corruption prosecution rates on the likelihood of an individual to report having voted?
- What is the effect of political corruption prosecution rates on a citizen's trust in the government?

- Does the rate of corruption prosecutions in a state change the way that certain socio-demographic variables affect self-reported voting and trust? In other words, are the effects of income, education, race or religion on self-reported voting and political trust in any way influenced by the amount of corruption in a state?

More specifically, my outline for this dissertation is as follows. First, in this chapter, I have broadly sketched what I will examine in the rest of this dissertation and provided my substantive research questions. In Chapter 2, I discuss the relevant literature on political corruption, voter turnout, and government trust. I also detail the theories on how corruption might impact political participation and trust, and provide hypotheses that guide the rest of the dissertation. In Chapter 3, I further introduce my analytical strategy, presenting my hypotheses, detailing my dataset, and discussing my methods. Chapter 4 is my analysis of the effect of corruption on voting, and Chapter 5 is my analysis of the effect of corruption on government trust. I conclude the dissertation in Chapter 6 with implications for both scholarship and social change.

CHAPTER 2

REVIEW OF THE LITERATURE

In this chapter, I discuss the current literature on political corruption, voter turnout, and citizen trust in government. First, I define political corruption and detail its causes and consequences, to provide a backdrop from which to understand the broader context within which this research fits. Second, I discuss voter turnout and scholars' work on the effect of political corruption on that turnout. Third, I discuss political trust, and how corruption might impact that trust.

For sections two and three, I introduce the literature on what affects voter turnout and political trust in terms of cultural (i.e., individual-level) and institutional (i.e., macro-level) factors. I then introduce the theoretical perspectives that argue why we might see certain relationships between voting and political corruption, and government trust and political corruption. After each of these introductions to the theoretical paradigms, I introduce my primary hypotheses, which are that corruption will have a negative effect on self-reported voting and political trust, in their respective sections.

Also in sections two and three, I suggest that we should examine how state-level corruption affects the influence that certain demographic characteristics – specifically, income, education, race and religion – have on self-reported voting and political trust. After introducing the rationale for testing these cross-level interactions in my analysis, I provide a hypothesis on how the effect of each of these characteristics on voting and political trust might vary as a function of state-level corruption.

SECTION I: POLITICAL CORRUPTION

Political scientists, and to a lesser degree sociologists, have been interested in the causes and consequences of political corruption since, seemingly, the onset of either discipline. Here, I discuss the work done to define corruption, the theoretical causes of corruption, and the real-world consequences of corruption.

Defining Corruption

Many have spent a good portion of their scholarly careers defining corruption, only to see others' interpretations disagree. Notably, the definition of corruption used by scholars often differs depending on their particular research question, or, perhaps more accurately, their data. Nevertheless, there are some common threads.

Most often, corruption is defined as the misuse of public office for private gain (Treisman 2007). In one early analysis of U.S. corruption, Brooks (1909) defines a corrupt act as when an individual, alone or as part of a group, *intentionally* fails to perform his or her entrusted duty with a motive of gaining some sort of advantage: "[t]he corrupt official must know the better and choose the worse; the inefficient does not know any better" (Brooks 1909:17). Others suggest that corruption occurs when officials misuse their positions to aid one's political party, family or friends (Treisman 2000, 2007); that corruption entails committing acts that one only could have committed due to one's political position (Maxwell and Winters 2005); or that corrupt acts must be clandestine, otherwise corrupt acts are merely "new approaches" to governance (Green and Ward 2004). Whichever the definition, "[c]orruption is not a single event, but a continuum, perpetrated day in and day out against citizens by crooked politicians and civil servants who enjoy positions of power" (Vogl 2012:12).

While scholars often disagree on what corruption is theoretically, for the purposes of this work I operationalize corruption in the United States as defined by the Department of Justice (DOJ). In 1976, the U.S. government formed the Public Integrity Section (PIN) as part of the DOJ, with the stated aim to consolidate responsibility of Federal efforts against all levels of official corruption (PIN 1978). The PIN defines corruption as "bribery, conflicts of interests and miscellaneous offenses" (PIN 1978:1) of government officials, as well as when election laws are not followed. While there is variability in the ability of DOJ officials to enforce laws against many types of corruption (Boylan and Long 2003) and this definition changes in substantive ways over time, this is the best available measure.

Measuring Corruption

Just as definitions of corruption differ by scholarly focus, researchers' operationalizations of corruption vary by analytic strategy. For instance, international quantitative work often employs the Corruption Perception Index (CPI), created by Transparency International (TI) (Bader et al. 2013). To arrive at each country's corruption score, TI surveys experts from both economic and educational spheres (i.e., people who work for large businesses as well as those who work for universities and think tanks) and asks them to rate their country on several questions related to corruption. For smaller studies of specific countries, researchers often survey citizens about their perceptions of corruption, and assess the corruptness of a location by aggregating responses (Bader et al. 2013). Relatively little comparative international work is done by triangulating survey responses with field-work or experimental studies (Bader et al. 2013). Thus, survey responses are often the gold standard in international corruption research.

In the U.S., most quantitative analysts use as their measure of corruption the annual statistics presented by the Department of Justice's (DOJ) Public Integrity Section (PIN). These

reports enumerate the number of corruption prosecutions by state for each year, which were either investigated by the PIN or were reported to the PIN by State Attorneys (Cordis and Milyo 2013). With a stated aim to consolidate responsibility of Federal efforts against all levels of official corruption, the PIN has a particular focus on "bribery, conflicts of interests and miscellaneous offenses in offices that are committed by officers or employees of the three branches of the Federal government" (Public Integrity Section 1978:1). As such, these data include a wide range of corrupt acts, and are best used as general indicators of a level of corruption in a state (Schlesinger and Meier 2002).

While these statistics are widely-used (e.g., Flavin and Ledet 2013; Johnston 1983; Meier and Holbrook 1992; Schlesinger and Meier 2002), they are not without criticisms. First, they only indicate where corrupt individuals have been found and prosecuted, failing to enumerate corruption that has remained hidden from the public eye. Of course, all official measures of crime miss out on unobserved crimes (Mosher, Miethe and Phillips 2002), though corruption is most likely harder to officially document than street crime because many illegal actors are in positions of power, and can better hide their misdeeds from the public eye compared to other lawbreakers (Harris 2003; Rose-Ackerman 1978). Furthermore, these measures do not include activities that the average American *might* consider to be corruption but are not legally codified as such, like if a lobbyist were to strongly influence a politician's perspective on a particular issue (Teachout 2014).

Nevertheless, there are some unique benefits of using these reports. First, the PIN provides a roughly standardized measure of corruption; corrupt individuals are ultimately prosecuted based on national, rather than state, laws (Flavin and Ledet 2013; Musgrove 2012). This provides scholars the ability to compare corruption across states. Second, PIN reports in the

early 1990s positively correlated with Boylan and Long's (2003) measure of statehouse reporters' perceptions of political corruption, showing that experts view the most "officially corrupt" states as the most corrupt in their professional opinions. Third, these data have been used in previous research and have produced results in line with early theoretical determinants of political corruption (Johnston 1983; Meier and Holbrook 1992; Nice 1983). Thus, while this measure undoubtedly underestimates what goes on "behind closed doors" (Johnston 1983), most scholars agree with Schlesinger and Meier (2002:642) in that "the level of prosecutions in the United States is a reasonably good surrogate for the level of official corruption in the state."

In sum, definitions of corruption differ, as do operationalizations of corruption in quantitative analyses. In the U.S., official statistics measuring corruption prosecutions are arguably adequate indicators of the overall level of corruption in a state. With this rough definition and operationalization, the question stands: what causes corruption?

Causes of Corruption

Before reviewing the literature on the consequences of corruption, it is important to analyze how corruption comes to be. This provides context for the analysis that follows because it details which attributes of states and political leaders are closely aligned with corrupt politics. Understanding the causes of corruption also allows us to better untangle the factors that might influence both state-level corruption and individual political participation. To that end, there are four relatively well-established paradigms that speak to the determinants of corruption. First, political corruption is tied to the history and culture of a geographic area. Second, corruption arises when institutional systems provide the opportunity for such corruption to occur. Third, corruption often occurs within established personal networks. Fourth, corruption is often accompanied by an uninformed or disengaged citizenry. I deal with each of these in turn.

History of Corruption. To begin, perhaps the most tried and true tenet of previous research is that political corruption is closely linked to a place's history (Heywood and Rose 2014). For instance, bribery and backroom deals in countries like China, states like Louisiana, and cities such as Chicago, are seen as normal, business-as-usual for politicians (Lessoff and Connolly 2013; Meier and Holbrook 1992; Rothstein and Uslaner 2005).

Political corruption is particularly enduring – or "sticky" (Rothstein and Uslaner 2005) – because in corrupt places acting corruptly makes sense to a rational actor. Such an actor would bribe an official, for instance, if he or she thought that was the expected course for a particular agreement to occur (della Porta 2000; Bardhan 1997; Rothstein 2013). It would not make much sense to try to influence a politician with legitimate means if you thought your competition were doing so with illegitimate means. And, if that competition believes the same thing about you, we can see how vicious cycles of corruption form. This is particularly true in underdeveloped countries or nations undergoing systemic change in their government (say, changing from a communist regime to a capitalist one) (Kostadinova 2009). Internationally, certain countries, especially those that are less developed economically (Schlesinger and Meier 2002), often suffer enduring legacies and likely futures of corruption (Vogl 2012).

The good news is that the amount of political corruption *can* change in a society, though not as quickly as some might desire or, indeed, might need to stay alive in particularly corrupt, genocidal regimes (Vogl 2012). Change often comes from sweeping good governance measures and increasing modernization (Glaeser and Goldin 2004). Thus, a society that is more advanced economically is often less corrupt (Andvig et al. 2000; Glaeser and Saks 2004; Schlesinger and Meier 2002; Treisman 2000, 2007). However, modernization is not a panacea; the most urban parts of America still face higher than average levels of corruption (Schlesinger and Meier 2002)

– perhaps due to their legacies of corruption from the political boss and political machine days (Meier and Holbrook 1992).

To wrap up, while reform attempts have succeeded in some cases, at least in the short term (Glaeser and Goldin 2004), the stickiness of corruption is hard to get past (Rothstein and Uslaner 2005; Treisman 2007). Unfortunately for historically corrupt states and countries, "[t]he distant past appears as important as — or more important than — current policy" (Treisman 2000:401).

Systemic Opportunity. Second, corruption is more likely to occur in places where governments are held less accountable, either from the government itself (De Graaf and Huberts 2008), or from external sources, like watchdog groups or a free press with access to fully cover transparent government activities (Green and Ward 2004; Smith 2010). Simply put, when nobody oversees government employees, or when there are not specific checks in place to hinder corruption, opportunities to engage in illegal activity abound, and are often taken (Ceresola 2015; Johnston 2005; Perrow 1984).

Empirical findings suggest the importance of transparency in the government to reduce illegal activities (Alt and Lassen 2003). For instance, the more people that work in a government, the higher the likelihood that corruption will occur (Winters 2012). This is clearly an artifact of the size of the government, but also indicates that there is more opportunity for corruption to occur when there is less likelihood of being caught (Goel and Nelson 2011; Meier and Holbrook 1992; Schlesinger and Meier 2002; Winters 2012). State governments and state legislatures that are run by different parties (e.g., a Republican governor and a Democratic state legislature) are also linked to higher levels of corruption, which indicates less government oversight, as the two groups maintain an air of secrecy concerning their decision-making processes (Alt and Lassen

2008). Finally, corruption rates are higher in states where judges are appointed by governors, instead of elected: another area where citizens and watchdog groups are given less information about their political process (Alt and Lassen 2008). Essentially, large government size, shadowy operations between political parties, and appointed judges reflect a more closed-off government, where information is less readily available.

Because corruption occurs when the watchers are not on guard, systemic opportunity for corruption exists in any type of governmental configuration imaginable: from dictatorships to autocracies to constitutional states (Von Alemann 2004). However, some evidence suggests that parliamentary and plurality election governments are perceived as less corrupt by their citizens (Treisman 2007). Of course, entire government regimes can be classified as corrupt by certain moral standards (Vogl 2012), but, overall, opacity, rather than a particular type of government, can lead to corruption. Unfortunately, the diminishing strictness of U.S. anti-corruption laws in recent years, as well as the great increase in the spending power and influence of corporate leaders in American politics, paves the way for more of this behind-closed-doors corruption to occur into the next century (Heywood 1997; Mills 1956; Teachout 2014).

Personal Networks. Third, corruption often occurs through established personal networks between politicians and external (or internal) influencers. Images of strangers leaving wads of cash for politicians to vote a certain way or to sell a certain lot of land to the highest bidder seldom occur in reality (De Graaf and Huberts 2008). Instead, politicians who have familial or friendly ties to individuals with a vested interest in changing public policy or buying a public good at an unfair price have more opportunity to engage in corruption than politicians with no such ties.

For example, in their qualitative examination of ten Dutch individuals convicted of corruption, De Graaf and Huberts (2008:646) found that "[i]n nine out of the 10 cases, there was a long, institutionalized relationship between the briber and corrupt official." For their sample, and in many other cases, corruption occurs when politicians have close, personal ties with their bribers or when officials do illegal things for those in their families or friend groups (Ceresola 2015). Quantitative research backs this finding as well. For instance, countries with higher levels of violent and property crime have higher rates of corruption (Meier and Holbrook 1992; Pratt and Cullen 2005; Schlesinger and Meier 2002), and Schlesinger and Meier (2002) interpret this finding by suggesting that political players in more crime-ridden states come into contact with people who are involved in criminal activity more than political players in states with less criminal activity, pointing to the effect of networks on corruption.

Furthermore, historically corrupt governments have had close ties with organized crime. For instance, in modern-day Italy, known members of the Mafioso have sat on governmental boards (della Porta and Vannuci 2009, 2012)! Della Porta and Vanucci (2009) found that the most corrupt politicians had long lasting relationships with individuals involved in private businesses as well as individuals involved in organized crime. In the early history of the U.S., the influence of networks was so well understood by the founding fathers that special care was taken to document (and in many cases reject) any gifts, services, or goods offered by *any* individual to politicians – even if these individuals were family members or friends with no interest in political influence (Teachout 2014). Times, however, have changed.

Notably, corruption is more prevalent in certain industry-government relationships than others. Extractive industries, construction companies, and large-scale agricultural organizations are businesses that have deep pockets and resources from which to potentially bribe a particular

politician to follow a certain line (Woods 2008). For instance, in an analysis of a Boston, Massachusetts road construction project called "The Big Dig", Smith (2010) found that officials used favoritism in contracting out the roadwork, because one of the companies had familial ties with a person organizing the construction. Unfortunately, construction companies exerting such influence can have particularly devastating consequences when these companies construct housing projects and large buildings, especially in the event of natural disasters (Green 2005; Green and Ward 2004). These relationships can endure over decades. As Mills (1956) points out, rich and powerful families have connections unknown to the general population, providing a rich breeding ground for these relationships to foster, which have continued into present-day (Domhoff 2013; Domhoff and Dye 1987).

Another area to see the strength of personal ties in fostering corruption is to investigate what happens when outsiders enter the political realm. In a natural quasi-experiment we can see what happens to countries' levels of corruption with the introduction of more women to positions of political power, which started in Western countries in the 1960s (Jackman 1987; Moore 1987). The argument here is not that women have higher moral standards or are innately more virtuous than their male political counterparts (Goetz 2007), but that their entrance into the political system disrupts relationships corruptors had with their previous political allies. Newly elected female officials often have less experience with, and access to, the public sphere, and "the ways women are recruited (or not) to the leadership and rank-and-file of political parties restrict their opportunities for engaging in corrupt activities" (Goetz 2007:99).

In a cross-sectional analysis of over 100 countries, Dollar, Fisman and Gatti (2001) found that countries with higher rates of female representation in government rank lower on various corruption indices. They conclude that encouraging more women to enter into the political realm

is not only valuable for gender equality, but might have a latent consequence of reducing corruption (Dollar, Fisman and Gatti 2001). However, not all agree with the idea that women impede corruption. Sung (2003, 2012) suggests that the negative effect women have on corruption vanishes when measures are included that tap into whether a country is a liberal democracy. Nevertheless, others suggest that women indeed could have a negative influence on corruption, especially in small governments (Goetz 2007; Moore 1987).

All told, while historical precedence and systemic opportunities are decidedly influencers of corruption, long-established personal networks play an important role in the manifestation of corruption in the real world as well.

Electoral Engagement. Fourth, corruption is often accompanied by an uninformed or politically disengaged electorate. In a review of ten years of cross-national research, Treisman (2007) reported that political bodies with more corruption have less citizen input, in terms of voting and political protests or activism. This is problematic, because citizen input can directly affect corruption policies in two ways: citizens' demanding reform from their government and voting in anti-corruption candidates during elections.

First, when citizens demand reform loudly and vehemently, they often get it. Historical evidence suggests that politicians engage in fewer corrupt practices when they sense their voting public is paying particular attention to their actions (Glaeser and Goldin 2004; Teachout 2014; Wallis 2014). In the United States, for example, Glaeser and Goldin (2004) looked at newspaper reports to assess any major changes in political corruption reporting over time. They found that reports on corruption declined from the year 1870 to about 1920. This decline followed a number of articles in the 1850s-60s that reported on public outcry against corruption and the introduction of sweeping reforms by the Federal government. They conclude that reforms of this era may

have accomplished their goals, noting that, because corruption stories still sold papers, the only reason for newspapers to report on corruption less would be if there really were less of it to report. Furthermore, both Teachout (2014) and Wallis (2014) detail an American history where citizens are very concerned with corruption (usually because they're seeing more of it), and take concrete measures to stamp it out (usually by calling for legal reforms). The problem then is that afterwards citizens might feel content with the reforms and stop paying attention to corruption, which usually leads to another rise in corruption, thus repeating the cycle anew.

Another way that citizens exert influence on challenging corruption is by voting out politicians they feel do not serve their best interest, and voting in those they believe will serve them better. Put simply, knowledgeable citizens can follow the careers of politicians and see if they're following the goals that they had originally laid out to the public (Lassen 2005; Rose-Ackerman 1978). If not, citizens could choose to not re-elect them. According to Rose-Ackerman (1978:58), "[i]t seems that the best checks on corruption are a well-informed and issue-oriented electorate and a political system that routinely produces challengers ready to take advantage of lapses by incumbents." On the other hand, if politicians feel that they have a safe seat they will feel less pressure to govern fairly (Heywood 1997). Semi-relatedly, the larger the citizen population, the more common corruption is in the states (Meir and Holbrook 1992), which might reflect a representative's lack of a connection to the majority of those he or she is elected to serve (Maxwell and Winters 2005). In other words, if politicians feel like most people are not paying attention, they'll be more likely to engage in corrupt activities.

Historians have documented downturns in corruption corresponding with increased citizen input and outcry, particularly at the ballot box (e.g., Glaeser and Goldin 2004). However, the question then is whether corruption decreased because citizens became more engaged, or

whether citizens became more engaged when they saw fewer corrupt politicians? Whichever, more corrupt regimes are not overseen by watchdog groups, nor the citizens that they serve.

To conclude, years of research state that corruption is most likely to occur 1) where it has occurred before, 2) when government agents are not held to any real oversight, 3) when elected officials and government employees have close, personal relationships with potentially corrupting influences, and 4) when the general public does not pay attention to politics. Now that we know some of its causes, we can ask, what does corruption *do*?

Impacts of Corruption

Contrary to the general view of scholar and political watchdog groups today, when scholars first assessed the consequences of corruption, many saw it as at least somewhat functional. These functionalists viewed corruption as merely 'greasing the wheels' of the political process, where actors could cut through political backlog and operate in more creative ways (as detailed in della Porta and Vanucci 2009; Hodgson and Jiang 2007; Huntington 1968; White, Jr. 2001). For example, political bosses, vying for citizens to vote a certain way, would fulfill needs in their communities like filling in potholes, or ensuring garbage would be picked up, that would otherwise be left unfulfilled due to government lag or disinterest (Merton 1968). In this way, corruption was seen as a positive in providing a benefit to poor ethnic whites and minorities, as long as they were sure to pay off a certain politician or pledge to vote a certain way (Lessoff and Connolly 2013; Merton 1968).

This halcyon view is no longer. It is now well-acknowledged that any potential positives of corruption are vastly outweighed by the negatives, *particularly* for those who are the least well off to begin with (Alatas 1990; Andvig et al. 2000; Chang and Chu 2006; Kaufmann and Wei 1999; Rose-Ackerman 1978, 1996; Von Alemann 2004). On a personal level, individuals in

more corrupt states are less likely to trust other citizens in those states (Richey 2010). On a more instrumental level, corruption can lead to a loss of citizen tax revenue, when money is given out unfairly or embezzled by government agents (Andvig et al. 2000). Corruption can lead to a loss of land or property; government agents can look the other way on building codes that collapse (Green 2005; Green and Ward 2004), or their favoratism can lead to unqualified construction companies tearing up public lands (Smith 2010). Also, because citizens in the most corrupt regimes often experience poverty, poor health and a lack of vital public services (Vogl 2012), it is not hyperpoblic to assert that corruption could lead to a loss of life (Green 2005; Green and Ward 2004). For example, in a study of 344 earthquakes from 1975 – 2003, Escaleras, Anabarci and Register (2007), found that more people die in earthquakes in more corrupt countries - even though all countries in their sample reported complying with international building regulations. They concluded that more corrupt countries' officials look the other way on these codes, and loss of citizen life in natural disasters is the consequence (Escaleras, Anabarci, and Register 2007).

In fact, even the "benefit" of an individual moving swiftly through bureaucracy can do lasting damage to a political system. Specifically, if nobody plays by the rules of the government of which they are apart, there is little pressure for the government to change its systems that led to that rule-breaking in the first place (Rose-Ackerman 1996). If individuals continue to bribe and be bribed, and things get done in that system, officials have little motivation to enact sweeping reform that would provide long-term positive impacts to all citizens. Furthermore, if the people perceive the government to be more corrupt, they often have less confidence in the government to do anything right officially (Caillier 2010). Thus, corruption harms individuals who are not corrupt and who try to enact some form of change or participate politically (because corrupt politicians give preference to citizens who actively bribe them, for instance), it gives

citizens a negative view of the government of which they are a part, and it leads to more long-term inefficiencies in the government.

Finally, corruption dramatically harms a country's economic well-being at the macro-level. International non-profit organizations are less likely to give money to countries that rank highly on corruption scales, no matter the need of the citizens in that country (Campos, Lien, and Pradhan 1999; Rose-Ackerman 1996). Relatedly Kaufmann and Wei (1999) found that industries that report it is more common to pay irregular, 'additional' payments to accomplish goals in various countries also report higher operating costs overall. Even more troubling in our globalized world, "elites in developed countries are exporters of corruption to poorer contexts – for example, where Western corporations obtain public contracts, pollute or get access to natural resources by paying bribes to local elites" (della Porta and Vanucci 2012:134). Thus, "corruption touches everybody" (Caiden 2001:229) through globalization; one country's bad practices influence other countries and the stickiness of corruption can attach to other nations.

By necessity, this overview of the impacts of corruption is brief and there are many more studies that assess corruption's impact in a variety of locales and under other specifications, each providing some insight into the potentially devastating consequences of unchecked political corruption. Nevertheless, scholars have shown that corruption leads to negative consequences for citizens, government inefficiency, and economic duress and sometimes catastrophe. Having established a rough definition of corruption, its causes, and some of its consequences, I now turn to the literature that examines voting and political trust.

SECTION II: VOTER TURNOUT

Political scientists, sociologists, and community activists have long been interested in the determinants of voting, with the tacit assertion that voting is a key way for average citizens to

engage with the democratic process (Alatas 1990). The considerable attention paid to voter turnout speaks to the importance that we place on this democratic process (Engstrom 2012). Since the U.S. is a republic with no real room for the majority of citizens to directly speak to political players, voting is one of the few opportunities where citizens as a whole are able to interact with the political sphere to achieve some measurable end. A robust electorate, in other words, is a manifestation of a healthy relationship between officials and those they serve.

If this is true, then the United States is in trouble. Not only does the U.S. boast lower rates of voting than most other industrialized countries (Geys 2006), the specter of decreasing voter turnout looms large in public discourse[1] (e.g., Engstrom 2012; McDonald 2003; McDonald and Popkin 2001; Putnam 2000). By almost all measures, the U.S. is unique in industrialized nations in that voter turnout is significantly lower than other countries, and shows no real sign of improving despite structural reforms.

Tangibly, if we hope to encourage voter turnout as a nation, it is necessary to understand the factors that influence it. I pay particular attention to the idea that corruption might influence voting, but I first situate this research in that which has come before. Thus, in the following section, I discuss voter turnout from two theoretical perspectives. The first is that cultural, or individual-level, characteristics are associated with filling out a ballot, in that individuals are influenced to vote because of some sense of socialized duty. The second takes the standpoint that institutional, or state-level, characteristics affect voting: voting is a manifestation of how easy or difficult it is to vote in a particular election. To be clear, the factors defined below that influence voter turnout could often be interpreted as belonging to either perspective; however, this distinction is a useful one for analyzing individual actions through a theoretical lens.

Cultural/Individual Factors Associated with Voting

By examining demographic characteristics of voters, we can better understand who votes and why (Rolfe 2012). Citizens decide whether to vote based on the way they were socialized into the political process (McClurg 2003), the social groups of which they are a part (Gerber, Greene and Larimer 2008; Rolfe 2012), how rational they think it is to do so (Birch 2010), and many other individual-level factors.

While it is easy to say that certain individual-level characteristics influence voting, it is more difficult to assert which characteristics do so. Despite years of research, a list of variables that serve as "usual suspects" does not exist (Smets and Van Ham 2013). The most commonly included variables in tests of individual-level characteristics are education, age, gender, race, income, marital status, party identification and political interest, though there is substantial variation in scholars' decisions to include them in their analyses (Leighley and Nagler 2014; Smets and Van Ham 2013). For instance, once the average American reaches retirement age, they usually have fewer responsibilities to their workplaces and families, and often fill that gap with political participation, including voting (Bhatti, Hansen and Wass 2013; Glenn and Grimes 1968). When tested, this relationship is robust to a variety of specifications, but a measure for age is not included in about twenty-five percent of empirical tests (Smets and Van Ham 2013). Like in much research, the available data greatly affect which particular questions we ask.

Another common finding is that the higher one's education, the more likely that person is to vote (Blais 2000, 2006; Gallego 2010; Leighley and Nagler 2014; Rolfe 2012; Smets and Van Ham 2013). This might be because the more highly educated are also more politically engaged and politically interested; thus, their higher likelihood of voting is a rational expression of their knowledge of and commitment to political issues. On the other hand, Rolfe (2012) argues that

people with higher levels of education are more likely to be a part of a social group made up of voters, and thus vote simply to fall in line with their friends. Finally, Leighley and Nagler (2014) suggest that voters are more highly educated because the U.S. voting system is set up in a way that is easier to participate in for people with higher levels of education. For instance, higher education often leads to jobs with more flexible schedules, so those with such degrees might have more of an ability to get off of work to go to a polling place. Alternatively, those with more education might have learned how to navigate difficult election rules and regulations, as well as have more knowledge on how to receive absentee ballots.

Closely tied with education is one's income, class, or wealth, and another common finding is that the wealthier one is, the more likely that person is to vote (Blais 2006; Jones-Correa and Leal 2001; Leighley and Nagler 2014; Rolfe 2012; Teachout 2014). While Smets and Van Ham (2013) found that in only half of the articles in which income or class was included as a variable did it reach statistical significance[2], the effect of wealth on voting is well-established in theory. Teachout (2014) points out that wealthier folks are more likely to be invested in the political process in general, as their money can actually make them de facto political players of more importance than the average citizen, and that they may feel especially catered to by politicians. Furthermore, political participation in general positively correlates with one's socioeconomic status, and turning out to vote is just one element of that (Jones-Correa and Leal 2001). In the aggregate, Leighley and Nagler (2014:4) report, "[t]he poor are a smaller percentage of voters than they are of the electorate, while the wealthy are a larger proportion of voters than they are of the electorate."

What the positive relationship between age, education, income and voting suggests is that elements of one's life that are associated with responsibility, stability, or higher social status are

associated with higher odds of voting. This holds in other tangible relationships; people who have lived in the same area for longer are more likely to vote (Smets and Van Ham 2013), as are married people (though that effect may be diminishing) (Leighley and Nagler 2014). Both marriage and residential stability (usually tied to homeownership) connote higher social status in mainstream society. Further, these characteristics put people in networks that positively influence voting, like socially stable neighbors or married friends (McClurg 2003; Rolfe 2012). Of course, it might be that people just value being seen as responsible citizens, as opposed to any inherent quality. In a unique field experiment of over 180,000 households in Michigan, Gerber, Green and Larimer (2008) found that individuals who were told that their voting record would be shown to their neighbors were about eight percentage points more likely to vote than citizens who had received no such message. Even if it is only due to social pressure, those with higher social status are more likely to vote than their lower status counterparts (Leighley and Nagler 2014; Smets and Van Ham 2013).

Additionally, previous experience with politics is a notable predictor for future experience. In fact, one of the strongest predictors of whether an individual will vote is whether that individual has voted in the past, as individuals who have been engaged politically are the most likely to do so again (Blais 2006). Furthermore, those who have been exposed to campaign materials, have higher levels of political knowledge, and have higher levels of overall political interest are also more likely to vote (Smets and Van Ham 2013). Citizens are also more likely to vote if they strongly identify with candidates or if a certain political party shares ideological values with them (Berry et al. 1998). These findings may seem self-evident, but it is important to remember that voting is essentially a political activity, and individuals are differently socialized into their relationships with the political realm.

Finally, political activity has been linked to religious affiliations, actions, and beliefs by many scholars of religion and politics (Manza and Wright 2003; Roof and McKinney 1987; Sherkat 2012, 2014; Sherkat and Ellison 1999). Even though religious characteristics are commonly ignored in studies on voting (Manza and Wright 2003; Smets and Van Ham 2013), one's relationship with religion is often a vital contributor to political participation by teaching adherents certain politicized values (Manza and Wright 2003; Sherkat and Ellison 1999). Specifically, "[r]eligious institutions and beliefs can produce distinctive preferences, and religious organizations provide institutional and cognitive support for engaging in political collective action" (Sherkat 2014:145). For instance, people who know the political ideology of their church are more likely to engage in political activities in line with that ideology (Djupe and Grant 2001). Other religious affiliation groups, like sectarian Protestants, might encourage their adherents to disengage from political life in general, like many have done in the homeschooling movement (Rose 1988; Sikkink 1999).

Beyond affiliation, another element of religiosity closely tied to civic engagement more generally is that of church attendance (Chaves and Stephens 2003). Higher rates of church attendance have been linked to higher odds of voting (Chaves and Stephens 2003; Jones-Correa and Leal 2001; Olson and Green 2006). Jones-Correa and Leal (2001) found that, for whites and Latinos, attending church led to higher rates of participating in political activities. Of course, the type of church an individual attends influences political actions, and those that regularly attend certain churches adhere to the values and goals of their churches more than casual churchgoers (Jones-Correa and Leal 2001).

On the other hand, scholars have also tested for plausible relationships only to come up empty-handed. According to Smets and Van Ham (2013:356) some variables found to

consistently have no effect on voting are "gender, race, occupational status and type, citizenship, union membership, trust in institutions, and the closeness of elections." Clearly, this statement suggests some counterintuitive results. For instance, there are theoretical reasons to expect union membership and institutional trust encourage citizen voting and political participation. In a multilevel analysis of 31 countries, Birch (2010) did find that citizens who think their government fairly conducts elections are more likely to vote. More recently, Leighley and Nagler (2014) report a gender gap in U.S. voting, where women vote more often than men. Also, as mentioned above, others state that one of the most important contributors to voting is wealth, something closely tied up with occupation. Finally, it is not uncommon to find, controlling for other factors, black people are more likely to vote than white people, and both are more likely to vote than members of other races (Musgrove 2012; Wilson 2012). What should we make of these discrepancies?

The answer is unclear. While there has been a great deal of work on voter turnout, we still know relatively little about its known determinants (Matsusaka and Palda 2001). While I attempt to provide some guidance on the correlates of turnout in Chapter 4, it is clear that though a great deal of work has been done to investigate the individual-level factors that affect voting, comparatively little can be stated about what we *actually* know.

Structural Factors Associated with Voting

In addition to these individual-level factors, it is important to remember that there are structural characteristics that might encourage or discourage voting. Understanding the context in which people live allows us to better hone in on the impacts of individual-level characteristics, and to know more about the causes of voting or apathy at the state level. In a study of international voter turnout, Jackman (1987) reported that voting is higher in countries with

compulsory voting laws, which can apply to the United States in terms of the difficulty or ease of voting in each state. Furthermore, voting is higher in nations during competitive races and voting is higher when elections are for more powerful political seats (Jackman 1987).

First, while the United States does not require citizens to vote, the closest parallel is that different states make it harder or easier for citizens to register and vote. Thus, scholars and activists argue that if voting were made easier, more people would do it (Blais 2006; Cemenska et al. 2009; Jackman 1987; Powell 1986). Empirical studies suggest that states with laws that make registration dates later and do not require voters to present identification have higher levels of voter turnout than states with more stringent laws (Gallego 2012; Highton 1997). For example, Leighley and Nagler (2014) found that states that allow citizens to register to vote on Election Day, and states with relatively lax absentee-voting requirements (e.g., voters can register for absentee ballots even without geographical or medical constraints) have higher levels of turnout. In one specific instance, in 1992, the average voter registration closing date was three weeks in advance of Election Day (Timpone 1998). However, in states with the registration date closer to Election Day (i.e., where citizens had a longer time frame in which to register), voter turnout was higher.

While these findings suggest the importance of structural reform in encouraging voter turnout, it is important to note that, such reforms' effects are more modest than what some have hoped for; any increase in voter turnout attributable to structural reforms has only been a few percentage points (Brians and Grofman 2001; Leighley and Nagler 2014). Furthermore, these reforms might only reflect a political culture in a given state that is already conducive to encouraging voter turnout. States that adopt more lax registration policies might already have other characteristics that encourage voting, and these lax registration policies are merely a

byproduct of such a governance structure (Engstrom 2012; Fitzgerald 2005). Finally, these reforms might not encourage people with the least amount of social status to come out to vote, as the people who already were more likely to vote might just use those laws to make it easier for themselves (Berinsky 2005; Leighley and Nagler 2014). For example, Brians and Grofman (2001) found that the members of the middle class are impacted the most by election-day registration – not the working class or poor. In short, structural reforms have a small impact, they might not encourage those with the least social status to come out and vote.

Second, Jackman (1987) found that voter turnout was higher during more competitive races. This makes sense, with the idea that people will be more invested in races that are close, and they may feel like it is their civic duty to vote in such an election. Indeed in a historical analysis of Congressional elections from 1840 – 1940, Engstrom (2012) found that competition had a small but modest effect on overall voter turnout in that elections where winners only narrowly beat out runners-up experienced higher overall voter turnout.

Some suggest that politicians might be the causal factor behind this phenomenon. Powell (1986) argues that politicians eager to win seats will acknowledge that the vote will be close, and thus will do their best to encourage their constituents to show up on Election Day. While there is no effect of campaign "get-out-the-vote" type messages on those already planning on voting, there is some evidence of a positive effect on those who had not yet decided (Hillygus and Jackman 2003). In fact, mobilization efforts have been shown to encourage citizens to vote, and campaigns that actively encourage constituents to come out to vote do just that (Holbrook and McClurg 2003). On the other hand, Matsusaka (1993) found that for ballot propositions in California from 1912 - 1990; in the very type of elections that are particularly close, turnout did not increase in particularly close years. This further suggests that the reason for increased turnout

is not closeness of the election per se, but the positive effect of politicians or campaigns encouraging voters. In short, there is reason to question under which circumstances this finding holds.

Third, Jackman (1987) points out that people vote more in elections where there is more power to be held at the end of the election. In the U.S., this is clear, as more vote in presidential elections than in midterm elections (Tolbert, Grummel and Smith 2001). This idea is closely tied to the notion that more competitive races increase voter turnout. Candidates who run for more influential seats will also most likely try to encourage voter turnout for those elections to a greater degree (Hillygus and Jackman 2003; Powell 1986). Finally, presidential campaigns often spend millions encouraging voters; though campaigns in midterm cycles certainly cost more than ever before, they are still no match to the funds it takes to elect a president (Teachout 2014).

In addition to the three broad themes taken from Jackman (1987), there are a number of institutional variables that should be considered in analyses of voter turnout that were investigated more fully after his work. In a cross-sectional analysis of the U.S., Hill and Leighley (1999) found that more racially diverse states experience overall lower voter turnout, a finding which was confirmed by Tolbert, Grummel and Smith (2001) even when controlling for the type of election. Further, voter turnout also has been shown to be lower in areas that are especially economically disadvantaged (Blais 2006), even though economic disadvantage might also be a reason for increased voter turnout for individuals to address their grievances (Radcliff 1992).

In conclusion, just as for individual-level variables, we can point to some relatively well-established findings, but the full picture remains somewhat elusive. Though there has been notable progress in the past sixty-or-so years of research on this subject, there is still much to know about what causes people to vote (or not) (Matsusaka and Palda 1999).

SECTION IIA: VOTING AND POLITICAL CORRUPTION

One understudied area deserving more attention is in examining how political corruption might influence voting. This absence of research is curious, because political corruption symbolizes, in many respects, the exact opposite of what voting symbolizes (Underkuffler 2013). While voting suggests an engaged and participatory public, political corruption symbolizes elected officials engaging in activities that directly remove citizen input and engagement. Assuredly, some have speculated that corrupt governments elicit passive citizens. For instance, Alatas (1990:125) theorized that corruption would produce an apathetic citizenry, and that, "[p]hilosophically, corruption promotes nihilism and cynicism. It is also nurtured by them." Furthermore, he writes, "[t]he habit of negligence combines with indifference and cynicism to become a deep-rooted attitude towards the authorities and life in general" (Alatas 1990:127).

With more U.S. citizens than ever before believing that corruption is common in the U.S. (Wilkes 2015), and with the (still questionable) assertion that voter turnout is on the decline, the question is raised, is there a relationship between corruption and voting? Because research on corruption and voter turnout is relatively scattered, especially research focused in the U.S., there is no current unifying theory about how corruption might affect voting. In fact, there are three.

The first, disengagement theory, suggests that corruption will decrease voter turnout because citizens will become less engaged with their government, per Alatas' (1990) thought. The second, voter mobilization theory, suggests that corruption will increase voter turnout, as citizens turn to the polls to oust corrupt officials. Third, voter acquisition theory, similarly suggests corruption increases voter turnout, but this time because corrupt politicians will work harder to ensure their constituents vote, because they value their potential seats more than seats with only legitimate authority. In the sections to follow, I present each of these theories in turn,

and then provide my hypothesis for how corruption will affect self-reported voting in the U.S., which aligns with the first theory presented, disengagement theory.

Disengagement Theory

Disengagement theory reflects Alatas' (1990) assertion that corruption produces an indifferent, nihilistic, and cynical citizenry and reflects the commonsensical idea that citizens will disengage from their governing bodies when they feel like their voices do not matter, and is probably the most widely-supported theoretical perspective of the three, especially in international research (Birch 2010; Chang and Chu 2006; Miles 2015; Putnam 1993; Theobald 1990; Warren 2004). Miles (2015) suggests that people vote more in well-functioning governments, where they feel that they have a say in the government's daily actions. Similarly, Birch (2010) argues that confidence in the electoral process will increase an individual's likelihood of voting. Alternatively, because the gains of corruption often benefit the illegitimate and powerful actors who are involved in the corrupt practices, citizens might feel alienated from their governors and fail to participate politically (Theobald 1990).

This idea holds in quite a few empirical tests on corruption, though the vast majority of these tests have been international (Stockemer, LaMontagne and Scruggs 2013). For instance, Miles (2015:373) compares thirty-five democracies using a cross-sectional analysis and finds that "systems with fair governance have higher levels of reported voting…[a]ll things being equal, a one-unit increase in procedurally fair governance predicts a more than 5 percent increase in voter turnout in a system." In a smaller study, Costas-Perez (2014) conducted an analysis of corrupt municipalities in Spain from 1999-2007, matching corrupt and non-corrupt municipalities on a variety of factors, and found that an incident of local corruption reduced citizens' voter turnout by 1.5%, and that it reduced the turnout of citizens who classified as

politically independent by 4.4%. Finally, European countries with higher voter turnout also report citizens with higher levels of overall satisfaction with the government. Sundström and Stockemer (2015), suggest this is because citizens are more satisfied with governments that they perceive listen to their needs: the antithesis of a corrupt government.

We also see disengagement theory play out when people learn more about corruption in their government. Disengagement theory suggests that the more that people learn about corruption in their state, the less likely they will be to turn out to vote. This idea was borne out in a field experiment in Mexico, where researchers delivered information to certain voters about the corrupt practices of politicians who were running in an upcoming election, and found that precincts where individuals had received such information had lower voter turnout than precincts without information, or those that were given traditional "get-out-the-vote" messages (Chong et al. 2011). This is in contrast to earlier research that showed that increased information on candidates in general increases voter turnout (Lassen 2005). Per disengagement theory, the importance is in that it was information about corruption.

The work to test this theory in the U.S. has been limited. Johnston (1983) found that in "moralistic" (Elazar 1970, 1984) states citizens vote less during years with higher levels of corruption, and suggested this was in direct response to citizens' perceived corruption in a state. However, "moralistic" states only refer to states with a background of citizens working towards a collective good and what Elazar (1970, 1984) classifies as holding a particularly demanding view of what their politicians should do for them. Thus, the generalizability of the effect of corruption to all of the U.S. is limited. In the only specific test of this theory in the U.S., Caillier (2012) conducted a phone survey of Louisiana citizens on their voting records and their perceptions of

corruption, and found people to be less likely to vote if they perceived the government as being more corrupt than before. This, however, only measures *perceived* corruption.

In sum, disengagement theory states that individuals participate more in electoral politics if they feel that the system is legitimate, and they will be less likely to vote if they feel their governments are corrupt. As Miles (2015:364) writes, "[w]hen people receive unfair treatment in their interactions with these institutions either because of corruption or their lack of political clout, the message they receive is that ordinary citizens do not count in their political system." Disengagement theory suggests that most people fall in line with this statement, that corruption would displease them, and that they will vote less often because of it.

Mobilization Theory

On the other hand, instead of not participating, citizens might vote *more* than they would if they felt the political process was working in their best interest. In essence, citizens might see corruption in their midst and then be *mobilized* to vote. Because voting is the most efficient way to oust corrupt leaders, citizens will vote more when they perceive corruption, says mobilization theory (Praino, Stockemer and Moscardelli 2013).

A major supporter of this theory comes from a specific study of voter turnout in post-communist countries (Kostadinova 2009). In this study of whether Eastern Europeans would abstain from voting or rebel at the ballot box in the face of corrupt governors, Kostadinova (2009) found that corrupt governments actually encouraged voter turnout, as citizens see an opportunity to "throw the bums out of office." In other words, while Kostadinova (2009) did find a negative effect of corruption on citizen participation, the mobilization of citizens to challenge their governments outweighed that corrosive effect, and overall voter turnout was higher. Similarly, in a study of legislative elections in Portuguese municipalities in the 2000s, Stockemer

and Calca (2013) found that more corrupt districts had higher levels of turnout than "cleaner" districts and suggested that was because citizens were mobilized to vote out corrupt politicians.

There is some support for mobilization theory in U.S. elections as well. For example, in an analysis of Congressional races from 1972 to 2006, Praino, Stockemer and Moscardelli (2013) found that political scandals for congress people 1) encourage voter turnout in the elections immediately after the scandal and 2) reduce incumbents' margins of victory or increase their margins of loss. Furthermore, the negative impact of a scandal on a congressperson holds for as long as the incumbent is in office (Praino, Stockemer and Moscardelli 2013). Even if they continue to win the seat, the smaller margins and the increased voter turnout rate suggest that angry citizens are using the electoral process to voice discontent.

All told, mobilization theory suggests that corruption will increase voter turnout because, while corruption has an overarching corrosive effect on many people's likelihood of voting as suggested by disengagement theory, the number of people who use the ballot box to vote out corrupt officials overpowers that negative impact. One limitation of mobilization theory is that the net positive effect might be held under only very specific circumstances. For instance, this might only apply to individuals who had recently moved into an area (Timpone 1998), or citizens in newer democracies who feel like they have a voice in making an impact (Kostadinova 2009). Does this hold for long-standing, established democracies? Under what situations mobilization theory explains voter turnout remains to be seen.

Voter Acquisition Theory

Finally, voter acquisition theory states that corrupt states will see higher voter turnout because their politicians will use more resources to encourage their constituents to vote. This theory explicitly looks at the actions of politicians to acquire more votes; politicians want to be

elected to seats in corrupt areas, so they will either maintain or receive for the first time that extra-governmental power such a seat entails. This is a much more cynical view of a positive effect of corruption on voter turnout. In the words of Stockemer, LaMontagne, and Scruggs (2013:82), "[t]his view basically contends that voters are bought off to participate."

Unlike disengagement and mobilization theories, this perspective is borne out of studies of the U.S. Specifically, Karahan, Coats and Shughart (2006, 2009) advance this view based on their research of a number of elections in Mississippi counties in 1987. In a quasi-experiment, where some Mississippi county supervisors were indicted on corruption charges and some were not, Karahan, Coats and Shughart (2006, 2009) found that counties with corrupt supervisors had higher levels of voter turnout than counties without such convictions, *notwithstanding* who was running (that is, whether it was a corrupt incumbent against a challenger or a non-corrupt incumbent against a challenger). Therefore, they suggest that politicians vying for seats in areas where corruption is more common will do more encourage voters. Specifically,

> [V]oter turnout in the average corrupt county was about 4.5% higher than a similarly situated county where supervisors were more honest, *ceteris paribus*. The demand for votes by politicians and the exchange of favors for votes by candidates competing for offices whose values are raised by the rents of public corruption apparently combine to produce higher levels of voter participation. (Karahan, Coats and Shughart 2006:102)

While this study examined one set of counties in Mississippi, this same practice might also affect elections for governors across the United States. Escaleras, Calcagno and Shughart (2012) found that corrupt states have higher than average turnout for gubernatorial elections, once again without significant variation based on who is running: convicted incumbent or new challenger.

Already bolstered by candidates' appeals to voters, people with ideological leanings in line with corrupt elected officials are more likely to forgive those officials, thinking that corruption is merely "business as usual," and that the benefits of voting for their candidate outweigh any illicit activity. Specifically, "[i]f partisan leanings are strong, citizens may

disregard corruption as a determining factor in their decision and continue to vote for the party to which they are ideologically aligned" (Costas-Perez 2014:7). This is in line with findings by Peters and Welch (1980) and Welch and Hibbing (1997) as they found that congressmen and congresswomen in elections from 1970 – 1990 experienced no real costs to reelection after involvement in political scandals. Essentially, a voter's political affiliation, and the strength of that affiliation, is a salient factor in how seriously one takes government malfeasance, with people substantially less concerned by the misdeeds of their favorite politicians (Van de Walle 2008).

To wrap up, voter acquisition theory suggests that citizens accept some level of corruption, at least to the degree that it does not negatively affect their voting habits, and that politicians who want to be elected to more corrupt locales will do what they can to encourage their constituents to vote. Notably, studies that test mobilization theory and voter acquisition theory are less common than studies that test disengagement theory (Stockemer, LaMontagne and Scruggs 2013). However, the way that U.S. citizens experience corruption may be different than the ways other countries' citizens experience it.

Hypothesis 1: The Effect of Corruption on Self-Reported Voting

While each theory has its merit and empirical support, I suspect that disengagement theory will be supported in the analysis, not mobilization nor voter acquisition theory, for reasons I detail below. In line with disengagement theory, I hypothesize that corruption will have a negative impact on the likelihood of self-reported voting in the analysis of all citizens, and that this effect will remain after controlling for other variables. Formally, I hypothesize:

> H1: Individuals who live in states with higher rates of corruption prosecutions will be less likely to self-report voting than citizens who live in states with lower levels of corruption prosecutions.

I have two reasons for this hypothesis. First, I believe that voter acquisition theory and mobilization theory better explain the voting habits of those who think that their votes make a difference in a particular election. Because I am measuring voting for presidential and midterm elections, and not local elections, I think that most citizens will think that their votes will not matter in any meaningful way. Voter acquisition theory, furthermore, might be best applicable for studying small-scale elections.

Secondly, I argue that disengagement theory taps into the general attitude U.S. citizens hold toward current politicians more accurately than the other two perspectives. Corruption has been a part of the U.S.'s history for quite some time (Glaeser and Goldin 2014), and people may believe there is little they can do to challenge it. In contrast to disengagement theory, mobilization theory better explains voter turnout in nascent countries or in places where elections are being held for the first time (Kostadinova 2009), before citizens might suspect there is little they can do on-the-ground to change policies.

SECTION IIB: VOTING, CORRUPTION, AND OTHER FACTORS

Moving on, in this section, I provide rationales to suggest that we should expect the effects of certain demographic characteristics on self-reported voting to depend in some way on the amount of corruption in a state. In line with my first hypothesis, I propose that state-level corruption differently influences the effect that certain social background variables have on voting as outlined next.

Education

While research shows that those with more education vote more often (Gallego 2010; Leighley and Nagler 2014), there is reason to believe that this relationship will not be as strong in more corrupt states. Essentially, in more corrupt states, those with more education are more

likely to come into contact with information that exposes them to the levels of corruption in their political bodies, because they are more connected through higher education to ideas about what good governance (and bad governance) looks like (Hakhverdian and Mayne 2012; Mishler and Rose 2005). Relatedly, those with more education might be members of social circles in which political corruption is commonly discussed (McClurg 2003; Rolfe 2012). Thus, the positive influence of education on voting might weaken in states with higher levels of corruption.

With this background, I provide my first sub-hypothesis:

> H1a: The level of state political corruption will influence the effect of education on self-reported voting in that the positive effect of education on voting will be reduced in more corrupt states.

Income

Leighley and Nagler (2014) show that individuals with higher incomes are more likely to vote. Would the amount of corruption in a state influence the effect that income has on voting? As Mills (1956) points out, members of the industrial elite (e.g., Americans with some of the highest wealth and earnings) are much more likely to travel in the same social circles as powerful political players, and perhaps be more forgiving of their misdeeds. If these citizens live in states where corruption is seen as normal, they may feel like their higher income could allow them to reach politicians they elect and feel *more* engaged in the political process because they believe their dollar can connect them to powerful political players (Rose-Ackerman 1996). Thus, the positive influence of higher income on voting may be increased in more corrupt states.

With this thought, I provide my second sub-hypothesis:

> H1b: The level of state political corruption will influence the effect of income on self-reported voting in that the positive effect of income on voting will be amplified in more corrupt states.

Race

There is also reason to believe that the effect of one's race on voting might be influenced by the amount of corruption in a state. Leighley and Nagler (2014) report that whites vote at higher rates than all other minority groups in the aggregate, but that, controlling for other factors like income and education, blacks vote more often whites. Both blacks and whites vote at higher rates, controlling for other variables, than Hispanics or members of other minority groups (Leighley and Nagler 2014; Musgrove 2012; Wilson 2012).

Historically, whites have consistently been given more political power than other racial groups (Omi and Winant 1994) and have disproportionate representation in elected positions (Musgrove 2012). Relatedly, in the U.S., the illegal actions undertaken in corrupt states often provide benefits for well-connected white families and not as much for blacks, Hispanics, or members of other races (Vogl 2012). Thus, in more corrupt states, the positive influence of identifying as white compared to Hispanics and other racial minorities who are not black on voting might be increased, with the idea that the corrupt government would be working in whites' favors anyway.

As a corollary, members of Hispanic communities, black communities, or communities of other races than these groups might already be primed to see the ways in which the government does *not* work for them (Musgrove 2012; Teachout 2014). Black voters in particular might be triggered by stories of political corruption that remind them of decades of racial inequality and institutional racism where their voices were not heard by politicians, both in law and in practice (Omi and Winant 1994). Therefore, the positive effect of being black on voting might decrease in more corrupt states.

With these thoughts, I provide my third sub-hypothesis:

> H1c. The level of state political corruption will influence the effect of race on self-reported voting in two ways; in more corrupt states, the positive effect of being white compared to any other racial group on voting will increase, and in more corrupt states, the positive effect of being black compared to being white on voting will decrease.

Religious Affiliation

Finally, the effect of membership in different religious affiliations might be influenced by corruption in different ways. The positive influence on voting of church membership in liberal Protestant groups, which are more accepted and part of mainstream society (Sherkat 2014), might not be as affected by political corruption as the effect of adhering to more other-worldly religious affiliations. Liberal Protestants might believe it is their duty to engage in the secular world through civic participation, despite the actions of the government (Bean 2010).

The effect of membership in religious affiliations based on other-worldly orientations (e.g., sectarian Protestants or Catholics and certain minority religious traditions) may be more impacted by political corruption, however. People with these religious affiliations might choose to disengage from the political process at higher rates in states where they see the government is doing something not in their best interest. For instance, Catholics and Evangelical Christians have a history of forming their own schools and social groups when the government tells them to do something against their views of freedom and choice (like integrate their schools) (Rose 1988; Sikkink 1999). Could the effect of membership in different religious affiliations on voting differ based on the level corruption in a state?

With this question, I provide my fourth, and final, sub-hypothesis of this section:

> H1d. The level of state political corruption will influence the effect of religious affiliation on voting in that in more corrupt states, the negative effect of being sectarian Protestant compared to liberal Protestant on voting will increase.

In conclusion, how education, income, race, and religion affect voting differently depending on the state level of corruption has not been fully investigated, although there are reasons to suspect an influence of corruption on the effect of these key demographic variables. Thus, an analysis of corruption and voting habits should include cross-level interaction effect tests that allow us to understand the impacts of these aspects of one's social location in more or less corrupt states.

SECTION III: GOVERNMENT TRUST

Along with understanding variation in voter turnout, academics also pay attention to the reasons why citizens trust the government, with another tacit assertion that democracies work more effectively if citizens believe their elected officials are trustworthy (Mishler and Rose 2005; Uslaner 2002; Wilkes 2015). Explicitly, "citizens who live in a country where they perceive that corruption or other forms of unfairness in the public administration is common are likely to be less supportive of the idea that the state should take responsibility for policies *even if they ideologically support the goals such policies have"* (Rothstein 2013:1015, emphasis mine). In contrast to Costas-Perez (2014), Peters and Welch (1980), and Welch and Hibbing (1997), Rothstein (2013) argues that even if individuals support the same goals as politicians, they may not be likely to support those politicians and their actions if they perceive them to be corrupt.

Understanding various levels of social trust in general has been of great interest to social scientists (e.g., Putnam 1993, 2000) because individuals who trust others are more active in civic organizations, more giving to charities, and more accepting of minorities (Bjørnskov 2007; Delhey and Newton 2005; Nannestad 2008): all traits that show a positive connection to society. Furthermore, countries with more trusting individuals have higher levels of overall racial tolerance, less crime, and greater economic success (Rothstein and Uslaner 2005). What is of

central interest in this analysis, however, is not whether individuals trust others in general, but whether they trust their government. This is a notable distinction, because the determinants of what makes individuals trust governments may be qualitatively and quantitatively different from what makes individuals trust each other (Uslaner 2002). Furthermore, recent survey data suggests that U.S. citizens are becoming less trusting of their government over time (Hetherington 1998; Nannestad 2008).

So, why do citizens trust their government? There are two distinct perspectives that emerge from current literature. The first deals with the way an individual is socialized and the culture that they live in (i.e., an individual-level view) (Mishler and Rose 2001). This view treats political trust as something citizens are taught about early, and as something relatively stable in their lives. The second view treats trust as a manifestation of a citizen's reaction to the world around them as manipulated and controlled by others (i.e., a structural-level view) (Mishler and Rose 2001). This view treats citizens as rational actors; when government officials do something they do not like, they trust less.

Similar to my discussion of voting and voter turnout, I now detail the relevant literature on political trust as 1) a socialized characteristic and 2) as a response to actions made by the government. I then turn to research that examines the influence of corruption on political trust, and I then bring up meaningful points that suggest that different social groups might be affected by corruption differently in their overall trust in the government.

Socialized/Individual Factors associated with Political Trust

This first perspective assumes that people are socialized to be more or less politically trusting from an early age, and that trust in the government comes from "long-standing and deeply seeded beliefs about people that are rooted in cultural norms and communicated through

early-life socialization" (Mishler and Rose 2001:31). This cultural theory would suggest that there is little that governments can do through reform or legislation to instill trust in people after this socialization. Instead, political trust is a culturally embedded aspect of people's lives, and people learn how trustworthy the government is at an early age. Uslaner (2002) theorizes that people are hard-wired from an early age to believe in the relative corruptness or purity of others around them, and this is a view that a person will have as he or she goes through the world. Hetherington (1998:799) uses different language but says that "trust is more a function of institutional than incumbent support," and later states that people have embedded values about the government that certain policies can do little to change in the long term.

This idea has some empirical support. For example, in an analysis of trust scores over time created from compiling several data sources, Keele (2007) found that declining social capital and civic engagement reduces trust in the government over time, while changes in government operations have little to do with citizen trust, and suggests that a lack of trust is tied with a citizen's lack of other social connections. This finding falls in line with Bjørnskov's (2007) work, which shows that trust scores in nations remain relatively stable over time, even over politically volatile times, signifying that people's opinions of government do not change very much.

Some of the socializing agents that would influence an individual's entrenched trust or lack of trust in the government are early influences (e.g., parents, teachers, and childhood peers) as well as later influences (e.g. religion and adult peers) that increase an individual's connectedness to the government or their own social status in their society. As is the case with voting, having a higher level of education is also a strong correlate of political trust (Rothstein and Uslaner 2005). For instance, in a survey of individuals in 16 democratic countries, Anderson

and Tverdova (2003) found that individuals with higher levels of education expressed more support for the political system in general, controlling for other factors.

Similarly, one's social class has much to do with one's trust in the government, with higher income and wealth associated with higher trust. Rothstein and Uslaner (2005) state that individuals who use government assistance, for instance, are more likely to distrust the government. This is a corollary to the finding that those with higher income and in a higher social stratum are more likely to say that the government is doing a good job (Anderson and Tverdova 2003). In the U.S., when people feel like others are receiving more out of life than they are, trust breaks down. As reported in one analysis, "*[i]nequality is the strongest determinant of generalized trust over time in the United States and across the American States"* (Rothstein and Uslaner 2005:48, emphasis theirs). In other words, those with the least materially are often those with the least trust, because they feel they are not receiving their fair share of the economic pie.

Another characteristic that might have a major impact on political trust is one's race. Blacks in particular may distrust the government more due to years of systemic inequality and racism (Omi and Winant 1994), unequal representation in the political sphere (Bowen and Clark 2014; Musgrove 2012), a history of being disenfranchised and unduly investigated when they are politicians (Musgrove 2012), and an ideology steeped in the history of the black experience of slavery, segregation, and discrimination (Wilson 2012). Nunnally (2014:7) writes that blacks' "enhanced socialization about historical race relations, black culture, and the effect of race on their living conditions influences how they perceive and relate to black nonblack group members." Blacks and Latinos report trusting the government more when they are told about members of congress who share their racial background, though there is no such effect for whites, suggesting the importance of being represented for these traditionally underrepresented

groups (Bowen and Clark 2014). With a primarily white government, historically not situated for (and most often actively against) black interests, it is clear how race-based political distrust comes to be (Howell and Fagan 1988).

Religion also influences overall levels trust in the government. More conservative churches and sects may see certain government actions (such as legalizing abortion) as overstepping its bounds and may trust the federal government less than progressives (Sherkat and Ellison 1999). While these opposing viewpoints have been exaggerated in the media (Sherkat and Ellison 1999), religious beliefs structure political values (Manza and Wright 2003), and certain religious affiliations socialize their congregants what to believe about the government, even in unofficial ways. For instance, in an analysis of two mainstream liberal Christian congregations and two conservative Christian congregations, Bean (2014:17) found, "[a] broad set of laypeople serve as opinion leaders, helping their less politically engaged peers to link evangelical identity to conservative politics." This subtle mechanism reinforces political ideologies that emphasize more or less governmental trust. One of those political values might be small government size, where the power of family can be stronger (Bean 2014), and thus more conservative religious groups may be less trusting of the federal government who they perceive has overstepped its bounds and limited that familial power. Often, this is not in reaction to a specific policy per se, but instead due to a sort of religious identity that can make analogous political trust and participation with a particular faith background (Bean 2014).

All in all, a cultural, or individual, perspective on political trust would emphasize social and demographic factors that are in many ways not part of the political process to explain where trust comes from. Our social location and social backgrounds frame our individual viewpoints. This emphasis on the culture which one is socialized in acknowledges that political trust might

be a manifestation of the intersection of one's identities in a variety of social spheres, and that policies that fall in line with an individual's beliefs may do little to change that overarching trust.

Institutional/Structural Factors Associated with Political Trust

On the other hand, citizens might trust the government more or less based on what the government actually does, and not based on preconceived notions or socialized ideals. This view suggests that citizens trust the government in a rational way, basing their trust on a specific set of circumstances (Mishler and Rose 2001, 2005). Essentially, if citizens perceive the government as acting in their best interest, they will be more likely to trust the government. Thus, countries that are more supportive of their citizens in terms of providing basic social services and the ability to flourish, should see a more trusting citizenry (Cullen and Chamlin 1999).

It makes sense that citizens will trust those governments they see working in their best interest, and there is reason to believe that trust is influenced by external sources. Certainly, experiments have shown that people are more likely to trust individuals who they perceive as being nice to them (Nannestad 2008). Others point out that an individual's political trust at a particular time is affected by how much an individual supports a particular policy (Hetherington 1998). Furthermore, news stories often paint bleak pictures of the U.S., and Hetherington (1996) showed how these stories influence citizens' political beliefs.

Some explicitly state that a lack of citizen trust is directly tied with an in individual believing the government cannot be effective. For instance, Miller (1974:970) determined that distrust mainly arises "out of dissatisfaction with the policy alternatives that have been offered as solutions to contemporary social problems." In other words, citizens will trust the government less when they think the policies the government makes are not in their best interests. Mishler and Rose (2005:1069) examined post-communist Russia and found that citizens did not support

the regime nor trust the government institutions, based on rational "assessments of the failure of political institutions to curb corruption and provide reasonable stability and growth." Catterburg and Moreno (2005:46) studied new and established democracies over the previous 20 years, and found "individual well-being proves to have a robust effect across different groups of nations, which means that citizens' confidence in their political system is tied to the system's ability to increase or maintain well-being." In short, empirical research often reflects the rationality of citizens in why they trust the government.

If citizens assess government policies and decide to trust the government more or less based on those policies, in an increasingly polarized public body such as the U.S., high levels of trust in government will be difficult to attain by any particularly conservative or liberal political agenda. Democrats are more likely to trust when democrats are in power, and vice versa (Anderson and Tverdova 2003). Furthermore, countries that are ethnically homogenous have higher levels of general social trust (Delhey and Newton 2005), and this might play a part in political trust. Citizens may be less trusting of governments that they perceive to be operating in the special interests of another ethnic group, for example (Delhey and Newton 2005).

Of course, bifurcating these predictors of political trust in strict cultural and structural camps does not allow for the inclusion of variables that have been shown to be statistically significantly correlated with political trust, but do not fit neatly into either side. For instance, is age a structural or cultural factor? Anderson and Tverdova (2003) found that older individuals are more trusting of civil servants, but are equally trusting of politics in general as younger individuals, and Mishler and Rose (2001) have found that older citizens are more trusting of the government. Thus, age should be investigated in studies of political trust, but the question that remains is whether one's age reflects early-established similarities shared by members of the

same cohort, or individuals' new experiences with the government and assessments based on that experience? Whatever the case, the cultural/institutional dichotomy serves as a useful ideal type, as long as we understand that certain attributes could plausibly fit in either camp.

In conclusion, both cultural – that is, those demographic group memberships that influence and socialize individuals throughout one's life course, but especially early on in the life course – as well as institutional perspectives find support in their takes of why people trust in the government. Nevertheless, Keele (2007) arrives at a different conclusion than either of the perspectives, stating that "trust is an evaluation of politicians and their management of the economy and responds immediately to any changes in government performance. But *trust also reflects the lessons learned in civic activity and feelings of personal misanthropy*." (Keele 2007:251, emphasis mine). Thus, it will be important to include variables that can tap into both internalized ideas of trust and external factors that might influence citizen trust, and those (like age) that are somewhere in between in future analyses.

SECTION IIIA: GOVERNMENT TRUST AND POLITICAL CORRUPTION

While there is debate over the causes and correlates of government trust, previous literature makes it clear that corruption in politics breeds distrust in the citizenry (Rothstein 2013; Uslaner 2008; Vogl 2012). This is true in terms of perceptions of corruption in the U.S. (Rothstein 2013), as well as in international research (Chang and Chu 2006; della Porta 2000). Unlike for voter turnout, there is no theory that suggests individuals will be more likely to trust governments that are more corrupt.

There is not much of a leap to theorize that corruption should impact political trust. Theorists presume corruption influences and dissuades individuals from being engaged with their political body (Alatas 1990), erodes public confidence (Green and Ward 2004), and decreases

citizen trust in the government (Woods 2008). In experiments, people report feeling cheated and less trusting of those they perceive as corrupt. For example, both in relatively highly corrupt Romania and relatively low corrupt Sweden, Rothstein and Eek (2009) found that individuals were more likely to report distrusting the government, and their fellow citizens, after reading vignettes about corrupt political regimes.

Previous research makes clear, and almost without exception that corruption negatively impacts political trust internationally (Alatas 1990; Chang and Chu 2006; Kostadinova 2009; Van de Walle 2008). Even in countries where corruption is so endemic that it might be considered "noise" to citizens, that is where citizens expect corruption as part of their daily life, such as offering bribes to teachers or police officers (Anderson and Tverdova 2003), citizens report feeling less trusting of the government when they are exposed to corruption more often (Chang and Chu 2006). Using surveys from sixteen new (such as the Czech Republic and Latvia) and old democracies (such as Britain and the U.S.), Anderson and Tverdova (2003) found that countries that ranked higher on the Corruption Perception Index (CPI) had fewer citizens report trusting civil servants and more evaluate the political system as ineffective.

The question, yet again, remains. Does this relationship, found in international research, hold true in the United States? To answer these questions, I now turn to literature that complicates the perceived role of corruption on political trust, particularly for the U.S.

Why Corruption Might Not Affect Political Trust

First, as mentioned, corruption might not affect political trust if one's trust in the political process is an individual characteristic, borne out of a distinct earlier socialization, and is relatively uninfluenced by political happenings. Specifically, one's cultural pre-disposition could influence how an individual interprets corruption. For example, using experimental vignettes

where individuals indicated their levels of political trust and then responded to blatantly corrupt, quasi-corrupt, and legal activities, less trusting citizens viewed political actions "using a preexisting negative schema about politics and the political class, while more trusting individuals employ a more positive schema" (Wroe, Allen and Birch 2013:5). Similarly, Pena López and Santos (2014) found that people's perceptions of political corruption and their assessments of political trust varied greatly, depending on their pre-established perceptions of the efficacy of the government and the trustworthiness of others. Pena López and Santos (2014) concluded that higher levels of perceived political corruption did reduce citizens' trust scores, but only when those citizens were more trusting of outsiders in the first place. Thus, there is reason to believe that the effect of corruption on trust might differ based on one's preconceived notions.

Perhaps even more problematically, international research that supports the notion that corruption reduces trust is often based in cross-sectional analyses that cannot acknowledge that "trust influences institutional performance just as institutional performance shapes the public's trust in their institutions and in one another" (Morris and Klesner 2010:1259). In this reality, we are left without really knowing whether corruption reduces trust, or whether a lack of trust leads to citizen disengagement that fosters corruption. Problematically, this interplay between state corruption and citizen lack of trust can definitely be self-reinforcing. If corruption reduces citizen trust in the legitimate methods of engaging with the government, they might then only engage with their government in illegitimate ways (della Porta 2000; Rothstein 2013).

Finally, another area of interest concerning trust is an individual's political orientation. One's political orientation might affect how much they trust a particular party of the government, independent of objective corruption. Specifically, those who are members of the political party in power might be less affected by political corruption's occurrence within that party. For example,

if a Democrat is in power, liberals may turn a blind eye towards discretions, like committing perjury for Bill Clinton, but if a Republican is in power, conservatives may ignore civil rights violations during the Bush II administration. Thus, it is not one's political party per se that influences an individual's perception of corruption, but rather *which* political party is committing the corrupt act. This would be in line with Anderson and Tverdova's (2003:101) finding that, "corruption has less negative effect on evaluations of the political system among respondents in the political majority."

Basically, even the relatively clear idea that corruption reduces political trust is complicated. One final area of complication is where I turn next - whether there is something unique about the American states in how corruption affects citizens' trust.

Corruption's Effect on Trust in the United States

While the U.S.' storied past reflects a culture of opposition to political corruption (Teachout 2014), there are a few reasons to suspect that corruption might *not* affect Americans levels of trust. Could there be something unique about Americans' experience to this regard? There are several reasons to think so.

First, compared to citizens of other industrialized countries, Americans often report being less trusting of their government (Teachout 2014). This trend exists while the average number of corruption convictions per year in the United States have not increased in any meaningful way over the past thirty years (as detailed in Public Integrity Section reports from 1978 – 2013), while other countries have experienced revolutions, regime-changes, and ostensibly less democratic governors (Kostadinova 2009; Vogl 2012) . In other words, American citizens have always had higher levels of distrust than citizens of other countries internationally, and this distrust has increased despite no real changes in corruption rates in the U.S. in the aggregate.

This suggests, citizens may have pre-established levels of trust that changes in rates of corruption do not affect.

Next, U.S. citizens may also form their beliefs through news sources, often receiving their news and ideas from sources that already reflect a pre-established worldview (Jamieson and Capella 2008). These "echo chambers" of cable news sources are a particularly American phenomenon at least to the degree that they affect people's decisions, and views on the world around them (Jamieson and Capella 2008). Furthermore, individuals who watch the news regularly are affected by the way the media frames stories. Hetherington (1996) argues that negative media portrayals may have helped to cost Bush I his re-election, especially because, historically, the objective measures of economic factors and his role as incumbent otherwise indicated a strong likelihood of re-election. Today, many of the news sources that people rely on to report misdeeds of corruption tell stories that fall in line with the political beliefs of their audience (Jamieson and Capella 2008), and they affect viewers' perceptions regardless of official measures. PIN measures are just such official measures of corruption, but we cannot always know the sources of media individuals view to help guide their interpretations of political life.

Furthermore, the PIN measures of corruption used in this analysis might not tap into what citizens actually care about in terms of corruption (Teachout 2014). PIN reports include a wide variety of corruption in their statistics, and they do not differentiate between a county clerk skimming money out of a bank account and cases where governors go to jail for attempting to sell state legislature seats. In a world where citizens are becoming more aware of issues related to the legal ways lobbyists and donors influence politicians, the types of corruption the PIN prosecutes might seem antiquated and unimportant to many.

Finally, and in a counterintuitive way, Americans might trust the government *more* if they see more corruption prosecutions in their state. If citizens who believe that corruption is rampant in their states see more politically corrupt officials being prosecuted, this might make them more trustful of the government, because they feel like the government is "doing its job." For instance, individuals are more likely to trust governments if they are told that a corrupt official will be caught and punished for his or her misdeeds (Nannestad 2008). Furthermore, Boylan and Long (2003) found that state corruption prosecutions were positively correlated with the amount of power statehouse reporters believed corruption investigators had in their states. Thus, more prosecutions might mean more oversight and a government that protects its citizens.

In sum, not only are U.S. citizens unique in their voting levels, they are also unique in their levels of political trust, and possibly how corruption affects that trust. These arguments suggest that it still is important to empirically test these relationships before assuming international findings apply to the U.S.

Hypothesis 2: The Effect of Corruption on Political Trust

While there are conflicting expectations regarding any effect of political corruption on citizen trust, my expectations derive from the work supporting the idea that people trust the government based on their interpretations of the operations of the government. More specifically, I take a structural perspective on the influence of corruption, meaning that I suspect that citizens trust the government more or less based on what the government actually does, not based on their own preconceived notions or early socialized ideas. Therefore, my second hypothesis is that:

> H2: Individuals who live in states with higher rates of corruption prosecutions will trust the government less than those in states with fewer corruption prosecutions.

This hypothesis stems from the great deal of work that suggests citizens are less trusting of their governments where there is more corruption internationally, with a suspicion that Americans should be affected similarly. In fact, because corruption is such a hot-button issue in the U.S., we might even expect the negative effect of corruption on American citizens to be larger than the effect is for citizens of other countries, although the focus of this dissertation work on the U.S. allows only room for speculation and not comparison.

SECTION IIIB: GOVERNMENT TRUST, POLITICAL CORRUPTION, AND OTHER FACTORS

Finally, in this section, I provide reasons to suspect that the effects of certain demographic characteristics on citizen trust depend in some way on the amount of corruption in a state. Similar to my discussion on voting, corruption, and other factors, I propose that state-level corruption differently influences the effect that certain social background variables have on voting as follows.

Education

The effect of education on political trust is complicated, though those with higher education have been shown to be more likely to trust the government in general (Anderson and Tverdova 2003; Rothstein and Uslaner 2005). How might the effect of education be influenced by the level of corruption in a state? In international research that studied this, using the 2008/2009 wave of the European Social Survey, Hakhverdian and Mayne (2012) found that education plays a major role in how people interpret corruption and how that affects their trust. Specifically, "the direction and magnitude of the effect of education is conditional upon the pervasiveness of public-sector corruption: in countries with low levels of corruption education boosts institutional trust; in countries with comparatively high levels of corruption education

dampens institutional trust" (Hakhverdian and Mayne 2012:747). In other words, for this sample of European countries, people with more education are more troubled by more corruption than people with less education in more corrupt countries.

By running cross-level interactions, I will test whether the effect of state corruption on the effect of education is similar to Hakhverdian and Mayne's (2012) finding in the U.S. Specifically, I hypothesize:

> H2a: The level of state political corruption will influence the effect of education on political trust in that the positive effect of education on trust will be reduced in more corrupt states.

Income

Individuals of different income strata also trust the government differently, with the poor less trusting and the rich more so, by and large (Anderson and Tverdova 2003; Rothstein and Uslaner 2005). This effect might be influenced by the context of the state in which individuals live, with reference to political corruption, as well. Those better off may not feel the sting of the negative consequences of what happens when governments are corrupt. Poorer individuals, on the other hand, are more likely to feel the impact in terms of fewer economic resources and lower overall quality of life (Green and Ward 2005). Thus, in more corrupt states, the theoretically-predicted negative effect of being poorer on political trust might be exacerbated while the positive effect for richer people might be unaffected.

In line with this thought, I hypothesize:

> H2b: The level of state political corruption will influence the effect of income on political trust in that the negative effect of having the lowest relative income compared to the highest relative income will increase in more corrupt states.

Race

Because of the priming many minorities have towards the government in terms of seeing the ways in which the government does *not* work for them (Musgrove 2012; Omi and Winant 1994; Teachout 2014; Wilkes 2015), the effect of residing in a more corrupt state might influence the effect that being a minority has on trust. This may be particularly true for blacks, because research suggests that the factors that positively influence institutional trust have smaller positive effects on African-Americans than for whites: in other words, blacks start at lower levels of trust overall and they also are not as easily influenced to trust as whites (Wilkes 2015). These ideas provide reason to suspect that, in more corrupt states, the effect of being black compared to white on political trust will be greater than it would be in less corrupt states, as corruption might exacerbate the negative influence this social location has on political trust.

With this in mind, I hypothesize:

> H2c: The level of state political corruption will influence the effect of race on political trust in that the negative effect of being black compared to being white will increase in more corrupt states.

Religious Affiliation

Finally, Christianity's many churches, sects, and affiliations influence churchgoers' overall trust toward the government in many different ways (Bean 2014) and the effect of one's religious affiliation on trust could be stronger or weaker based on the amount of corruption in the state. As mentioned above, Sherkat (2014) points out that liberal Protestants are less likely to focus on other-world compensators in their ideologies, thus attempting to live a "good life" in *this* world more than sectarian Protestants and even many Catholics. What this means in terms of the effect of corruption on political trust is unclear. Might the positive effect of being a liberal Protestant on trust in the government be weakened when these individuals live in more corrupt

states, because they are more invested in the problems that face people in modern society? Or, might they be more forgiving towards human foibles, with the level of state political corruption not affecting the influence of their religious affiliation? On the other hand, would the negative effect of being sectarian Protestant compared to liberal Protestants on trust be exacerbated in more corrupt states?

Because there is little guidance in the literature as to what to expect here, I speculate that the effect of state-level corruption on the effect of religious affiliation on trust will be amplified. Specifically, I hypothesize:

> H2d. The level of state political corruption will influence the effect of religious affiliation on political trust in that in more corrupt states the negative effect of being sectarian Protestant compared to liberal Protestant on political trust will increase.

In conclusion, corruption might play a part in explaining the effect of key demographic characteristics on citizen political trust. However, the majority of the work done on corruption and trust is from international research, and whether its effect exists in the U.S. remains to be seen. Furthermore, it will be important to see how the effect of corruption influences the strength of the effects of education, income, race and religion as well, to better understand the nuance of the relationship between corruption and trust.

Endnotes

1. For quite some time, the media has told the story that voter turnout is on the decline, since a peak period in the 1950s (McDonald 2015; McDonald and Popkin 2001). However, this assertion is problematic. First, there is no historically comparable "official" rate of voter turnout for all U.S. elections. Different sources use different denominators (e.g., the number of people over eighteen in an area, the number of people registered to vote in an area, or the number of citizens eligible to vote in an area) (Leighley and Nagler 2014; McDonald and Popkin 2001). In fact, if you use as a denominator the number of individuals who are eligible to vote (i.e., those that are not aliens or barred from voting because of felonies), the voter turnout rate has remained relatively stable (McDonald and Popkin 2001) or might even be increasing (Leighley and Nagler 2014).
2. It is indeed curious why class did not more often reach statistical significance in the articles Smets and Van Ham (2013) analyzed, but it is telling (and beneficial for this dissertation) that they suggest that some of these null findings might exist because many of the analyses they reviewed were undertaken using basic regression models, not the MLM that would be required to assess the impact of individual variables within the context of overarching, state-level characteristics.

CHAPTER 3

METHODS

Now that we are well-situated in what has come before, it is time to understand what this dissertation contributes to the conversation (and, perhaps more importantly, what it does not). This chapter will be presented in three parts. First, I introduce my data sources, which are:

1) The American National Election Study (NES), which provides the two dependent variables and the individual-level, demographic variables;

2) The annual reports from the Department of Justice's (DOJ) Public Integrity Section (PIN), which provide the key independent variable of the number of political corruption prosecutions in a state; and

3) The various data sources I use for level-2, structural variables.

In this section, I also detail transformations I made for these data and how similar variables are coded differently in voting and government trust specifications.

Second, I present the analytic strategy, detailing the benefit of using multilevel modeling (MLM). In this section, I also discuss preliminary steps I took to prepare the data, as well as the choices I made concerning methodological issues such as weighting, what to do with missing data, and the impact of outliers. I close in the third section with a brief description of my analytic tool, Stata 13, and the most common commands I used for the analyses in Chapters 4 and 5.

SECTION I: DATA SOURCES

Next, I discuss each variable in detail, highlighting their sources and any of their transformations. Specifically, I discuss my 1) dependent variables, 2) my key independent variable of political corruption, 3) level-1 (i.e., person-level) independent variables or covariates used in each analysis, 4) level-2 (e.g., state-level) variables used in each analysis, 5) level-2 variables used only in the analysis of voting, and 6) level-1 variables used only in the analysis of

political trust. I conclude with a brief methodological remark on the necessity of centering my continuous variables for use in MLM.

To begin, however, I detail the individual-level dataset in its own section, to provide context for all demographic characteristics.

American National Election Studies Dataset

All individual-level data come from the American National Election Studies (NES) dataset. This is a particularly rich source of data, as it is a cross-sectional, nation-wide survey, ongoing since 1948. In the study, surveyors ask respondents questions related to their political lives every two or four years. The NES is a well-respected dataset in the field, and has been used in several studies that investigate the relationship between people and political actions (e.g., Hetherington 1998; Richey 2010).

Over the years, NES has used in-person surveys as its predominant method, although telephone surveys are being conducted with more frequency than ever before. Surveyors are trained by field supervisors to ask questions systematically, and to offer the same response choices for each individual. From 1980 to 2000, the years in which the response rate is available from NES, the average response rate was about 69%, which includes accounting for individuals who were selected to be surveyed, but could not complete the survey for health reasons, because of language barriers, or because they were not home after repeated attempts by surveyors.

Two reasons make this a particularly appropriate dataset for the analysis. First, every respondent since 1948 (even though the earliest year used in this analysis is 1980) has been asked whether or not they voted in their most recent election, which makes this an ideal dataset to assess questions related to voting. Second, NES provides codes for individuals' states of residence for all years in which data are available on political corruption prosecution scores. This

makes this dataset an ideal one for MLM because citizens can be grouped into states, and we can more accurately see the effects of level-1 variables controlling for this state membership.

Next, I turn to a discussion of all variables used in this analysis.

1. Dependent Variables

Self-Reported Voting. This dependent variable comes from citizens' responses when asked if they voted in the last election. This question was asked after the surveyor introduced the concept of voting by saying that many people do not vote because they forgot, were not registered, were sick, or did not have time, thereby potentially alleviating the social desirability bias on behalf of the interviewee to say the socially desirable choice.

Despite the potential for this statement to reduce some over-reporting, there is evidence that respondents do over-report in the NES (Belli, Traugott and Beckman 2001; Holbrook and Krosnick 2010). Indeed, the average percentage of people who reported voting in the last election in the NES sample from 1980 – 2012 is 63%, (SD = 48.0), which is well above the overall percent voter turnout as determined by using both voting age population turnout (50%, SD = 10.2) and voting eligible population turnout (52%, SD = 10.6) for states at the aggregate level (McDonald 2003). Figure 3.1 shows how estimated voter turnout varies over time more clearly.

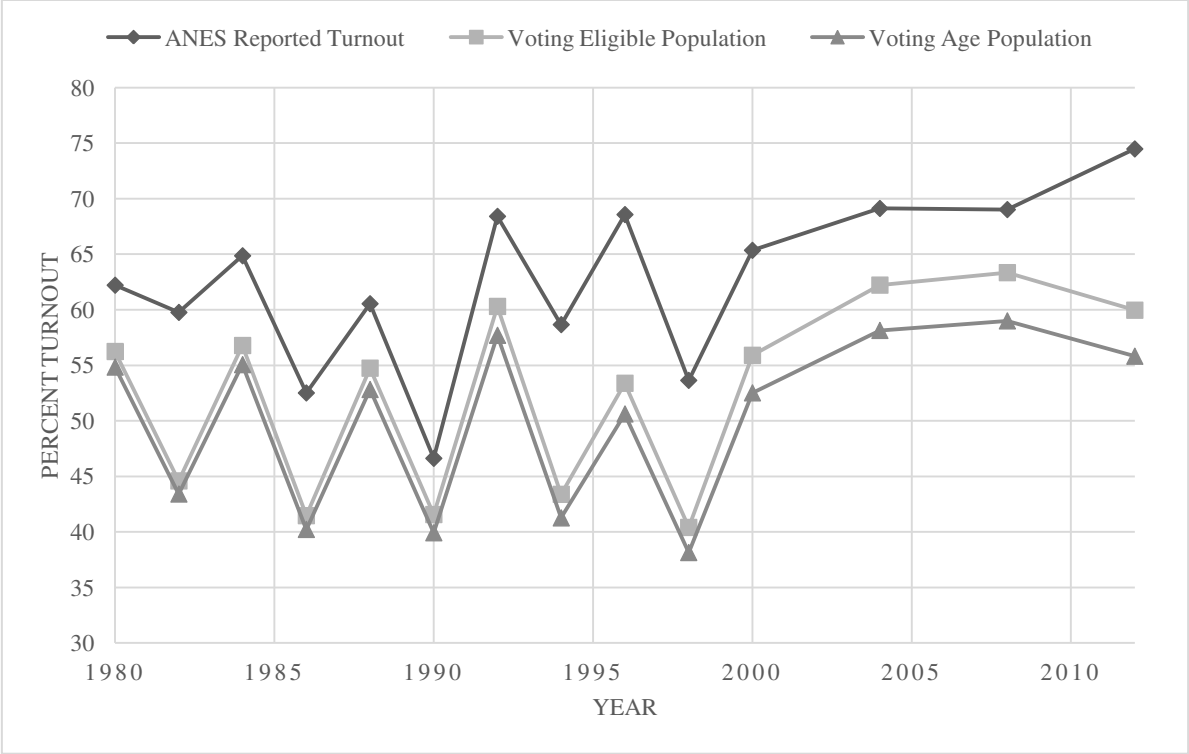

Figure 3.1. Voter Turnout by Year

This figure shows that aggregate self-reported voting behavior from the NES, while higher than the other two estimates, follows the same general trends in voting patterns. That is, at least until 2012 where we see the first time that the NES does not mirror the other measures in terms of going lower when they go lower, or higher when they go higher. However, McDonald (2003) suggests that while people may over-report, they do so at approximately the same rate over time: social desirability bias has not increased. To address the incongruity in 2012, I ran models with 2012 omitted and found the same statistically significant relationships presented in chapters four and five.

While any inaccuracies are not desirable, this over-report bias would become especially problematic if certain types of people are more likely to over-report, thus introducing nonrandom bias into the model. In fact, scholars suggest that those with higher levels of education and who express more political interest are more likely to report voting when they did not (Presser and

Traugott 1992), and thus care should be taken in using an individual's response to measure a characteristic perceived to be socially desirable (Belli, Traugott and Beckman 2001). However, most self-reported voters are similar to actual voters in many demographics (Belli, Traugott and Beckman 2001; Presser and Traugott 1992). Nevertheless, a limitation of using individual-level self-reported voting as a dependent variable is that this research cannot speak directly to *actual* voting, but instead *self-reported* voting, and should be interpreted as such.

Government Trust Index. This second dependent variable is the NES' own trust index, which compiles citizens' views of the government's fairness, who the government most benefits, how efficiently the government uses tax dollars, and a citizen's general sense of "crookedness" of government officials. Notably, this index of trust has face validity as a measure of an individual's trust in the government or other official institutions, rather than generalized trust, an important differentiation in the literature (Morris and Klesner 2010; Nannestad 2008). Specifically, this index is constructed from four survey questions:

- How much of the time do you think you can trust the government in Washington to do what is right?

    1. None of the time/Never
    2. Some of the time
    3. Most of the time
    4. Just about always

- Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?

    1. Few big interests
    2. Benefit of all

- Do you think that people in the government waste a lot of money we pay in taxes, waste some of it, or don't waste very much of it?

    1. A lot
    2. Some
    3. Not very much

- Do you think that quite a few of the people running the government are crooked, not very many are, or do you think hardly any of them are crooked?

    1. Quite a few
    2. Not many
    3. Hardly any

This index was constructed by the NES by assigning equal weight to each of the questions and adding them in a way that an individual's maximum score is 100 and minimum score is 0. The first question in this list is recoded as (1=0) (2=33) (3=67) (4=100); the second is recoded as (1=0) (2=100); and the lattermost two are recoded as (1=0) (2=50) (3=100). The recoded values are then totaled and divided by the number of valid responses. To avoid losing cases, respondents are included as long as they answer at least one question. The results are then rounded to the nearest whole number. It is important to note that this index is technically an ordinal measure: however, in practice, I treat this as a continuous variable because of the range of variation for respondents' overall scores.

There are two final points to note. First, although this index is created by NES, it is possible to construct the index using the variables they include, following their coding procedure. I did so as a reliability check, and found no differences between their scores and mine, indicating no inaccuracies in their reported output (at least terms of computing the index).

In terms of reliability, this trust index is somewhat internally consistent with a moderate Cronbach's Alpha of .68. Omitting any of the variables only reduces the alpha. However, because the Cronbach's Alpha is not higher, I also consider the impact of corruption on each component of the index in further analyses in Chapter 5. Basically, I test the effect of all of the independent variables on each of the individual questions to see if corruption influences an individual's perspective on one issue but not the other.

2. Key Independent Variable: State Political Corruption Rate

The measure of state corruption is the number of corruption prosecutions in a given state as reported by the PIN, per 100,000 citizens. This measure gives a number of prosecutions that is comparable state to state, as it is simply a case of dividing corruption prosecutions by the population and multiplying it by 100,000. Further, this measure has been used in a number of studies, which allows for comparability of key findings over time (Boylan and Long 2003; Flavin and Ledet 2013; Glaeser and Saks 2004; Meier and Holbrook 1992; Schlesinger and Meier 2002). Finally, while this only presents objective measures of corruption prosecutions, and not individuals' knowledge about corruption, there is reason to believe that citizens are indeed well-aware of the amount of corruption in their states. For example, in the U.S. public opinion of corruption is highly correlated with official statistics (Boylan and Long 2003; Goel and Nelson 2011), suggesting that citizens can roughly interpret the amount of corruption in their states.

Because this variable exhibits a positive skew (i.e., most states report very low levels of corruption), the measure might benefit by logging it, taking the square root of it, or squaring it, so that the distribution approximates normality. In fact, taking the square root of this corruption rate did in fact normalize it. However, including this more normalized measure of corruption did nothing to alter the significance of coefficients in final analyses for voting (and only did a little to change the actual coefficients), and so I use the more interpretable corruption rate score in chapters four and five.

3. Level-1 Control Variables used in Both Analyses

Female. Respondents were categorized as either male or female by NES. I created a dummy variable where 1 = female respondent and 0 = male respondent.

Married. Marriage as a social institution often represents social stability and can change individuals' attitudes towards voting and the role of the government. Respondents reported if they were married, never married, divorced, separated, widowed, or cohabitating. Because hypothetically those who are married are more invested in the status quo of society, I collapsed the categories into a dummy variable where 1 = married and 0 = not married.

Race. NES reports race in a 7-category measure. I intended to include all categories of white, black, Hispanic, Asian/Pacific Islander/Native Hawaiian, Native American or Alaskan Native, and Multiple or Other races. However, I found very low representations of certain minority groups. For example, in the analysis of self-reported voting, which had an ultimate N of 21,678, I found only 248 Asian/Pacific Islander/Native Hawaiian, 206 Native American or Alaskan Native, and 250 individuals who identify with other or multiple races – all between .9 and 1.2 percent of the total sample. Therefore, I compiled all three of these groups into an "other race" category, which gives more power to the model by reducing the degrees of freedom it uses, but limits the generalizability of this analysis for those groups.

Another limitation of NES race data is that it does not include a measure of Latino, but only of Hispanic. In much sociological literature, the designation of Hispanic is an ethnicity and not a race, and thus individuals could racially identify as white or black and still consider themselves Hispanic. However, the categories offered by NES are mutually exclusive, and thus Hispanic is the best indicator of Latino that we have. In other words, although individuals might prefer to identify as Hispanic and white, Hispanic and black or Hispanic and another race, they are forced to select one primary identity. Thus, I am left with a dummy set of four race categories: white, black, Hispanic, and other race.

Age. Respondents were asked to provide their age and their responses are put into the models as coded.

Own Home. Homeownership is often used as a measure of social stability and investment in one's community. Respondents reported whether they or their family owned their home, rented, or had a different arrangement. Thus, I include a dummy variable where 1 = own their home and 0 = do not own their home.

Education. Surveyors asked respondents to describe their level of education and then placed them into seven categories: 1) eighth grade or less, 2) high school but not a graduate, 3) high school graduate, 4) technical training after high school, 5) some college or an Associate's degree, 6) Bachelor's degree, and 7) Advanced degree (Master's or Doctorate). For my analysis of self-reported voting in Chapter 4, I use NES' scheme by turning each response into a dummy variable in a dummy-set.

While preliminary bivariate analyses led me to retain all 7 categories in the case of voting, I had the ability to collapse adjacent categories in the analysis of political trust and still capture sufficient variation in the relationship between education and trust. Therefore, in Chapter 5, the dummy set for education and political trust is a four-category set of no high school degree, high school graduate, some college, and bachelor's or advanced degree.

Income. To code income, NES lists income groups and asks respondents to stop them when they arrive at their average household income before taxes. Then, NES aggregates all of the responses and separates them into five distinct categories, or quasi-quintiles, for each year: 0-16%, 17-33%, 34-67%, 68-95% and 96-100%. Based on their statement of their annual household income, respondents fall into one of these categories. This classification allows for

comparisons to be made over time in a more meaningful way. For my analysis of self-reported voting, I use NES' five-categories in my own dummy set.

Once again, however, I was able to collapse my categories in the analysis of political trust. Thus, in Chapter 5, I treat income as a three-category dummy set to tap into the different levels of trust expressed by those in the lower class (roughly the bottom third), the middle class (the middle third), and the upper class (the top third).

Union Membership. Because union membership might tap into political engagement other than turning out to vote and might show a sense of trust in unionized labor and engagement in civil society, this variable is also used in both analyses. NES asked respondents whether they or anyone in their household is a member of a union, and I code this as a dummy variable where 1 = someone is in the union and 0 = nobody in the household is in a union.

Employed. Like union membership, being employed is a way to be a functioning member of society and connected to a larger social organization. To measure this particular type of social connectivity, I create a dummy variable where 1 = respondent is full-time employed, a homemaker, a student, or retired and 0 = respondent is unemployed, temporarily laid off, on strike, or disabled. These correspond with the eight choices presented by NES. I chose this dichotomous distinction because the first four categories all represent a connection to the social system, in terms of contributing to the status quo, which the latter four do not. More specifically, homemakers contribute to their families through unpaid labor. In this schema, students and retired individuals are merely at different time periods in their lives, but are likely similarly-minded in terms of employment values. To contrast, the unemployed, laid off, disabled, and those on strike might be less engaged with mainstream employment.

Political Party. One's political party might influence voting and political trust as well, and therefore I code for political affiliation. Respondents were asked "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?" While NES provides seven categories (coding both strong and weak Democrat or Republican and having separate codes for Independent Democrat, Independent, and Independent Republican), I collapse weak and strong Democrat and Republican into their respective political parties, and code all three Independent types as just Independent. With this recoding, I am left with a dummy set which represents Democrats, Independents, and Republicans.

Political Interest. Because it makes sense that individuals with more of an interest in politics will vote more, and because there might be some relationship between one's interest in politics and the amount that they trust the government, I include a measure of political interest in both analyses. Respondents were asked whether they were not much interested, somewhat interested or very much interested in politics and campaigns during that year's election. I thus used NES' breakdown to create a three-category dummy set representing individuals who are not interested, somewhat interested, and very much interested in politics.

Religious Service Attendance. NES asks respondents how often they attend religious services, apart from special events like weddings, baptisms, or funerals and give five choices for respondents. I treat this variable as ordinal in the analysis of self-reported voting, with 1 = Never and 5 = Weekly. However, because of a curvilinear relationship where those who sometimes attend church have the most trust, and those on the other end have lower levels, it is treated as three dummy variables in the analysis of political trust, as never attending church, sometimes attending church (which includes respondents who state they attend church once or twice a year, less than once a month, or two or three times a month), and attending church weekly.

Notably, and similarly to voting, people tend to over-report church attendance in surveys (Hadaway, Marler and Chaves 1993, 1998). Even so, this measure at least taps into people who think they are "the type of people to go to church" and thus is still associated with some element of religious connection (Chaves and Stephens 2003).

Religious Affiliation. NES provides codes religions affiliation in many ways. For instance, NES provides a dichotomous affiliation variable (i.e., Protestant or not) or a three-group variable (i.e., Protestant, Catholic, or Other). However, scholars suggest that it is more useful to break down religious affiliations in a way that allows for comparisons to be drawn between religious groups in the same faith (Kluegel 1980; Roof and McKinney 1987; Sherkat 2012, 2014). Thus, I do not use one of NES' coding classifications but construct my own based on Sherkat's (2014) thirteen-group coding scheme, which provides a careful separation of religious affiliation categories based on "tradition, denominational similarity, and ethnicity" (Sherkat 2014). NES provides the respondent's self-reported religious affiliation and details each of the 100-plus codes in an appendix.

While ideally the full thirteen-group scheme would be useful here, due to small sample sizes for some groups (e.g., Unitarians and Jews), I have merged some of these affiliations to create seven unique groups in the dummy set of religious affiliation: 1) Liberal Protestants (e.g., mainstream affiliations such as Presbyterians and Episcopalians) 2) moderate Protestant (e.g., slightly more conservative denominations such as Methodists, Lutherans, Northern Baptists, and reformed groups), 3) sectarian Protestant (Southern Baptists, Pentecostals, and other independent or fundamentalist groups), 4) Other Protestants (Unitarians, non-affiliated Christians, Eastern Orthodox groups) 5) Roman Catholics, 6) members of minority religious tradition (Jews,

Muslims, those of Eastern faiths), and 7) the religiously unaffiliated. For a complete list of religious denominations and their coding, see Appendix A.

Biblical Literalism. Taking into account how strongly one believes in the messages and tenets of one's faith is another important aspect of religiosity, but the NES' best measure of this belief is a question that asks about the inerrancy of the Bible. This question might mean something different for those who use a different text in their faith background, although these people are still included in the analysis. This, unfortunately, is another limitation of the data, although descriptive statistics reveal that members of minority religious traditions are no less likely to answer the question about biblical inerrancy than other groups. I code this in three dummy variables as respondents claiming the Bible is the actual Word of God and is to be taken literally, respondents claiming the Bible is inspired by God, and respondents claiming that the Bible is a book of fables.

4. Level-2 Control Variables Used in Both Analyses

Next, I include four state-level characteristics in both analyses, as well as a measure of time. While state-level variation is controlled for in MLM, including these variables affords us the opportunity to provide these models with greater specificity. The first two measures assess a state's level of modern urbanity and household income, and the lattermost two assess a state's and citizenry's overarching ideology in a way that expands on Elazar's earlier (1970, 1984) tripartite scheme of moralistic, traditionalistic, and individualistic states. Finally, I discuss my measure of time, the year of the analysis, and the fact that I treat it is a random effect, in which the effect of time is allowed to vary state-by-state (Gelman 2005).

Modern Urbanity Index. To tap into state-level influences on voting and citizen trust, I originally planned to separately include measures of state diversity, income inequality, percent

living in cities, population, and percent college educated. However, initial analyses indicated a high level of multicollinearity between these variables. While multicollinearity could be ignored if there were no substantive impacts on the results of the analysis, I found relatively high VIFs for population (*VIF* = 6.46), percent college-educated (*VIF* = 6.86), and Gini coefficient (*VIF* = 5.09). While these values do not reach the widely-cited threshold of a problematic *VIF* of 10 or higher, others have argued for omitting variables with *VIF*s higher than four, especially for aggregate variables (O'Brien 2007), and thus I erred on the side of a more conservative assessment. Constructing this index and running the models led to no VIFs higher than 2, which is well within reason. Furthermore, given the complexity of the analysis, I was interested in reducing degrees of freedom by reducing the number of state-level variables, and this index worked well in that undertaking.

To start, I assessed the ability of these variables to be used in an index. In analyses not shown, I first examined a correlation matrix of level-2 characteristics, and found that all of the variables in this index are highly correlated between the .600 and .800 level. That encouraged me to investigate whether these variables could be included in the same index using the process of principal components, or factor, analysis (Land, McCall and Cohen 1990). This analysis confirmed the idea that each of these variables could be constructed into one index. Specifically, the eigenvalue of the first factor in the analysis scored a 2.63, and is the only factor that reached an eigenvalue above 1 (the second component only reached an eigenvalue of .43). Furthermore, as is clear on a screeplot, there is a significant break between the first component and the latter four. This first factor also explained 92% of the variance of all of the variables combined.

Notably, all variables loaded on one factor, with every variable scoring above .6 in the factor loading. The smallest loading was .684 for the percent college educated, and the highest

was .767 for the percent living in cities, and these values are within a reasonable range of each other to add them together without weighting them beforehand. With this knowledge, I standardized the mean-centered scores for each of the variables and created a simple summed index. I did this because there was no missing data for any of the variables, because they all somewhat equally tap into ideas of modern urbanity as evidenced by the small range of factor loadings, and because, for each variable, a higher score indicates more urbanization. Finally, in the index, the Cronbach's Alpha for these five values is a high .848.

In naming the index, I based it in part on the fact that factors are often named based on the variables with the highest loading. Even though all of these factors load highly, percent living in cities had the highest loading factor, and thus it became clear to include some reference to urbanization in its title (Land, McCall and Cohen 1990). This was also the case with the diversity index, percent college educated, and the state population, all of which are associated with more urban environments. Further, the inclusion of income inequality as a positive correlate of the index, and the fact that income inequality has increased in modern times, encouraged me to name the index with that in mind. Thus, modern urbanity seemed to be the best descriptor of the underlying mechanisms.

All told, a methodological issue uncovered a complicated series of reciprocal relationships among these variables, indicating the importance of taking their effects as a whole, and not as piece-meal variables. As Land, McCall and Cohen (1990:954) caution, "attempts to estimate separate effects for individual elements of resource deprivation/affluence may be tenuous at best and misleading at worst." In much the same way, I found the underlying state-level score presented here to be a more accurate descriptor of overarching, modern urbanity than any individual measure.

To be more explicit, the elements that I standardized and added to make this index are as follows.

1) Diversity Index. To assess racial diversity, I use data from the U.S. Census for racial categories from each year by state. I construct this diversity index by subtracting the proportion squared of each racial category in the state from one. So, if a state were 100% of one racial group, the diversity index equation would be $1 - 1^2 = 0$. If a state were made up of ten different racial groups with 10% of the population each, the score would be .90. Thus, the higher a state scores on the diversity index, the more diverse the population.

This variable is easily constructed for 1980 and 2000 which are the only two census years in this sample. For the other years, I construct a variable for that year's diversity index based on the change in score from the census measures using a process called linear interpolation. For example, if the diversity index changed from .50 in 1980 to .60 in 1990, I would find the difference between the two variables (.60 - .50) and arrive at .10. Then, I would divide that number by ten (the number of years between measures) and arrive at .01. Next, I would simply add that number to the original diversity index score for each year between the measures (so, for 1984, the diversity index score would be .54 and for 1988 the diversity index score would be .58). I did this for each decade, and I continued the change from 2000 to 2010 up to 2012, because there is no measure for 2020 yet. The problem with this measure is that it assumes a linear growth in diversity between each intermittent year and the census measurements before and after that year, but I believe it has the potential to more accurately measure the change in each year than simply controlling for the past census year's score.

2) Percent Living in Cities. This measure reflects the proportion of the state's population that lives in urban areas for each year. A historical dataset of this information is available from

Iowa State University as part of the Iowa Community Indicators Program at http://www.icip.iastate.edu/tables/population/urban-pct-states. The percentage of urban and rural citizens is only available at the decennial level, because it is produced by the U.S. Census. Therefore, I calculated the percent change for each state per year using linear interpolation in the same way as the diversity index.

3) Income Inequality. The measure of income inequality is a state's Gini coefficient for each year. The Gini coefficient is a common measure of income inequality, where the lower the number, the more equally wealth is distributed among people. A score of 0 translates to complete and total economic equality (e.g., ten people in a group of ten each have $100) and a score of 1 translates to complete and total economic inequality (e.g., one person in a group of ten has $1,000 while everyone else has zero). Gini coefficients were obtained from the U.S. State Level Income Inequality database compiled by Mark Frank of Sam Houston State University (http://www.shsu.edu/eco_mwf/inequality.html) (Frank 2009, 2014).

*4) Population. T*his variable also comes from the U.S. Census annual population estimates. This is included with the idea that citizens who live in states with more people may differ in terms of voting turnout and political trust than those in less populated states. Furthermore, increasing population is a sign of a state's level of urbanization.

5) Percent College Educated. Similarly, research suggests that a more educated population has higher percentages of voters than states with lower levels of education, so I have a measure of the percentage of citizens with at least a bachelor's degree in each state per year. This data comes from the U.S. State Level Income Inequality database as well. (http://www.shsu.edu/eco_mwf/inequality.html) (Frank 2014).

Average Household Income. Finally, and separately from the modern urbanity index, I include average household income with the idea that if a citizen lives in a state with a higher average household income, that citizen will be more likely to vote. This is operationalized as the median household income in each state per year, in 2014 dollars. Data for 1984 through 2012 come from the single-year estimates from the Census' American Community Survey. For 1980, the median household income for each state comes from the U.S. Census, and I adjusted it for 2014 dollars using the Consumer Price Index inflation calculator from the Bureau of Labor Statistics (http://www.bls.gov/data/inflation_calculator.htm).

Notably, average household income might also be considered a good measure of modern urbanity. However, I do not include it in the modern urbanity index because 1) it reduces the Cronbach's alpha from the index from .848 to .802, and 2) the relationship between household income and corruption rate is notably different than the other measures of modern urbanity in analyses not shown here. Specifically, while all other measures are positively correlated with corruption rate in the bivariate analyses, household income is not significant.

Liberal Citizenry and Liberal Government. The final two measures used in each analysis are citizen and government ideology scores as constructed by Berry et al. (1998), last updated in March of 2015. These scores are based on assessments of voting habits of citizens and elected officials for each state, which are then measured in such a way as to gauge ideologies as more conservative or more liberal. Updated to reflect the most current reports from scores presented by the interest groups Americans for Democratic Action (ADA) and the AFL-CIO Committee on Political Education (COPE) which actually score the ideologies of key players taken into account in these data, these measures allow researchers to move beyond Elazar's (1970, 1984) tripartite scheme of traditionalistic, moralistic, and individualistic states (Berry et al. 1998).

Specifically, the liberal citizenry measure is constructed based on first measuring the ideological position of each member of Congress in each year based on ADA and COPE ratings and then estimating,

> citizen ideology in each district of a state using the ideology score for the district's incumbent, the estimated ideology score for a challenger (or hypothetical challenger) to the incumbent, and election results that presumably reflect ideological divisions in the electorate. (Berry et al. 1998:330-1)

In other words, Berry et al. (1998) assess candidates' ideologies, and construct citizen ideology by measuring how much support candidates receive in the most recent election. For instance, if a staunchly conservative candidate wins a particular election by a landslide, the citizenry would be measured as conservative. As an approximate rule of thumb, the higher the score on Berry et al.'s (1998) index, the more liberal the citizenry's ideology. In this work, this variable is named *Liberal Citizenry*, to reflect that a higher score indicates a more liberal populace.

Next, Berry et al. (1998) identify government ideology by assigning ideology scores based on COPE and ADA scores for a state's governor, as well as those in each state's legislature. They then weight each score in a way that reflects the amount of power held by each actor, where the governor's ideology makes up fifty percent of the final ideology score, and the rest of the actors make up the other fifty. For this ideology as well, a higher score reflects a more liberal government of a particular state; lower scores indicate a more conservative government.

Beyond reflecting a more nuanced view of a citizenry's politics than Elazar's (1974, 1980) measure, these scores allow for state and citizen ideology to vary over time (Berry et al. 1998). Berry et al. (1998) also report high reliability (where their measures of state and citizen ideology closely align with reports from ADA and COPE), as well as report validity of these measures (by finding similar results to earlier peer-reviewed studies when using their measures in place of Elazar (1970, 1984) and others' measures of citizen and government ideology).

Similar to above, in this work, this variable is renamed *Liberal Government*, once again indicating that a higher score on this variable indicates a more liberal government.

Year. Finally, I include a measure of time, which I treat as a random effect. What this means is that I allow the effect of time, as measured in year, to vary state-by-state, which is the way random effects are structured in MLM (Gelman 2005). For multilevel models, Gelman (2005:21) furthers this distinction by stating that one can, "define effects (or coefficients) in a multilevel model as constant if they are identical for all groups in a population and varying if they are allowed to differ from group to group." In terms of statistical interpretation, the effect of time will have random slopes for both self-reported voting and government trust in the later analyses (Huber 2013). According to Huber (2013), this is the appropriate way to model time (which could be considered a level-three variable, as all states are necessarily nested in a particular year at each time of the NES survey), and it is a way of modeling that approximates a three-level MLM when there are not sufficient respondents for each state per year to nest individuals within states within years. While there are many tough choices to make in deciding whether a variable should be fixed (as all but this one are) or random, I ultimately treat this as a random variable to test whether we might have reason to believe that certain states are more or less impacted by the year than others, which would then allow us to better interpret the effect of time on an individual's self-reported voting or government trust. If year were treated as a fixed effect, it would not allow for the state variance to be accounted for. Just how much variation there is in the effect of time for states remains to be seen, but setting up the model in this way allows us to see if we should expect a differential impact of time state-by-state.

For the analysis of self-reported voting, the range of years is from 1980 – 2012, with the measure centered on the mean of 1996. For the analysis of government trust, the range of years is

1992 – 2008, and I center the variable on the mean year of 1997. The raw number and percent total of people in each sample are presented in Table 3.1.

In Table 3.1, the first three columns refer to the years of analysis for the analysis of self-reported voting in Chapter 4. The modal year is 2012, with almost a quarter of the respondents coming from this wave of the survey. Notably, only about 5,000 respondents come from midterm election years. In multivariate analyses, just as I center all continuous and ordinal variables on their grand means, I center year around the median year of 1996, and treat the variable as an ordinal one (ranging from 1980 to 2012) in the analysis. It should be noted that although many respondents came from 2012, I found that the models performed substantively the same with and without 2012 included in the analysis.

Compared to the analysis of self-reported voting, it is somewhat disappointing to only be able to use six years and about 6,700 respondents. However, one useful aspect of this analysis is that it contains three years that citizens were surveyed about their trust in the government immediately following Republican regimes (Bush I in 1992 and Bush II in 2004 and 2008), and three years that followed years helmed by Clinton's Democrat administration (1996, 1998 and 2000). Also, the sample is somewhat evenly-split with 44% surveyed after a Republican administration and the remaining 56% asked after a Democrat's regime.

5. *Variables Unique to Voting*

Next, I present the four variables that are unique to the analysis of voting that present specific structural elements of elections that are necessary to control for.

Table 3.1. NES Sample Size by Year for Self-Reported Voting

a. Voting			b. Political Trust		
Year	N	%	Year	N	%
1980	1,137	5.24	--	--	--
1984	1,591	7.34	--	--	--
1986	1,831	8.45	--	--	--
1988	1,499	6.91	--	--	--
1990	1,689	7.79	--	--	--
1992	1,836	8.47	1992	1,784	26.55
1994	1,492	6.88	--	--	--
1996	1,349	6.22	1996	1,331	19.81
1998	1,140	5.26	1998	1,117	16.62
2000	1,223	5.64	2000	1,175	17.49
2004	894	4.12	2004	877	13.05
2008	911	4.20	2008	435	6.47
2012	5,086	23.46	--	--	--
Total	21,678	99.98^	Total	6,718	99.99^

^ Rounding Errors lead to percent totals slightly less than 100%

No-fault Absentee Ballots. Both this measure and the next tap into the idea that citizens will be more likely to vote if they live in a state that makes it easier to do so. One way that states can do this is by allowing individuals to use for any reason – not only if people are temporarily away from the state or their district. The dummy coding here is 1 = states allow citizens to obtain absentee ballots for any reason, 0 = absentee ballots are only allowed for specific reasons.

For information on whether a state allows no-fault absentee ballots, I use the PEW Non-Precinct Voting in the States dataset compiled by Cemenska et al. (2009) (http://www.pewtrusts.org/en/research-and-analysis/reports/0001/01/01/nonprecinct-place-voting). Cemenska et al. (2009) reviewed state laws up to 2008, including whether a state had no-fault absentee ballots as well as in which year such laws were implemented. Because their report ended in 2008 and this analysis extends into 2012, I turn to the National Conference of State Legislatures (NCSL) (2015a) for a breakdown of states' absentee policies for 2012 (http://www.ncsl.org/research/elections-and-campaigns/absentee-and-early-voting.aspx).

Election Day Registration. For the measure of Election Day registration I use a dummy variable for whether or not a state has same-day voting registration laws (coded as 1) or not (coded as 0) during a particular year. Like no-fault absentee ballots, this information is available at the website of the NCSL (2015b) (http://www.ncsl.org/research/elections-and-campaigns/same-day-registration.aspx).

Closeness Proportion. In line with previous literature that suggests individuals know when elections will be close in their state and thus will be more likely to vote, I include a measure of the closeness of the presidential election. I subtract the percentage of votes for the second-place presidential candidate from the winning candidate, using data from every presidential election (data from http://www.uselectionatlas.org). For non-presidential election years, I use the previous years' closeness proportion, because of data availability and with the idea that states that have closer races between presidential candidates might be more politically "up for grabs" between the candidates in midterm elections as well.

6. Variables Unique to Trust in Government

In addition to the twenty individual- and state-level variables used in both analyses, I include four individual-level variables unique to the analysis of political trust.

Social Trust Index. One of the theoretical perspectives I take to understand the factors that affect political trust is that individuals are socialized to be more or less trusting in general. Previous scholars have used one's generalized level of social trust to tap into a preconceived trust orientation. NES asks three questions that can be used to assess an individual's generalized social trust:

- Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?

1. Can't be too careful
2. Most people can be trusted

- Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?

    1. Just look out for themselves
    2. Try to be helpful

- Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?

    1. Would take advantage
    2. Would try to be fair

Because these questions measure similar concepts, I create an index to reduce the used degrees of freedom. I coded each response as follows: 0 is the less trusting option, and 1 is the more trusting one. I then added the responses together to arrive at the trust score. However, I allowed respondents to be missing on one or two of the responses to maximize the number of people included in the analysis. For instance, if an individual answered two out of the three questions, then that person still was included in this analysis in the following way: individuals were coded as missing if they responded to none of the questions, 0 (not trusting at all) if they responded to at least one of the questions with a not trusting answer and no questions with a trusting answer, 1 (somewhat trusting) if they responded to one of the questions with a trusting answer, 2 (more trusting than not) if they responded to two of the questions in a trusting manner, and 3 (very trusting) if they responded to all three questions in a trusting manner. While this coding scheme treats some missing data as "not trusting," there were very few missing responses in the final, and so I do not think that these results would be significantly affected by this coding process. Furthermore, the index performed moderately well with a Cronbach's alpha of .733.

Better Off. Another theoretical perspective states that individuals make rational assessments of their place in life, comparing it to the past, and that assessment affects their trust

in the government: if they think they're better off, they're more likely to trust. To assess this belief, I use a question which asks respondents whether they were better off compared to last year. Similar, to my measure of political interest, I treat this variable as a dummy-set, of whether respondents report they were better off than they were the year before, the same as they were a year prior, or worse off to tap into their assessments of their current economic standings.

Approve. To assess one's rational perspective on an administration, I also include a question which asks an individual: "Do you approve or disapprove of the way that [the president] is handling his job as President?" The thought here is that individuals who disapprove of the president might be less trustful of the government as a whole. I code this as a simple dummy variable where 1 = approve and 0 = does not approve.

Political Allegiance. Finally, there is also reason to believe that individuals are more trusting of governments with which they identify politically. To assess this claim, I constructed a measure of political allegiance. I assigned a 0 if an individual identified as a member of the same political party as the current president and the current governor, I assigned a 1 if an individual identified as a member of the same political party as *either* the current president or current governor (but not both), and I assigned a score of 2 if an individual said they were in a political party that was different from either their governor or the current president. Thus, the lower one scores on this scale, the more of an affiliation one has with the political powers. Once again, this variable will be treated as ordinal in this analysis, although ordinal variables should at least have five levels.

Political affiliation of an individual came from their response to political party above. Data for the party orientation of presidents came from common knowledge, and data for state governors' affiliations came from the National Governors Association (www.nga.org).

SECTION II: ANALYTIC STRATEGY

My primary analysis takes the form of multilevel modeling (MLM): specifically, multilevel logistic regression for the examination of self-reported voting, and multilevel linear regression for the examination of political trust. While MLM is known by many names, including hierarchical linear modeling, random-effects modeling, and mixed-effects modeling, I prefer the term multilevel modeling because it parsimoniously speaks to the idea that the researcher takes into account the nested nature of the variables.

MLM allows one to test for level-1 effects (at the individual level), and level-2 effects (at the group level) to find effects of key level-1 variables while holding constant any effects that might affect group members as a whole. Essentially, this method allows us to see both within-group variation (i.e., variation that occurs in one level of analysis) and between-group variation (i.e., variation that occurs in a level of analysis that is hierarchically above the first unit) (Bickel 2007; Steenbergen and Jones 2002; Rabe-Hesketh and Skrondal 2008). Thus, MLM takes into account the reasons why groups differ as a whole from a grand mean, and *then* how individual factors affect people in those groups (Snijders and Bosker 1999).

The classic example used to describe MLM in the literature, and indeed one of its more common uses, is in investigating students. MLM allows the researcher to take into account the impact of which school a student attends and still make meaningful inferences based on each student's demographic and individual-level characteristics. This allows researchers to compare effects on students at different schools, taking into account the overarching effect of school membership. For each of these analysis, that means I examine respondents' scores controlling for literature-derived variables at the individual level (e.g., race, religious affiliation, and political

party), while simultaneously controlling for the effect that state-level variables (like the political culture of a respondent's state) might have on individual's outcome.

Simply put, MLM allows us to see the determinants of individual actions (turning out to vote), while taking into consideration the nested nature of other potential covariates at the state-level. Essentially, MLM controls for the influence of the group that an individual is a part of, and *then* analyzes the impact of variables on that individual (Rabe-Hesketh and Skrondal 2008; Snijders and Bosker 1999). For nested data, this is a superior method than simple logistic or linear regression, because those methods assume that individuals are entirely independent in the ways that variables affect them (Bickel 2007). What MLM allows us to do is see the actual impact of a variable (say, being a Republican) on a certain individual aspect (say, political trust), controlling for the group that he or she is a part of (say, the state of Montana or Washington). MLM assesses an overall level of political trust for each state, and *then* tests how an individual's demographic characteristics affect his or her trust scores.

Figure 3.2 presents in graphic form the way that the multilevel structure works. Here, the level-1 variables are the individual characteristics and the level-2 variables are the characteristics of the state. This model first takes into account how states differ from the grand mean, and then examines how individual characteristics influence individuals.

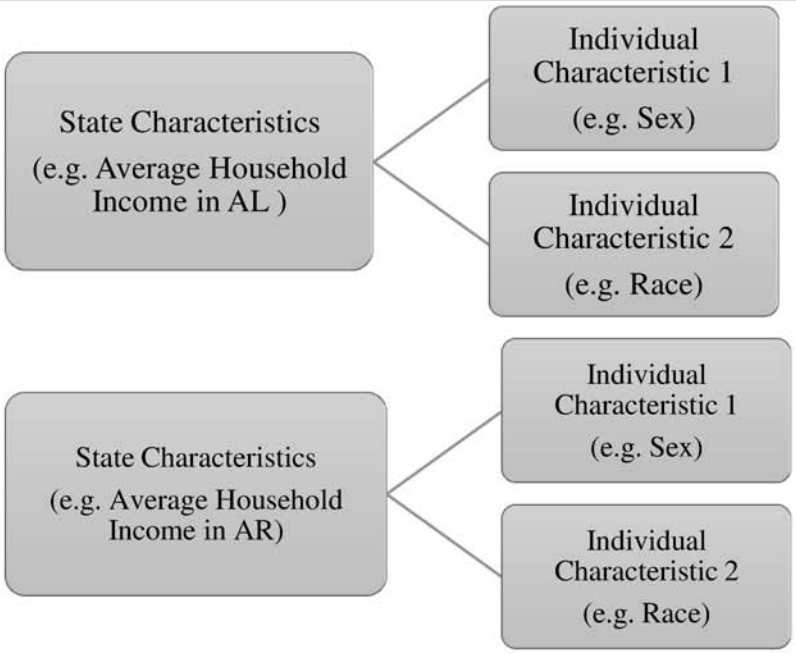

Figure 3.2. Multilevel Structure of Data

The analytic strategy is as follows: for each research question, I determine the sample and examined the descriptive statistics to get a sense of the individuals under analysis. I then run appropriate tests to establish bivariate relationships between the dependent variable(s) and independent variables. I then run MLM models (logistic regression for self-reported voting, and linear regression for political trust). Notably, for my analysis of self-reported voting, I run post-hoc tests that show partial proportional odds models, which allow us to interpret the effects of key independent variables in a similar way to the coefficients produced by the linear regression used to analyze government trust. Finally, I also test for cross-level interactions between state corruption rates and several key social demographic variables to see if their effects on voting and trust vary by state corruption level (Jaccard 2001).

In conclusion, MLM is relatively easy to explain on a theoretical level, but the practice of interpreting results and running models correctly to account for this between- and within-unit

variation is complicated, as evidenced by the wide array of material available on the subject (e.g., Bickel 2007; Snijders and Bosker 1999; Steenbergen and Jones 2002; Rabe-Hesketh and Skrondal 2008). To assert the robustness of the models which I employ in the following chapters, I now turn to a description of these preliminary steps and analyses.

Preliminary Analyses

Before beginning the analyses presented in the next two chapters, I conducted initial tests to assure that the data were input into the dataset correctly, double-checking data and immersing myself in the NES codebook. While specific issues related to the models are discussed in their respective chapters, I also took more general steps in establishing the sample size, addressing issues of missing data, weighting multilevel models, testing for the effect of outliers, and centering all continuous and ordinal variables on the grand mean for their specific analysis.

Establishing Sample Size. As shown, I used a variety of sources to construct this dataset, with all individual-level characteristics coming from the NES dataset itself. From the onset, I started with data that spanned from 1980 – 2013 (data are available from 1948, but PIN availability began in 1980). Thus, I originally I began with an N of seventeen time periods (NES surveys the population every two years), fifty states, and 31,502 individuals. However, due to various data limitations, as outlined below, the final sample is smaller.

First, I lost some years of analysis. For the most part, NES conducted their survey every two years (i.e., 1980, 1982, 1984…). However, in 2006 NES administered a pilot study to test new questions and check for methodological issues with older questions, and in 2010 they spent their resources conducting the second wave of another panel study started in 2008-2009, and did not issue their normal time series study. Thus, while there are seventeen two-year periods in the time from 1980 to 2012, there are no comparable data for 2006 and 2010. Furthermore, NES'

question wording about household income for 2002 was incomparable to the rest of the years. Therefore, 2002 is omitted from the analysis. With that, the number of time periods decreased to fourteen (1980, 1982, 1984, 1986, 1988, 1990, 1992, 1994, 1996, 1998, 2000, 2004, 2008, and 2012) for the model of self-reported voting. Some of these years (1984, 1986, and 2008) implemented split-level designs and only asked variables of interest in half of the surveys, so those years, while included, had smaller sample sizes.

The sample size in the analysis of political trust took an even greater hit: the questions used to construct the index of social trust were only asked in the usable years of 1992, 1996, 1998, 2000, 2004, and 2008. Thus, the analysis of political trust only speaks to results from 1992 – 2008, a period of time that is about half that for self-reported voting.

Next, while initially the goal was to use all fifty states in each model, complications with data availability precluded that option. In the full sample for voting, for example, there were only three observations (i.e., individuals who had data reported for all variables of interest) for Alaska, seven for Hawaii, ten for Vermont, and 15 for South Dakota. While there are many opinions on the necessary sample size to make meaningful interpretations using MLM (Bickel 2009), I believe that having this low of an N for each of these states would be problematic. For instance, if I were to use the individual vote response in this sample, everybody in Alaska would be coded as saying they had voted. Furthermore, South Dakota had only two individuals report that they had not voted, and although the overall sample size for that state is 15, which might be an acceptable number for MLM (Bickel 2009), I erred on the side of safety, using the rule of thumb that I needed enough respondents to ensure the ability to conduct other statistical analyses that breaks as few formal rules as possible in other analyses (e.g., to run chi square tests each cell needs to have at least five cases, and South Dakota would have violated that). For that reason, the

final sample size of states for voting is 46, which excludes Vermont, Hawaii, South Dakota, and Alaska. Similarly, for the model of political trust, the final sample size of states is 41 (excluding the aforementioned four as well as Delaware, Idaho, Maine, Montana, and Rhode Island) due to low cell sizes in the much smaller complete sample.

With these omissions of years and certain states, the N for the NES dataset for voting was 26,091 and the N for political trust was 9,646. However, due to unique combinations of missing data for each study, the N's for each analysis are somewhat smaller: 21,678 for the first, and 6,719 for the latter.

Missing Data. Missing data are a major issue with survey research: while there is not too much concern with data that is randomly missing from a sample, if there are systematic differences in missing data (e.g., women are much less likely to report voting turnout than men), then the data are biased and conclusions drawn from such data should be made with caution. Fortunately, I did not notice systematic differences in missing data, and preliminary analyses in SPSS suggested that the data were indeed missing at random. Thus, I chose to use complete-cases data for each analysis instead of using multiple imputation, and I outline why in more detail next.

Voting. There were 26,091 cases in this analysis, including those with missing values. After listwise deletion, the N was 21,678, a difference of 4,323 cases, or 16.57% of the full sample. This is not a small portion of the full sample, so care should be taken in assessing why these individuals are missing.

Thus, I tested whether results would differ using multiple imputation to estimate the effects that would occur if I had no missing data. Thus, I employed multiple imputation to "fill in" the missing data points with values that would impute the most likely values for each missing

cell based on a person's characteristics (White, Royston and Wood 2010). To do this, I used the `mi mvn` function in Stata 13, which allows for the input of variables of different types (i.e., continuous and ordinal) to be imputed at the same time. While some of the variables to be imputed were dichotomous (e.g., married or not married), researchers suggest that `mi mvn` is robust for certain bivariate relationships as well, especially with large samples (Allison 2001; Lee and Carlin 2010). Furthermore, I did not impute values for state-level variables, because there were no missing data for state-level characteristics, nor for the dependent variable of self-reported voting, which is in line with scholars' recommendations (e.g., Allison 2001). All in all, this allowed me to reach the full N of 26,091.

Upon review of multilevel logistic regression models with both imputed and complete-cases data, I found that most of the odds-ratios were approximately the same, and that most variables maintained the same level of statistical significance. However, there were four variables that changed in their level of statistical significance in the multiply imputed and complete cases models, and two of these were state-level variables. To be clear, the coefficients for percent urban and no-fault absentee ballot changed in their significance due to controlling for newly-imputed level-1 data, not because these variables had any values themselves that were imputed. These results are shown in Table 3.2.

Table 3.2. Statistical Significance from Imputed to Complete-Cases Model for Self-reported Voting

	Imputed N = 26,091	Complete Cases N = 21,678
Religious Affiliation[a]		
Moderate Protestants	H.O. ***	H.O. **
Catholics	H.O. ***	H.O. **
Unaffiliated	H.O. **	H.O. *
Percent Urban	H.O. **	H.O. ***
No-Fault Absentee Voting	H.O. **	H.O. ns

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$
Reference Category: [a] Liberal Protestants
H.O.: Higher Odds; L.O.: Lower Odds; ns = not significant

Differences in significance for religious affiliation and urbanity are not too problematic, because they are both just more or less significant, and still substantively *mean* the same thing upon interpretation in both data sets. However, the coefficient for no-fault absentee voting is significant in the imputed data analysis but not in the complete-case analysis. This is an issue because no-fault absentee voting has been empirically established to have a small, but significant, positive effect on voter turnout in the aggregate (Leighley and Nagler 2009, 2014).

Specifically, in their analysis of voter turnout from 1972 - 2008, Leighley and Nagler (2009) make the argument that no-fault absentee voting impacts citizen likelihood of voting. However, they 1) do not include many state-level demographic variables I include like household income, ideology, or factors associated with urbanization, and 2) do not employ MLM to allow for untapped state variation. Furthermore, some pay more attention to the sign and strength of the coefficient rather than statistical significance in comparing imputation vs. complete-cases models, and the coefficients for the multiple-imputed absentee ballot variable (1.169, $p < .01$) and that of the complete cases absentee ballot variable (1. 123, *ns*) are quite similar. Thus, while

we should take this discrepancy into consideration, I err on the side of the more conservative complete-cases dataset rather than using the dataset constructed with multiple imputation. All in all, any transformation to the data one undertakes might affect the findings, and I prefer to do as few transformations as possible, unless absolutely necessary.

Government Trust. For this analysis, the N for the full model with missing data is 6,719. This seems like a dramatic amount of missing data compared to the full sample of voting. However, a great deal of this is explained by the fact that questions relating to social trust were asked only in 1992, 1996, 1998, 2000, 2004, and 2008. In fact, the complete number of cases, after removing years where questions simply were not asked was 9,654. Still, this is about a thirty percent reduction in the sample size.

As above, I first saw that there were no systemically missing data, and then I used `mi mvn` in Stata 13 to discern whether there were substantive differences in results for imputed and non-imputed models. While some demographic characteristics did indeed change their significance (e.g., union membership became significant in the multiple imputation dataset at $p < .05$, believing the Bible is written by man and not by God went from significant at $p < .05$ to $p < .01$, and year went from $p < .001$ to $p < .05$), no other variables changed. Furthermore, union membership, barely achieves significance at $p < .041$. Thus, I once again chose to err on the side of the complete-cases models, on the off-chance that the imputed data would influence the analyses in subtle ways, and because I found no reason in SPSS descriptive panels or Stata 13 to think that data were non-randomly missing.

Weighting Multilevel Models

While NES provides a probability survey weight for each variable, which should be included in regression models in order to help alleviate biased results (Carle 2009), in practice it

is difficult to know exactly how to interpret that weight with respect MLM. This is because NES does not include state-level and individual-level weights, and includes a single measure to weight the probability of including a certain individual (usually by racial background). Thus, NES provides a weight for simple regression models, but 1) does not provide information about how to use it in MLM, 2) the literature on MLM is unclear as to how to use weights, and 3) the necessity of using weights for surveys over the course of time in MLM, especially when controlling for a variety of demographic statistics, is debatable and not common in practice.

First, NES does not document how to use its survey weights for MLM. Additionally, scholars have not agreed upon an adequate method for including *any* weights in MLM (Asparouhov 2006; Carle 2009; Goldstein 2011). For instance, some argue that the best practice is to include the reciprocals of probability weights as covariates in the equation itself (Rabe-Hesketh and Skrondal 2006, 2008), whereas others suggest the importance of scaling the weights and including them as weights in regression analyses (Pfefferman et al. 1998). Because weighting in MLM is so new (Chantala, Blanchette and Suchindran 2011), what is more common is to offer theoretical guidance on the use of sampling weights, but no practical method of utilizing them in software packages (Goldstein 2011).

Furthermore, in practice with a large sample such as the one in this analysis, the tangible differences between weighted and unweighted models may not be very different at all. In a simulation exercise, Carle (2009) ran weighted and unweighted ML models for individuals (N = 750) nested within states (N = 51, including Washington DC), and found similar results in terms of standard errors and variance components for every model. Carle (2009) suggests that with sufficiently large Ns of individuals within each cluster (i.e., individuals with a state), weighting

might not be an issue. Carle (2009) recommends an N > 20 for each cluster, and the lowest cluster size I have is 19, which is awfully close.

Practically, scholars have developed other methods to help account for not being able to properly model weights. For instance, Osorio, Tovar and Rathmann (2014) report including sufficient demographic characteristics in their multilevel (ML) model to essentially control for any variation that weighting the samples would have helped explain. In his analysis of NES data, Richey (2010) also includes several demographic characteristics and does not mention the impact of survey weights (although his analysis looks at panel survey data, and not the cross-sectional time series data under investigation here). In other words, weighting data in MLM might not be ultimately necessary for understanding relationships as long as enough variation is adequately controlled for in the statistical models themselves.

Thus, we are left with a situation where the correct way to weight models in MLM is unknown and is currently being researched (Gelman 2007), and including weights might not be necessary. Thus, in the following chapters, I present unweighted results. However, readers should be clear that, though this is the best available data and methodology to answer the research questions currently available, there is much work to be done for MLM to deal with complicated, long-term survey data.

Outliers and Residuals

I also conducted tests to ensure that there were no outliers or influential cases, which might unduly influence results. I first ran full ML logistic regression models for voting and ML regression models for political trust and built residual plots (Long and Freese 2014). Residual plots show how much a case's predicted value (i.e., the intercept of the equation) is different than what its actual value is, after controlling for all necessary variables. For the most part, residuals

were clustered together, and I investigated the few cases that appeared to be outside of the average range for coding errors that might explain this larger-than-average difference, and found none. I also ran models with the outlying cases omitted, and found no substantive differences. Thus, I found no evidence that outliers unduly influence the results in the coming chapters.

Centering Continuous and Ordinal Variables

One final note is necessary about the transformation of variables in both analyses. MLM is best served when continuous and ordinal variables are centered on their means, in order to present results in a meaningful way (Hayes 2006). Specifically, while the results would be statistically correct if these variables were not grand mean centered, they would be difficult to interpret. When centered on the mean, coefficients in the following analyses reflect the impact of a certain independent variable when an individual has the average amount of all other variables, and the constant or intercept reflects the likelihood of voting or the mean score on government trust when all continuous or ordinal variables are held at their averages. Furthermore, when running interaction effects, it is important to center these variables at their mean for accurate modeling (Jaccard 2001).

Thus, for chapters four and five, I use "grand mean centering" for the continuous and ordinal variables, which is accomplished "by subtracting the sample mean from each case's score on the variable" (Hayes 2006:397). This means that the continuous and ordinal variables used in each chapter are centered around a slightly different mean, based on the mean of the particular sample used in that chapter. In descriptive statistics, however, I present the non-centered scores on such variables.

SECTION III: ANALYTIC TOOL

Finally, I should speak briefly on the analytic tools used herein. While a benefit of MLM is that many statistical software packages are now able to run some type of MLM, some software provide better tools and instruction than others. Thus, for the analyses, I use Stata 13. Stata 13 provides a valuable set of tools for initial analyses commonly available in most statistical packages (e.g., descriptive statistics, bivariate relationships, simple logistic and linear regressions). It also allows the user to conduct ML linear regression using the command `mixed`, which is similar to an option in later SPSS packages. However, to my knowledge, Stata 13 is one of the few statistical packages that allows the user to run ML logistic regression, using the command `meqrlogit`. Finally, Long and Freese (2014) provide syntax for Stata 13 to produce predicted probabilities following ML logistic regression, using their statistical package and the command `mchange`. For these reasons, I opted for Stata 13 for all analyses.

CHAPTER 4

THE EFFECT OF STATE POLITICAL CORRUPTION ON SELF-REPORTED VOTING

In this chapter, I present the results of the examination of the effect of state political corruption prosecutions on self-reported voting in five parts. First, I detail the descriptive statistics of the complete sample of 21,678 individuals. Next, I present the bivariate relationships between the independent variables and self-reported voting, also to establish baseline associations from which to compare later findings.

In the third section, I present ML logistic regression results of the effect of the independent variables on self-reported voting using odds ratios, finding support for the first hypothesis. In the fourth, I present these results using predicted probabilities, a useful way to show the relative impact of each of the statistically significant independent variables in a way that is more easily interpretable than the standard logistic regression output of odds ratios. Here, I show just how small of an effect a change in a corruption rate really has.

I then use section five to present interaction effects testing whether the effects of income, education, race, and religion depend on the level of corruption in the state (testing hypotheses H1a – H1d). In these cross-level interaction models, I find that state-level corruption does not affect the influence of any of the key demographic variables, in contrast to these hypotheses. I conclude in section six with brief summarizing remarks.

SECTION I: DESCRIPTIVE STATISTICS

To begin, I present descriptive statistics in Table 4.1.

Table 4.1. Descriptive Statistics of NES Sample, 1980 – 2012, N = 21,678

		Mean	SD	Min.	Max.
Level-1 Variables					
Vote		0.697	0.459	0	1
Female		0.538	0.499	0	1
Married		0.540	0.498	0	1
Race					
White		0.732	0.443	0	1
Black		0.133	0.340	0	1
Hispanic		0.102	0.303	0	1
Other		0.032	0.177	0	1
Age		46.274	17.234	17	94
Own Home		0.662	0.473	0	1
Education				0	1
8 grades or less		0.056	0.229	0	1
9 – 12 grades		0.099	0.299	0	1
High school diploma		0.292	0.455	0	1
HS diploma + non-academic training		0.037	0.189	0	1
Some college		0.262	0.440	0	1
Bachelors' degree		0.165	0.371	0	1
Advanced degree		0.089	0.284	0	1
Income	*1: 0 – 16 Percentile* *2: 17-33* *3: 34 – 67* *4: 68 – 95* *5: 96 – 100*	2.856	1.136	1	5
Union Membership		0.180	0.383	0	1
Employed		0.901	0.299	0	1
Political Party					
Democrat		0.380	0.485	0	1
Independent		0.361	0.480	0	1
Republican		0.260	0.438	0	1
Political Interest					
Not at all Interested		0.181	0.385	0	1
Somewhat Interested		0.450	0.498	0	1
Very Interested		0.366	0.482	0	1
Religious Service Attendance	*1: Never* *2: A few times a year* *3: Once or twice a month* *4: Almost every week* *5: Weekly*	2.818	1.587	1	5
Religious Affiliation					
Liberal Protestant		0.071	0.257	0	1
Moderate Protestant		0.345	0.475	0	1

Table 4.1. (continued)

	Mean	SD	Min.	Max.
Sectarian Protestant	0.171	0.376	0	1
Catholics	0.244	0.430	0	1
Minority Religious Traditions	0.027	0.162	0	1
Unaffiliated	0.142	0.349	0	1
Biblical Literalism				
"The Bible is the Word of God"	0.403	0.490	0	1
"The Bible is inspired by God"	0.455	0.498	0	1
"The Bible is a book of fables"	0.142	0.349	0	1
Level-2 Variables				
Modern Urbanity Index	.115	.801	-5.932	4.084
Diversity Index	33.338	13.397	1.990	62.098
Percent Urban	76.729	12.236	36.100	95.120
Income Inequality (GINI)	57.653	5.659	45.180	76.010
Population (in 100,000s)	113.442	93.060	4.534	379.999
Percent College Educated	14.832	3.912	6.118	26.448
Average Household Income (in 10,000s)	5.318	0.765	3.353	7.497
Liberal Government	52.957	24.360	2.581	95.190
Liberal Citizenry	49.453	12.598	9.074	93.912
Presidential Year	0.716	0.451	0	1
No-fault Absentee	0.312	0.463	0	1
Election Day Reg.	0.066	0.248	0	1
Closeness Percentage	12.598	9.074	0.010	85.920
Corruption Rate	0.307	0.221	0	2.583

The total sample size is 21,678 individuals, with about 70% of individuals reporting to have voted in the previous election. As mentioned before, this number is likely inflated by people who state that they voted when they actually did not. For the key independent variable, the average corruption rate is .307, which directly translates to about one political corruption prosecution for every 325,000 citizens living in a state.

Demographically, just over half of the sample (54%) is made up of women, and about the same percentage are married. The sample is predominantly white (73%), followed by black (13.3%), Hispanic (10.2%), and then another race (3.2%). The average age of an individual in the sample is about 46 years old. About 66% are homeowners. About 25% of the sample has a bachelor's or advanced degree, with the modal response of a respondent as having a high school diploma (29%). Also, the average reported household income falls somewhere between the second and third quintile, which, for example, suggests that most respondents earn between $17,000 and $70,000 in 2004 (the most recent income distribution offered by the ANES). Finally, about 18% of the sample is either in a union or has a family member who is a member of a union, and about 90% are either employed, retired, students, or homemakers.

Politically, the sample is relatively evenly distributed, though it does lean slightly left. Approximately 38% of respondents identify as Democrats, 36% as political Independents and 26% as Republican. About 18% report that they have a low level of interest in political matters, 45% state that they are somewhat interested, and 37% report having a high level of interest in political matters.

In terms of religiosity, the average citizen attends church somewhere between a few times a year and once or twice a month. Most of the sample (34.5%) are classified as moderate Protestants, followed by Catholics at 24%, sectarian Protestants at 17%, and then the religiously unaffiliated at about 14%. Liberal Protestants (7.1%) and members of minority religious traditions (2.7%) make up the rest. Finally, 40% report that the Bible is the literal Word of God, 46% report that the Bible is inspired by God but written by men, and the rest (14%) state that the Bible is a book of fables.

Moving towards level-2 characteristics, the average citizen lives in a state that has a population of 1.13 million, about 77% of citizens living in urban areas, and where about 15% are college-educated. The average citizen's state scores about 33 on the diversity index, which indicates a moderate amount of ethnic diversity (with 0 reflecting no diversity and 100 reflecting complete diversity), and the average citizen lives in a state that scores about 58 on the Gini index, signifying that there is slightly more income inequality than there is income equality. As mentioned above, all of these variables were standardized and then summed into the modern urbanity index used in later analyses, which has a mean score of .115.

Moving to other level-2 demographic characteristics, the average citizen lives in a state with an average household income (in 2014 dollars) of $53,000. The average liberal government score and the average liberal citizenry score are both pretty close to 50 (on a scale of 0 – 100), although governments are slightly more liberal on the index than citizens, scoring an average of 52.96 compared to 49.45.

Election-wise, about seven-tenths of this sample consists of citizens answering questions about a previous presidential election. Also, about 31% of individuals report living in a state with no-fault absentee ballots. In other words, while there are 27 states that now have no-fault absentee ballot laws currently (i.e., 54% of the states that make up the U.S. in 2016), this figure reflects the fact that such laws were adopted at different points in time over the past 30 years. Thus, the 31% figure consists of individuals living in states with no-fault absentee ballot laws now and in the past (when the number was much smaller). Similarly, while 20% of states have enacted Election Day registration (EDR) laws by 2012, only 6.6% report living in such a state in this sample. Finally, the average citizen lives in a state that, for the nearest past presidential

election, had about 12 percentage points differ between the winner and the runner-up for that election.

SECTION II: BIVARIATE ANALYSES

Now that we have an idea of what the data look like, we can move towards understanding the bivariate relationships between the independent variables and self-reported voting. Because different tables require certain formatting, I separate dichotomous, multi-categorical, and ordinal/ratio bivariate relationships throughout this section.

To begin, I present the level-1 bivariate relationships between dichotomous variables and reported voting. This provides us with an understanding of any baseline relationships that exist before including other predictors of the dependent variable. In the following table, a statistically significant chi square indicates that the observed frequencies of people in either category are significantly different from what we would expect if there were no association between the independent variables and voting. Cramer's V presents the strength of the relationship (weak, moderate, or strong), and ranges from 0 (no relationship) to 1 (a perfect relationship). Notably, because this sample size is so large, it should be noted that considering the strength of the coefficient is more important than the statistical significance. Specifically, even small effect sizes will be detected as significant, and percentage point differences less than approximately five percentage points should be interpreted with caution. Table 4.2 presents these bivariate relationships.

Table 4.2. Bivariate Analyses of Level-1 Dichotomous Variables and Self-Reported Voting

			χ^2	Cramer's V
Not Voted	6,561			
Column Percent	*30.27*			
Voted	15,117			
Column Percent	*69.73*			
	Male	Female		
Not Voted	2,915	3,646	12.103***	.024
Column percent	*29.09*	*31.27*		
Vote	7,104	8,013		
Column percent	*70.91*	*68.73*		
Total	10,019	11,659		
	Not Married	Married		
Not Voted	3,565	2,996	271.814***	.112
Column percent	*35.85*	*25.53*		
Vote	6,378	8,739		
Column percent	*64.15*	*74.47*		
Total	9,943	11,735		
	Non-Homeowner	Owns a Home		
Not Voted	3,229	3,332	995.295***	.214
Column percent	*44.03*	*23.23*		
Vote	4,104	11,013		
Column percent	*55.97*	*76.77*		
Total	7,333	14,345		
	Not in a Union	In a Union		
Not Voted	5,589	972	63.968***	.054
Column percent	*31.43*	*24.84*		
Vote	12,191	2,926		
Column percent	*68.57*	*75.06*		
Total	17,780	3,898		
	Not Employed	Employed		
Not Voted	952	5,609	222.069***	.101
Column percent	*44.28*	*28.72*		
Vote	1,198	13,919		
Column percent	*55.72*	*71.28*		
Total	2,150	19,528		

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$, N = 21,768

This table shows that all of the level-1 dichotomous independent variables are statistically significantly associated with self-reported voting. Not controlling for any other factors, men report voting at a higher percentage than women; married folks report voting more than non-married people; homeowners report voting more than renters; people with a family member in a union report voting more than non-union families; and those employed report voting more than the unemployed, disabled, or those on strike.

However, union membership, and gender have Cramer's Vs of less than .100, signifying a very weak or almost non-existent relationship. Once again, the difference here between women and men (about two percentage points) and union-membership and non-union membership (about six and a half percentage points) should be interpreted with caution. Gender, specifically, could very likely be considered significant due to chance, because of the large sample size. Measures associated with residential and familial stability – being married, being employed, and owning a home – have slightly stronger, but still weak, relationships with Cramer's Vs of .101 (employment), .112 (marriage), and .214 (owning a home, which is actually moderate). All chi square test statistics are statistically significant at the $p < .001$ level.

Moving on, Table 4.3 presents the level-1 relationships between the three-or-more-category categorical variables and self-reported voting. Here, I present adjusted standardized residuals, which point to which groups are different from what the expected value would be, chi square test statistics, which show whether there are significant differences between groups, and Cramer's Vs, which assess the strength of the relationship between the independent and dependent variables.

Table 4.3. Bivariate Analyses of Level-1 Multi-Categorical Variables and Self-Reported Voting

					χ^2	Cramer's V
	White	Black	Hispanic	Other		
Not Voted	4,516	897	879	269	139.391***	.080
Vote	11,355	1,990	1,337	235		
ASR	*9.60*	*-1.01*	*-10.17*	*-4.67*		
Total	15,781	2,887	2,216	704		
	Democrat	Independent	Republican			
Not Voted	2,153	3,131	1,277		568.661***	.162
Vote	6,075	4,697	4,345			
ASR	*10.28*	*-23.45*	*14.32*			
Total	8,228	7,828	5,622			
	< 8	9 – 12	H.S.	H.S.+	SC	BA	AD		
Not Voted	591	1,169	2,413	175	1,472	534	207	1,800.00***	.288
Vote	613	979	3,927	633	4,212	3,040	1,713		
ASR	*-14.63*	*-25.68*	*-16.06*	*5.43*	*8.35*	*21.82*	*19.47*		
Total	1,204	2,148	6,340	808	5,684	3,574	1,920		
	Not Much Political Interest	Some Political Interest	Very Much Interested						
Not Voted	2,518	2,905	1,138					3,100.000***	.379
Vote	1,416	6,849	6,852						
ASR	*-50.92*	*1.40*	*39.23*						
Total	3,934	9,754	7,990						
	Lib. Prot.	Mod. Prot.	Sect. Prot.	Cath.	Min. Rel. Trad.	Unaff.			
Not Voted	304	2,319	1,190	1,492	99	1,157		228.552***	.103
Vote	1,240	5,154	2,514	3,806	484	1,919			
ASR	*9.39*	*-1.78*	*-2.71*	*3.84*	*7.08*	*-9.58*			
Total	1,544	7,473	3,704	5,298	583	3,076			
	Bible is the Word of God	Bible is Inspired by God	Bible is a Book of Fables						
Not Voted	3,168	2,593	800					251.947***	.108
Vote	5,560	7,273	2,284						
ASR	*-15.87*	*11.67*	*5.65*						
Total	8,728	9,866	3,084						

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$

Note: ASR = Adjusted Standardized Residual. N = 21,768

< 8 = No High School Education, 9-12 = High School Dropout, HS = High School Graduate, HS+ = High School Education plus some non-college training, SC = Some College, BA = Bachelor's Degree

Table 4.3 shows that there are significant differences in self-reported voting for those in different political parties, racial groups, education divisions, different religions. Not controlling for any other factors, whites have a higher percentage of self-reported voting than do blacks, Hispanics, or others. Republicans have the highest percentage of self-reported voting, Democrats have the second highest, and Independents report voting the least. Members of minority religious traditions have the highest percentage of self-reported voting, and the religiously unaffiliated have the lowest percentage.

Importantly, for categorical variables with more than two choices (political party, race, and religious affiliation), the chi square test can only tell us whether the variables are independent from each other or not. To see which groups are different from the expected values, one must look to the adjusted standard residuals. Groups with residuals greater than two or less than negative two could be interpreted as having a notable difference from what the expected value of the group would be, if there were no relationship between group membership and self-reported voting.

However, for categorical variables that could be interpreted as being ordered in some way (e.g., political interest ranging from less interested to very interested), the Cramer's V indicates that the association between self-reported voting and people's attention paid to politics is quite strong. Explicitly, there is a strong Cramer's V of .379 associated with political interest and self-reported voting, and the adjusted standardized residuals indicate that there are many more people saying they voted who have a strong interest in politics than we would expect if there were no influence of political interested on self-reported voting. This indicates that those interested in politics are more likely to report voting, not controlling for other factors. This is similarly present in educational categories: those with less education (i.e., those with a high school degree

or less) report voting less than we would expect if there were no association between education and self-reported voting, and those with more education (i.e., those with some college or higher) report voting more than we would expect.

Next, I present relationships between level-1 ordinal and ratio-level independent variables and self-reported voting using a modified t-test approach. While a typical t-test examines the effect of a dichotomous independent variable on a dependent continuous variable, to see if two groups' means significantly differ, this modified approach switches the independent and dependent variables, because the dependent variable is dichotomous. Specifically, the two categories are "vote" and "did not vote" and these t-tests will show if the mean of an ordinal or ratio-level variable (age, education, family household income, and religious service attendance) is significantly different between self-reported voters and non-voters. Results of these t-tests are presented in Table 4.4.

Table 4.4. t-tests of Self-reported Voting on Level-1 Ordinal and Ratio Variables, N = 21,768

	Mean	Std. Err.	Std. Dev.	[95% C. I.]		Mean Difference	Cohen's *d*	t-score
Age								
Not Voted	40.439	.209	16.962	40.028	40.849	-8.367	.498	-33.689***
Voted	48.806	.136	16.729	48.540	49.073			
Family Household Income								
Not Voted	2.505	.014	1.126	2.478	2.533	-.503	.451	-30.560***
Voted	3.008	.009	1.107	2.990	3.026			
Religious Service Attendance								
Not Voted	2.444	.018	1.487	2.407	2.479	-.538	.343	-23.202***
Voted	2.980	.013	1.601	2.955	3.006			

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$

Once again, there are statistically significant differences between voters and non-voters for all four of these relationships, all at the level of $p < .001$. The mean age of a voter in this sample is 49, whereas the mean age of a nonvoter is 40, indicating that voters are older than nonvoters not controlling for other factors. Self-reported voters have slightly higher average incomes than non-voters, and attend church slightly more often. However, as shown by the Cohen's *d*, the difference in religious service attendance between voters and nonvoters is a relatively small .34 standard deviations (Cohen 1988). For family household income, those who vote have an average income of that in the third quintile (which ranges from the 34 – 67 percentile), whereas non-voters earn somewhere in the 17 – 33 percentile, or second quintile. Nonvoters attend church less regularly than voters as well, not controlling for other factors, with voters reporting attending approximately once or twice a month, and nonvoters reporting attending just a few times a year.

Everything considered, all of the level-1 variables are significantly associated in some way with individuals reporting to have voted in the bivariate analysis. Now, we turn to level-2 variables that the literature suggests should affect voting. As above, I first examine the level-2 dichotomous variables for the 21,768 person sample, which all tap into the structural factors of particular elections that might influence voting, and their relationships to voter turnout using chi square test statistics and Cramer's Vs first. These results are presented in Table 4.5.

Table 4.5 shows that self-reported voting is higher in presidential election years than midterm election years, with about 53% of the sample claiming to have voted in midterm elections and a whopping 77% claiming to have voted in presidential elections ($p < .001$). This relationship also has a moderate association with a Cramer's V of .234. This table also shows that election reforms like allowing Election Day registration and allowing voters to use absentee ballots without justification increase self-reported voting, not controlling for other factors: higher percentages of potential voters report turning out in states with election-day registration and no-fault absentee ballots. However, both of those relationships, as shown by Cramer's V scores of less than .100, are relatively weak.

Table 4.5. Bivariate Analyses of Level-2 Dichotomous Categorical Variables, N = 21,768

			χ^2	Cramer's V
	Midterm Election	Presidential election		
Not Voted	2,913	3,648	1,200.000***	.234
C.P.	*47.35*	*23.50*		
Vote	3,239	11,878		
C.P.	*52.65*	*76.50*		
Total	6,152	15,526		
	No Absentee Ballots	Absentee Ballots		
Not Voted	4,983	1,578	223.041 ***	.101
C.P.	*33.40*	*23.34*		
Vote	9,935	5,182		
C.P.	*66.60*	*76.66*		
Total	14,918	6,760		
	No Election Day Registration	Election Day Registration		
Not Voted	6,290	271	90.294 ***	.065
C.P.	*31.05*	*19.07*		
Vote	13,967	1,150		
C.P.	*68.95*	*80.93*		
Total	20,257	1,421		

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$

Moving on, we can now see the bivariate relationships between ordinal and ratio-level independent variables and self-reported voting using the modified t-test approach. While of course all voters are drawn from the same pool of forty-six states, by looking at the average scores for states where individuals report voting and individuals report not voting, we begin to tap into the structural elements of states that affect turnout, beyond characteristics of elections. Table 4.6 documents these relationships, which showcase the different means for voters and non-voters on continuous variables.

Table 4.6. Bivariate Analyses of Voting on Level-2 Continuous Variables, N = 21,768

	Mean	Std. Err.	Std. Dev.	[95% C. I.]		Mean Difference	Cohen's *d*	t-score
Modern Urbanity Index								
Not Voted	.054	.009	.718	.037	.071	-.087	.109	-7.346***
Voted	.141	.007	.833	.128	.154			
Liberal Government Score								
Not Voted	54.616	.283	22.906	54.061	55.170	2.379	-.098	6.612***
Voted	52.237	.203	24.831	51.839	52.634			
Liberal Citizenry Score								
Not Voted	47.979	.158	12.787	47.669	48.288	-2.115	.167	-11.321***
Voted	50.091	.102	12.569	49.893	50.294			
Average Household Income								
Not Voted	5.251	.010	.771	5.232	5.270	-.096	.126	-8.530***
Voted	5.347	.006	.761	5.335	5.360			
Closeness Percentage								
Not Voted	12.908	.114	9.264	12.684	13.132	.444	-.049	3.313
Voted	12.464	.073	8.988	12.321	12.607			
Corruption Rate								
Not Voted	.331	.003	.244	.325	.337	.034	.152	10.313***
Voted	.297	.002	.210	.294	.300			

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$

In this final table of bivariate statistics, we see the statistical significance of almost every t-score in the table, although some of the average standard deviation differences are quite small and substantively not interpretable (e.g., for closeness percentage and a liberal government). First, the t-score for the modern urbanity index indicates that voters live in states that have a higher mean modern urbanity score (.141) than do non-voters (.054) and that this effect is significant at $p < .001$. In analysis not shown here, I find that means are significantly different

for voters and nonvoters in terms of the average diversity of a state, with the average diversity index score for voters being a point higher than the average state score for non-voters (indicating more diversity). Furthermore, states that are more urban, have larger populations, score higher on the Gini index, and have a higher percentage of college educated are all associated with voting ($p < .001$), at least not controlling for other factors. Thus, the modern urbanity index seems to reflect the trends of each underlying factor, at least in the bivariate case.

We also see that the mean average household income is higher in states where individuals report voting than where citizens report voting. Voters' mean scores for a liberal government and liberal citizenry suggest that voting is more common in states with conservative governments though this effect size is very small, but also more common in states with liberal citizenries, which has the largest standard deviation gap of these level-2 variables, although that is quite small as well. Also, we see the effect of the corruption rate in the bivariate case: voters' states have a lower average corruption rate (.297) than non-voters' states (.331), a difference which nets a t-score of 10.313 ($p < .001$) and a Cohen's d of .152.

Finally, while the means of the groups reflect that those citizens report voting more in states where the closeness percentage is smaller (indicating closer elections), these results are not significant here. As mentioned in the literature, scholars suggest that closer elections *should* encourage voter turnout because 1) citizens will think their vote counts more in a closer election, and 2) nervous politicians will increase their campaign efforts in order to ensure enough voters come out to vote for them. While it is too early to rule out the idea that close races encourage self-reported voting, because these relationships do not control for any other variables, it is certainly a relationship to look out for in later models.

In sum, all bivariate relationships, with the exception of closeness percentage and self-reported voting, reached statistical significance, not controlling for other factors. Now, we turn to MLM to test whether these variables' relationships hold when controlling for each others' effects.

SECTION III: MULTILEVEL LOGISTIC MODELS

Like standard logistic regression, MLM presents the effect of a certain independent variable on a dependent variable, controlling for the effects of all other independent variables in the model. However, MLM acknowledges that the data are nested: individuals live in states with certain characteristics, and that fact should be accounted for in the model. While comparability diagnostics presented below are novel to this particular method, and thus require some explanation, the interpretations of odds ratios are the same as with standard logistic regression.

My ML models are presented in Table 4.7 and 4.7a. Table 4.7 provides Models 1 and 2: a baseline model and another model where the corruption rate is the sole independent variable. Table 4.7a presents Models 3 through 6, which test the rest of the independent variables, starting with demographic characteristics (Model 3), the addition of time (Model 4), the rest of the state-level characteristics (Model 5), and a model that includes all of the variables in Model 5 and reintroduces corruption rate (Model 6). As mentioned in the introduction, I first describe my results in terms of Odds Ratios here, and then (in table 4.8) in terms of predicted probabilities.

To begin, Models 1 and 2 are presented in Table 4.7.

Table 4.7. Multilevel Logistic Regression Empty Model and with Corruption Rate, N = 21,678

	(1) Odds Ratio (95% C.I.)	(2) Odds Ratio (95% C.I.)
Constant	2.457*** (2.207 -2.735)	2.472*** (2.220 -2.752)
Corruption Rate	--	.563*** (.481-.660)
Reliability Diagnostics		
Log Likelihood	-13110.784	-13085.358
df difference		1
χ^2 score		25.426 ***
Constant SD	.331 (.259-.424)	.331 (.256-.428)
State ICC	.032 (.020-.052)	.032 (.020-.053)
McKelvey and Zavoina's Pseudo-R^2	0.000	0.005

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$

In Model 1 in Table 4.7, also known as the empty or "null" model (Hayes 2006), we see that the odds of voting are about 2.5 times higher than the odds of not voting. Model 1 also provides us with some baseline characteristics from which to compare later models: 1) log likelihood, 2) constant standard deviation, 3) state intraclass correlation coefficient (ICC), and 4) McKelvey and Zavoina's Pseudo-R^2.

First, and similar to standard logistic regression, the smaller the log-likelihood of a model, the better the fit of that model, and one can use the baseline log-likelihood to compare later log-likelihoods. The constant standard deviation and state ICC are unique to MLM. The constant standard deviation refers to the standard deviation of the grand mean (i.e., the constant or intercept of the model as a whole). The larger the standard deviation, the larger the amount of variation there is in individuals' means with reference to the grand mean. Thus, the smaller this score, the better the model does at narrowing individual variance around the mean.

A more useful statistic for MLM in many ways is the state ICC. This measure is a way to show how much of the variance of individual scores is explained by state-level characteristics. For instance, a certain percent of the variation in an individual's likelihood of voting might be due to unmeasured characteristics of the state that individual is in (for instance, the effect of a state-wide radio campaign on individuals to encourage them to get out and vote). Thus, some of the within-unit (i.e., people-level) variance is due to the variance of states. The state ICC is calculated as (Subject Variance / [Subject Variance + Residual Variance]) (Rabe-Hesketh and Skrondal 2008). In this case, the model suggests that 3.2% of the variance in individuals' self-reported voting is explained by something having to do with the attributes of states, not with the attributes of citizens themselves. While this number is small, multilevel models are still appropriate because a) individuals are nested in states and b) "values of ICC as small as .05 can invalidate hypotheses tests and confidence intervals when MLM is not used" (Hayes 2006:394).

Finally, the McKelvey and Zavoina's Pseudo-R^2 is an attempt to show the amount of variation explained by a particular model, taking into account the nested nature of the models. Scholars debate on the ways to interpret Pseudo-R^2s in MLM, and about R^2s in general (Hayes 2006; Krueger and Lewis-Beck 2007; Nakagawa and Schielzeth 2013), but the best current practice is to understand the McKelvey and Zavoina's Pseudo-R^2 as an assessment of the "goodness-of-fit" of a model, which takes into consideration the variation that occurs at the individual- and state-level (Nakagawa and Schielzeth 2013). However, care should be taken in interpreting this as a concrete measure of fit like in standard regression equations: there is still much to be understood about the efficacy of this test statistic, and other measures like variance and ICC might be more meaningful (Bickel 2007). Understandably, the McKelvey and Zavoina's Pseudo-R^2 is zero in a model with no independent variables.

Model 2 in Table 4.7 shows the impact of including the corruption rate in ML logistic regression models. Here, we see that for every one unit increase in the corruption rate, the odds of voting decrease by a factor of .563 ($p < .001$). The chi square score for the difference in Model 1 and Model 2's log-likelihoods shows that adding the corruption rate into the model provides a statistically significant better fit. However, the effect of the corruption rate (a state-level, or level-2, characteristic of the data) does not do much in explaining some of the untapped variation that exists at the state level: both the constant standard deviation and the state ICC are not reduced by the introduction of a corruption rate, suggesting that all of the state-level variation is still present. Finally, adding the corruption rate in and of itself does little to explain the variation in the model as a whole: McKelvey and Zavoina's Pseudo-R^2 suggests that only .5% of the entire variation is explained. Table 4.7a continues this work and presents results for Models 3 through 6.

Model 3 in Table 4.7a shows the effects of the individual-level, demographic characteristics on self-reported voting. Most of the results reflect the idea that individuals with higher social status and with more ties to their areas are more likely to vote than those without such ties or with lower social statuses. For instance, married individuals, those who own their home, the employed (or retired, or student, or homemaker), and those with at least one member of their family having membership in a union have higher odds of reporting to have voted, controlling for other factors. Also controlling for other factors, blacks have higher odds of voting than whites, but Hispanics and other minority groups have lower odds, with all else being equal. In terms of political orientation, Independents are less likely to have reported voting than Democrats, but there is no difference between the odds of self-reporting to have voted for Republicans compared to Democrats, net all else.

Table 4.7a. Multilevel Logistic Regression Coefficients Predicting Self-Reported Voting, N = 21,678

	(3) Odds Ratio (95% C.I.)	(4) Odds Ratio (95% C.I.)	(5) Odds Ratio (95% C.I.)	(6) Odds Ratio (95% C.I.)
Constant	1.258	1.319***	.686*	.704***
	(.946-1.673)	(.991-1.757)	(.511-.923)	(.524-.946)
Female	1.046	1.045	1.027	1.026
	(.974-1.123)	(.974-1.122)	(.956-1.104)	(.954-1.103)
Married	1.147***	1.155***	1.145***	1.145**
	(1.061-1.239)	(1.068-1.248)	(1.058-1.239)	(1.058-1.239)
Race[a]				
Black	1.487***	1.450***	1.493***	1.492***
	(1.321-1.674)	(1.287-1.634)	(1.323-1.686)	(1.322-1.685)
Hispanic	.916	.871*	.859*	.859*
	(.808-1.039)	(.766-.990)	(.754-.980)	(.753-.979)
Other	.632***	.609***	.583***	.582***
	(.523-.763)	(.503-.736)	(.481-.706)	(.480-.705)
Age	1.028***	1.027***	1.028***	1.028***
	(1.025-1.030)	(1.024-1.029)	(1.025-1.030)	(1.025-1.030)
Own Home	1.633***	1.628***	1.622***	1.623***
	(1.506-1.771)	(1.501-1.765)	(1.494-1.762)	(1.494-1.762)
Education[b]				
8th Grade or Less	.181***	.193***	.177***	.179***
	(.145-.226)	(.155-.242)	(.141-.223)	(.142-.223)
9-12 Grade	.198***	.203***	.190***	.191***
	(.163-.240)	(.167-.247)	(.157-.232)	(.156-.233)
High School Grad	.362***	.365***	.361***	.362***
	(.305-.430)	(.308-.434)	(.303-.430)	(.304-.431)
High School Plus	.691**	.719**	.471**	.470**
	(.540-.883)	(.562-.922)	(.366-.607)	(.365-.604)
Some College	.575***	.579***	.554***	.555***
	(.484-.683)	(.487-.688)	(.465-.660)	(.465-.662)
Bachelor's Degree	.926	.930	.907	.907
	(.770-1.114)	(.773-1.119)	(.751-1.095)	(.752-1.096)
Income	1.130***	1.135***	1.156***	1.157***
	(1.086-1.175)	(1.091-1.181)	(1.110-1.204)	(1.111-1.204)
Union Membership	1.154**	1.163**	1.141**	1.139**
	(1.048-1.270)	(1.056-1.281)	(1.034-1.259)	(1.032-1.256)
Employed	1.214***	1.229***	1.248***	1.247***
	(1.085-1.358)	(1.098-1.376)	(1.112-1.399)	(1.112-1.399)
Political Party[c]				
Independent	.576***	.572***	.545***	.545***
	(.531-.625)	(.527-.621)	(.501-.593)	(.501-.593)
Republican	.937	.933	.941	.942
	(.850-1.034)	(.846-1.029)	(.852-1.040)	(.852-1.041)

Table 4.7a. (continued)

	(3) Odds Ratio (95% C.I.)	(4) Odds Ratio (95% C.I.)	(5) Odds Ratio (95% C.I.)	(6) Odds Ratio (95% C.I.)
Political Interest[d]				
Somewhat Interested	3.341*** (3.060-3.648)	3.243*** (2.967-3.545)	3.055*** (2.789-3.348)	3.053*** (2.787-3.346)
Very Interested	7.057*** (6.374-7.814)	6.706*** (6.039-7.446)	5.992*** (5.381-6.671)	5.983*** (5.373-6.661)
Religious Service Attendance	1.215*** (1.183-1.249)	1.218*** (1.186-1.251)	1.237*** (1.204-1.272)	1.238*** (1.204-1.272)
Religious Affiliation[e]				
Moderate Protestants	.816* (.695-.957)	.807** (.688-.947)	.787** (.668-.927)	.787** (.664-.922)
Sectarian Protestants	.707** (.597-.837)	.682** (.575-.807)	.665** (.558-.792)	.661*** (.555-.787)
Catholics	.852 (.723-1.005)	.837* (.710-.987)	.828* (.700-.981)	.825* (.697-.977)
Min. Rel. Traditions	1.075 (.808-1.429)	1.064 (.800-1.414)	1.079 (.806-1.444)	1.070 (.799-1.432)
Unaffiliated	.851 (.713-1.016)	.816* (.683-.975)	.805* (.670-.966)	.799* (.666-.960)
Biblical Literalism[f]				
"The Bible is inspired by God"	1.237*** (1.138-1.343)	1.221*** (1.123-1.326)	1.231*** (1.131-1.340)	1.223*** (1.130-1.338)
"The Bible is a book of Fables"	1.398*** (1.231-1.588)	1.366*** (1.202-1.552)	1.396*** (1.226-1.591)	1.394*** (1.224-1.588)
Year	--	1.008*** (1.003-1.012)	.991*** (.985-.996)	.992** (.986-.997)
Modern Urbanity Index	--	--	.997 (.926-1.072)	.991 (.925-1.062)
Average Household Income (in 10,000s)	--	--	.970 (.888-1.059)	.961 (.882-1.048)
Liberal Government	--	--	.999 (.996-1.001)	.999 (.996-1.001)
Liberal Citizenry	--	--	1.004 (.999 -1.009)	1.005 (.999-1.011)
Presidential Year	--	--	2.846*** (2.620-3.093)	2.803*** (2.579-3.048)
No-fault Absentee Ballots	--	--	1.146* (1.004-1.311)	1.122 (.979-1.798)
Election Day Registration	--	--	1.359 (.998-1.850)	1.327 (.979-1.798)
Closeness Proportion	--	--	.999 (.994-1.003)	.999 (.994-1.003)

Table 4.7a. (continued)

	(3) Odds Ratio (95% C.I.)	(4) Odds Ratio (95% C.I.)	(5) Odds Ratio (95% C.I.)	(6) Odds Ratio (95% C.I.)
Corruption Rate	--	--	--	.742** (.612-.946)
Reliability Diagnostics				
Log Likelihood	-10,082.232	-10,074.589	-9,732.772	-9,728.176
df difference	28	1	12	1
χ^2 difference	3,028.552***	7.64**	341.81***	4.59*
Constant SD	.292 (.224-.381)	.291 (.223-.381)	.262 (.193-.355)	.255 (.188-.347)
Year SD		.004 (.001-.023)	.003 (.000-.053)	.002 (.000-.891)
State ICC	.026 (.015-.042)	.025 (.015-.042)	.020 (.011-.037)	.019 (.011-.035)
McKelvey and Zavoina's Pseudo-R^2	.369	.370	.411	.412

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$
Reference Categories: [a] White, [b] Advanced Degree, [c] Democrat, [d] Not Interested, [e] Liberal Protestant, [f] "The Bible is the Word of God"

In terms of religiosity, all religious affiliation groups have higher odds of self-reported voting than sectarian Protestant, the reference group. This finding is somewhat mirrored in the fact that those who believe the Bible is inspired by God and those who believe the Bible is just a book of fables have higher odds of voting than those who believe the Bible is the literal word of God, which is a view more in line with sectarian affiliations. While these views are closely intertwined, the fact that they both achieve statistical significance in the model (and that post-reports show no issues with multicollinearity) suggests that there is something unique about each of these elements in explaining self-reported voting. Also, an increase in church attendance is associated with higher odds of having voted. Finally, though this test statistic should be

interpreted with caution (Long and Freese 2013), the Pseudo-R^2 suggests that about 37% of the variation in the model is explained by the included demographic characteristics.

Model 4 introduces the effect of time and offers a suggestion about whether the odds of reporting to have voted are actually on the decline. The introduction of year suggests that controlling for all of the individual-level variables, the odds of turning out to vote increase by a factor of 1.008 ($p < .001$) for each year. This suggests that people are more inclined to vote over time, controlling for other demographic factors. Thus, any declines that we might see are actually brought on by increased levels of other demographic characteristics that have a negative effect on the odds of reporting to have voted (like the rise of identifying as a political independent, or the declining rate of marriages).

It is important to repeat, however, that I allow the effect of time to vary by each year, treating it as a random-effect as opposed to a fixed-effect. In simple terms, variables with fixed effects are held constant in terms of how they affect individuals, while random effects are allowed to vary by group membership, which is, in this case, state citizenship (Gelman 2005). Thus, I have chosen to treat the effect of year as random, or "varying," to see if there is much variation in the effect of time state-to-state (e.g., if the passing of time in Illinois affected voting habits more or less than that same time passing in Wisconsin). While treating this as a random-effect does not let us to see which years have more or less of an impact, the fact that the standard deviation for year is a quite-low .004 suggests that the effect of time is relatively stable across states. Furthermore, time does little to explain any of the variation in state-level self-reported voting as evidenced by approximately the same scores on constant standard deviation, state ICC, and the Pseudo-R^2 with the introduction of year as a measure, although it does improve the model as shown by a statistically significant chi square test statistic. However, this lack of

influence on standard deviation, ICC and Pseudo R^2 makes sense. Because, the effect that time has is allowed to vary from state-to-state, it *should* not explain away any of the variation left in the model. A fixed effect, on the other hand, would tell us how time affects individuals and would change the variation explained for those individuals just like every other type of fixed effect.

Model 5 introduces all state-level variables to the analysis, with the exception of the corruption rate. First and foremost, the introduction of state-level characteristics to the analysis did nothing to change the demographic associations. This indicates that these demographic characteristics are particularly robust: including state-level characteristics that might "explain away" these effects does nothing to change their relationship and effect size, roughly speaking. Clearly, it pays to understand individual-level factors as important predictors of self-reported voting.

In fact, the only two state-level variables with statistically significant effects on self-reported voting were 1) that presidential election years increase the odds of an individual reporting to have voted by a factor of about three ($p < .001$), compared to the odds of self-reported voting in midterm elections, and 2) the odds of an individual reporting to have voted increase by a factor of 1.15 ($p < .05$) if they live in a state with no-fault absentee ballots, both controlling for other factors. Because there is no relationship between how close an election is and self-reported voting, it is important to note that these effects hold in closer elections as well as elections where one candidate receives most of the voters' support.

Perhaps the more suggestive and interesting change that occurs when adding in these state-level characteristics, however, is that the effect of time maintains statistical significance, but reverses its sign. Here, the effect of time becomes negative, indicating that as time goes on,

and controlling for other factors, citizens are less likely to report having voted. The positive effect of time in the previous model, then, is most likely explained away by the increase in states allowing no-fault absentee ballots and election-day registration.

All told, the introduction of these variables adds to a statistically significantly better fit of the model (according to a chi square difference of 342 ($p < .001$)) and the Pseudo-R^2 of this model suggests about 41.1% of the variation in self-reported voting is explained – an increase of about four percentage points from the previous model.

Finally, Model 6 includes all of the previous variables and the state corruption rate. Model 6 shows that as the corruption rate increases, the odds of an individual self-reporting to have voted decrease. In other words, for every one-unit increase in the corruption rate, the odds of an individual reporting to have voted decrease by a factor of .742 ($p < .01$), controlling for all else. This signifies that corruption does have a statistically significant effect on self-reported voting and supports Hypothesis 1: Individuals who live in states with higher rates of corruption convictions will report voting less than citizens who live in states with lower levels of corruption convictions.

While this indicates that a state's level of corruption influences individual's likelihood of voting, it is important to point out that this effect is quite small. For instance, the Pseudo R^2 only changes by .1%, and the chi square difference score from the previous model is only borderline statistically significant. Other measures of comparability diagnostics also point to the small effect of including corruption in the model.

This raises the question: how does the effect of corruption compare to the effects of other variables used in this analysis? A good way to show the size of the effect of a variable used in ML logistic regression is by showing the predicted probabilities of variables, which I do next.

SECTION IV: PREDICTED PROBABILITIES

Despite their common use, odds ratios offer little guidance as to what the real impact of the variables they describe is. You learn how an independent variable increases or decreases the odds of a certain dependent variable occurring, but you cannot tell which effect is stronger or more impactful. In other words, the utility of odds ratios is limited because they do not show the magnitude of the change in a predicted outcome's probability (Long and Freese 2014).

A good way to show the actual impact of these results is by presenting the Average Marginal Effects (AMEs), a type of predicted probability, as I do in Table 4.8. AMEs assume that every variable, except for the one being explicitly examined, is held at its mean, and "[b]ecause it averages the effects across all cases in the sample, it can be interpreted as the average size of the effect in the sample" (Long and Freese 2014:245). Specifically, the analysis reports the actual difference that occurs in the predicted probability of self-reported voting for a one-unit increase in the independent variable under examination (for continuous variables, like education), or the difference that occurs in the predicted probability for one group as compared to a reference category (for a categorical variable, like race). For continuous variables, I also present the predicted probability difference for a change in one standard deviation, which is particularly useful if the one-unit change is particularly small, such as a single year of age.

For the sake of space, Table 4.8 presents the expected changes that would occur for only statistically significant relationships in Model 6. This table shows the ascertained, or "new," predicted probability (that is, the value after undergoing some sort of change, like the predicted probability of voting if you are connected to a union being .715), and compares that to the original predicted probability (e.g., the value before undergoing some change, like the predicted

probability of voting for people without union ties is .696) (Long and Freese 2014). The change in the score is reported in the final column (in this case a change of about .019, $p < .001$). When change scores do not add up to differences between old and new values, it is because of rounding errors.

Table 4.8 shows that the predicted probability for reporting to have voted, controlling for all other factors, and with those factors centered at their mean, is .700. That means, if somebody scored absolutely the average on all of the variables included in the analysis, their predicted likelihood of voting is .700, roughly translated to 70% odds of voting, rather than not voting. This makes sense, recalling that about 69.7% of people report voting in the past election.

Examining each variable in order allows us to see their actual impacts, and the comparative size of each impact. Of course, it is impossible to score the mean score on something like homeownership, because homeownership is a 0 or 1 variable. To see the real effects of these categorical variables, one must examine Table 4.8 a bit more deeply. Looking at the very first statistically significant dummy variable, that of being married, we see that if all other variables were kept at their means, an unmarried person would vote about 68.9% of the time. If that person were married, however, they would vote about 71% of the time. This reflects a difference of .020 predicted probability points. Another example is that being employed compared to being unemployed increases the predicted probability of voting by .034 points. And, looking at predicted probabilities allows us to compare effect sizes. In this case, the predicted probability of voting changes more if one gets a job than if one gets a spouse.

Table 4.8. Average Marginal Effects of Independent Variables on Self-Reported Voting, N = 21,678

		To (New Value)	From (Original Value)	Change
Average Predictions	.700			
Married	*vs Not Married*	.710	.689	.020***
Race	*Black vs White*	.751	.698	.053***
	Hispanic vs White	.673	.698	-.025*
	Other vs White	.611	.698	-.087***
	Hispanic vs Black	.673	.751	-.078***
	Other vs Black	.611	.751	-.140***
	Other vs Hispanic	.611	.673	-.062***
Age	*+1 year of age*	.704	.700	.004***
	+SD	.763	.700	.064***
Own Home	*vs Not Owning a Home*	.728	.654	.074***
Education	*Nine to twelve years vs less than eight*	.558	.545	.013
	High school graduate vs less than eight	.669	.545	.124***
	High school plus some training vs less than eight	.710	.545	.165***
	Some college vs less than eight	.735	.545	.190***
	BA vs less than eight	.800	.545	.256***
	Advanced degree vs less than eight	.812	.545	.267***
	High school graduate vs nine to twelve years	.669	.558	.111***
	High school plus some training vs nine to twelve years	.710	.558	.152***
	Some college vs nine to twelve years	.735	.558	.177***
	BA vs nine to twelve years	.800	.558	.243***
	Advanced degree vs nine to twelve years	.812	.558	.254***
	High school plus some training vs high school graduate	.710	.669	.041**
	Some college vs high school graduate	.735	.669	.066***
	BA vs high school graduate	.800	.669	.132***
	Advanced degree vs high school graduate	.812	.669	.143***

Table 4.8. (continued)

		To (New Value)	From (Original Value)	Change
Education	*Some college vs high school plus some training*	.735	.710	.025
	BA vs high school plus some training	.800	.710	.090***
	Advanced degree vs high school plus some training	.812	.710	.102***
	BA vs some college	.800	.735	.066***
	Advanced degree vs some college	.812	.735	.077***
	Advanced degree vs BA	.812	.800	.012
	+ 1 SD	.770	.700	.070***
Income	*+ 1 unit of income*	.722	.700	.022***
	+ 1 SD	.725	.700	.025***
Union Membership	*vs Not in the Union*	.715	.696	.019***
Employed	*vs Not Employed*	.703	.670	.034***
Political Party	*Independent vs Democrat*	.644	.736	-.091***
	Republican vs Democrat	.727	.736	-.008
	Republican vs Independent	.727	.644	.083***
Political Interest	*Somewhat Interested vs Not Interested*	.708	.506	.202***
	Very Interested vs Not Interested	.805	.506	.300***
	Very Interested vs Somewhat Interested	.805	.708	.097***
Religious Service Attendance	*+ one unit of religious service attendance*	.430	.700	.030***
	+ one SD	.747	.700	.047***
Religious Affiliation	*Moderate Protestant vs Liberal Protestant*	.699	.734	-.035**
	Sectarian Protestant vs Liberal Protestant	.673	.734	-.061***
	Catholics vs Liberal Protestant	.706	.734	-.028*
	Minority Religious Tradition vs Liberal Protestant	.742	.734	.008
	Unaffiliated vs Liberal Protestant	.702	.734	-.032*
	Sectarian Protestant vs Moderate Protestant	.673	.699	-.026**
	Catholics vs Moderate Protestant	.706	.699	.007

Table 4.8. (continued)

		To (New Value)	From (Original Value)	Change
	Minority Religious Tradition vs Moderate Protestant	.742	.699	.043*
	Unaffiliated vs Moderate Protestant	.702	.699	.003
	Catholics vs Sectarian Protestant	.706	.673	.033***
	Minority Religious Tradition vs Sectarian Protestant	.742	.673	.069***
	Unaffiliated vs Sectarian Protestant	.702	.673	.028**
	Minority Religious Tradition vs Catholics	.742	.706	.036*
	Unaffiliated vs Catholics	.702	.706	-.004
	Unaffiliated vs Minority Religious Traditions	.702	.742	-.041*
Biblical Literalism	*Bible is inspired by God vs Bible is the Word of God*	.710	.679	.031***
	Bible is a book of Fables vs Bible is the Word of God	.728	.679	.049***
	Bible is a book of Fables vs Bible is inspired by God	.728	.710	.017*
Year	*+ 1 year*	.698	.700	-.001**
	+ 1 SD	.687	.700	-.013**
Liberal Citizenry	*+ 1unit*	.700	.700	.001*
	+ 1 SD	.710	.700	.010*
Presidential Election Year	*vs A Non-presidential year*	.748	.585	.163***
Corruption Rate	*+1unit*	.654	.700	-.046**
	+ 1 SD	.690	.700	-.010**

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$
Note: Controlling for all variables included in Table 4.7a, Model 6

Moving to the bottom of Table 4.8, we see the AME predicted value for the effect of a one-unit increase in state corruption on the odds of self-reported voting. Sensibly, the starting point is .700, the predicted probability of voting with everything held at its mean. Looking at the effect of a one-unit increase in the corruption rate, we see that it decreases the predicted probability of voting by .046 points, leaving us with a predicted probability of .654. This seems

impressive, but a more realistic view of the impact would be to examine the change that occurs with one standard deviation change. Each standard deviation increase in the corruption rate reduces the predicted probability of voting by .01, which is a much smaller difference. Furthermore, comparing the statistically significant effect sizes for changing from one unit to the next for dummy categories as well as one standard deviation for continuous variables, we see that the change of one standard deviation for the corruption rate is the smallest change out of the statistically significant variables.

SECTION V: INTERACTION EFFECTS

Having established the statistically significant, but very small, effect of corruption on self-reported voting, it is now time to turn to the sub-hypotheses: Does living in states with higher rates of corruption change the effect of the social location variables of income, education, race or religious affiliation?

To answer these questions, I calculated cross-level interaction terms to test whether the effects of social demographic characteristics on voting depend on the state corruption rate (Jaccard 2001). Using the centered variables for corruption rate, and including the original variables in addition to product terms in all models, I assessed each of these potential impacts from hypotheses H1a – H1d.

To begin, I found no statistically significant cross-level interactions for state corruption and income nor state corruption and race. Therefore, I find no support for H1b:

> H1b: The level of state political corruption will influence the effect of income on self-reported voting in that the positive effect of income on voting will be increased in more corrupt states.

Nor did I find any support for hypothesis H1c:

> H1c. The level of state political corruption will influence the effect of race on self-reported voting in two ways; in more corrupt states, the positive effect of being white compared to any other racial group on voting will increase, and in more corrupt states, the positive effect of being black compared to being white on voting will decrease.

I did find an effect of corruption on education in that in more corrupt states the positive effect of having an advanced degree compared to having less than eight years of education, and the positive effect of having an advanced degree compared to having a high school degree are weaker than in less corrupt states. This provides some evidence in support of H1a: The level of state political corruption will influence the effect of education on self-reported voting in that the positive effect of education on voting will be reduced in more corrupt states. However, adding these product terms does not statistically significantly improve the model fit as evidenced by a nonsignificant chi square coefficient change from the previous model of 1.696. Jaccard (2001) cautions against placing emphasis on interaction effects combined with nonsignificant model improvements or presenting them, and I follow that call here.

Similarly, I found that in more corrupt states the negative effect of identifying as a moderate Protestant, sectarian Protestant, Catholic, or as religiously unaffiliated versus identifying as a liberal Protestant on voting was amplified. This provides support for H1d: The level of state political corruption will influence the effect of religious affiliation on self-reported voting in that in more corrupt states, the negative effect of being sectarian Protestant compared to liberal Protestant on self-reported voting will increase. However, the introduction of these product terms once again failed to improve model fit, and thus the results do not explain the effect of corruption on self-reported voting better than the results that are provided more parsimoniously above (Jaccard 2001).

SECTION VI: CONCLUDING REMARKS

Overall, these results suggest that a state's level of political corruption has a small, negative impact on self-reported voting for citizens in general. It is important to note, however, that many other factors – particularly individual-level factors – influence one's likelihood of self-reported voting more than political corruption. This, as well as the multilevel structure of the data testing for the relative variation in state influence on self-reported voting, suggests that individual-level factors are more influential than state-level factors in people's likelihood of voting. I show no support for the idea that corruption influences the effects of income and race on self-reported voting, and I only find minimal and nonsignificant support for the idea that corruption influences the effects of education and religious affiliation on voting. I discuss these findings in more detail in the final chapter of this dissertation.

CHAPTER 5

THE EFFECT OF STATE POLITICAL CORRUPTION PROSECUTIONS ON INDIVIDUAL TRUST IN GOVERNMENT

In this chapter, I present the results of the examination of political corruption and trust in government in five parts. First, I detail the descriptive statistics of the complete sample of 6,719 individuals, to ground the research. Next, I present the bivariate relationships between independent variables and the government trust index, also to establish baseline associations.

In the third section, I present the ML linear regression results of the effect of independent variables, specifically paying attention to the effects of corruption, on the government trust index. Surprisingly, I find no support for the hypothesis that citizens trust the government less in more corrupt states. In section four, I briefly mention the interaction effect tests I ran to test Hypotheses 2a through 2d. I also find no support no support that race, education, religious affiliation, and income depend on the state corruption rate in their effects on trust.

In part five, I test the effect of corruption on each of the four individual aspects that made up the government trust index using ML logistic regression and ML ordinal regression. To reiterate, these aspects include 1) a dichotomous variable asking individuals whether they believe the government is run for a few big interests or for the benefit of the people, and three multi-categorical variables measuring 2) how much of the time respondents think Washington does the right thing, 3) how much the government wastes on taxes, and 4) how many people in the government the respondent thinks are "crooked." As above, I find no significant relationships between corruption and any of these measures. I offer brief summarizing remarks in the final section of part six.

SECTION 1: DESCRIPTIVE STATISTICS

To begin, I present the descriptive statistics in Table 5.1 that situate this research.

As this table shows, the mean government trust score for this sample is about 32, which can be roughly interpreted as being less trustful of the government as a whole. The average corruption rate is .273, which roughly translates to one political corruption conviction per 400,000 citizens. Once again, the N of states in this sample is 41, with some of the less populated states omitted because of a lack of respondents for certain states, which helps to explain the relatively large difference in the mean corruption rate from the previous chapter to this one.

As is clear in this table, about 46% of this sample is female, and about 54% are married. The sample is made up of 12% blacks, 9% Hispanics, 3% another race, and the remaining 76% white. The mean age is about 47 years old. Approximately two-thirds of the sample own their own home. In terms of educational background, 13 % of the sample have less than a high school education, 31% of the sample have graduate high school, 28.5% of the sample have some college background, but no degree, and 28% of the sample have earned a Bachelor's degree or higher. Fifteen percent of the sample is identified as members of the lower class, 52% are coded as middle class, and 33% are coded as belonging to the upper class. The minority of the sample has a family member in the union (16%) and the majority of the sample is employed, retired, a student, or a homemaker (92%).

Politically, the sample is made up of 37% democrats, 37% independents and 27% Republicans. About 20% of the sample expressed no interest in politics, 47% expressed some interest in politics, and the rest stated that they were very interested in politics.

Table 5.1. Descriptive Statistics of NES Sample, 1992 – 2008, N = 6,719

	Mean	*SD*	Min.	Max.
Level-1 Variables				
Government Trust	32.270	22.193	0	100
Female	0.462	0.498	0	1
Married	0.538	0.498	0	1
Race				
White	0.761	0.426	0	1
Black	0.119	0.324	0	1
Hispanic	0.088	0.284	0	1
Other	0.031	0.173	0	1
Age	46.566	17.139	17	94
Own Home	0.665	0.472	0	1
Education				
Less than High School	0.129	0.335	0	1
High School Graduate	0.306	0.461	0	1
Some College	0.285	0.451	0	1
Bachelor's Degree or Higher	0.280	0.449	0	1
Class				
Lower	0.152	0.359	0	1
Middle	0.523	0.500	0	1
Upper	0.325	0.468	0	1
Union Membership	0.164	0.370	0	1
Employed	0.916	0.277	0	1
Political Party				
Democrat	0.369	0.483	0	1
Independent	0.366	0.482	0	1
Republican	0.265	0.441	0	1
Political Interest				
Not at all Interested	0.198	0.398	0	1
Somewhat Interested	0.465	0.499	0	1
Very Interested	0.338	0.473	0	1
Religious Service Attendance				
Never	0.322	0.467	0	1
Sometimes	0.419	0.493	0	1
Weekly	0.259	0.438	0	1
Religious Affiliation				
Liberal Protestant	0.074	0.262	0	1
Moderate Protestant	0.329	0.470	0	1
Sectarian Protestant	0.175	0.380	0	1
Catholics	0.253	0.435	0	1
Minority Religious Traditions	0.029	0.168	0	1
Unaffiliated	0.139	0.346	0	1

Table 5.1. (continued)

		Mean	*SD*	Min.	Max.
Biblical Literalism					
"The Bible is the Word of God"		0.364	0.481	0	1
"The Bible is inspired by God"		0.492	0.500	0	1
"The Bible is a book of fables"		0.144	0.351	0	1
Social Trust Index		1.218	1.028	0	3
Political Allegiance					
Governor and President are of Respondent's Affiliation		0.124	0.329	0	1
Governor or President are of the Respondent's Affiliation		0.385	0.487	0	1
Governor and President are not of Respondent's Affiliation		0.491	0.500	0	1
Better Off	*1: Worse Now 2: Same as Last Year 3: Better Off than Last Year*	2.124	0.808	1	3
Approve of President		0.570	0.495	0	1
<u>Level-2 Variables</u>					
Modern Urbanity Index		.002	.286	-1.728	1.538
Diversity Index		34.186	12.501	4.712	61.115
Percent Urban		77.258	11.441	38.100	94.880
Income Inequality (GINI)		58.684	3.307	52.631	69.629
Population (in 100,000s)		110.285	89.634	4.635	366.043
Percent College Educated		15.810	3.207	7.927	25.648
Average Household Income (in 10,000s)		5.542	0.759	3.353	7.497
Liberal Government Score		49.738	20.641	6.514	92.214
Liberal Citizenry Score		50.572	12.082	9.251	90.957
Corruption Rate		0.273	0.193	0	2.585

In terms of religiosity, 32% of the sample report never having attended church, 42% report attending sometimes (ranging from once a year to a few times a month), and about a quarter of the sample report attending weekly. The majority of the sample is coded as moderate

Protestant (32.9%), 25.3% are Catholics, 17.5% are coded as sectarian Protestants, 13.9% express no religious affiliation, 7.4% are liberal Protestants, and the remaining 3% are made up of members of minority religious traditions. Fourteen percent of the sample believe that the Bible is a book of fables, with 36% stating that the Bible should be considered the literal word of God, and 49% saying that the Bible is written by man, but still essentially inspired by God.

Moving on to the individual-level variables included in this analysis but not in Chapter 4, we see that the average respondent scores a 1.2 (on a scale of 3) on the social trust index, indicating a slightly less trustful view of others than trustful. Next, about 12% of the sample state that they are members of the same political party as both the governor and the president in the year that they were surveyed, 39% reporting the same political affiliation as at least one of these individuals, and about half reporting that they did not share the same political affiliation with either elected official. Of course, it should be noted that this sample does have more political independents than might be expected in an essentially two-party system of governance in the U.S., and that might help explain why so few report having the same political allegiance as both elected officials in an essentially two-party system.

Finally, the mean score on the measure of whether an individual considers him or herself to be better off is slightly over two. This indicates that the average respondent thinks that they are the same in terms of well-being compared to the previous year. Further, 57% approve of the president in general terms. Whether these variables explain each other away remains to be seen in multivariate analyses.

Turning now to level-2 characteristics, we begin with the measures that make up the modern urbanity index, which has a mean score of .002 (*SD* =.286). The average citizen lives in a state that scores about a 34 on the diversity index, and that is populated by 77% of the people

living in urban areas. The average citizen lives in a state that has a Gini coefficient of just below sixty and that has a population of about 11,000,000. Finally, the average citizen lives in a state where about sixteen percent of the population has a college education. In other words, the average citizen lives in a state that is less diverse than more so, where about three-quarters of the population live in urban areas, that has a population of about 11 million, which has more income inequality than income equality, and where just over sixteen percent of folks have bachelor's degrees or higher.

My other macro-level variables are household income and the two ideology scores. In terms of household income, the average citizen lives in a state where the average is right around fifty-five thousand dollars. Also, the liberal government and liberal citizenry scores are very close for citizen's states in these samples, with only a one-point difference between them, signifying the average citizen lives in a state where citizens are slightly more liberal (score of 51) than their governments (score of 50).

SECTION II: BIVARIATE ANALYSES

In this section, I present level-1 dichotomous relationships, level-1 categorical relationships, and then level-1 and level-2 continuous relationships.

Table 5.2 presents level-1 relationships between dichotomous variables and government trust. T-scores in this table show each group's mean score on government trust, and whether the scores significantly differ.

Table 5.2. Bivariate Analyses of Level-1 Dichotomous Variables on Political Trust, N = 6,719

	n	Mean	S.E.	*SD*	[95% C. I.]		Mean Difference	t-score
Males	3,107	32.209	.396	22.073	31.433	32.986	-.114	-.202
Females	3,612	32.323	.371	22.298	31.595	33.050		
Not Married	3,096	31.925	.400	22.254	31.141	32.710	-.640	-1.178
Married	3,623	32.565	.368	22.139	31.844	33.286		
Not Homeowner	2,246	32.858	.474	22.483	31.928	33.789	.883	1.540
Homeowner	4,473	31.975	.330	22.042	31.329	32.621		
Not in a Union	5,617	32.499	.297	22.273	31.916	33.082	1.395	1.908*
In a Union	1,102	31.104	.655	21.748	29.819	32.390		
Not Employed	562	31.610	1.004	23.496	29.639	33.582	-.720	-.736
Employed	6,157	32.331	.281	22.042	31.780	32.881		
Disapprove of the President	2,890	27.029	.378	20.306	26.289	27.770	-9.196	-17.182***
Approve of the President	3,829	36.226	.367	22.732	35.506	36.946		

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$

In Table 5.2, most of the groups do not significantly differ in terms of their mean trust scores. Men are not statistically significantly likely to trust less than women, married folks are no more trusting than those who are single, and employment and homeownership also has no effect.

Only two of these relationships reach significance. First, those with a family member not in a union score higher on the trust index than those with a family member in a union ($p < .05$). Next, those who approve of the president score about nine points higher on the government trust index than those who do not approve of the president ($p < .001$). Not controlling for other variables, this suggest that citizens base their trust on a somewhat rational assessment: if the president is doing something of which they approve, there will trust it more.

I next present group means and one-way analysis of variance (ANOVA) tests for the multi-categorical variables. One-way ANOVAs test for differences in the means of continuous dependent variables (in this case government trust) broken down by the levels or categories of the independent variables. One-way ANOVAs do not by themselves, however, tell us which groups significantly differ from each other: just that there is significant variation between the group means. Thus it is appropriate to run post-hoc tests, and for most of the variables I use Scheffe tests. However, it should be noted that Levene's tests for five of these variables (race, class, political interest, biblical literalism, and whether respondents felt they were better off than the previous year) failed to reject the null hypothesis that the variances in each category were equal, which is one of the central assumptions of ANOVA analysis. Therefore, I use a Tamahane post-hoc analysis instead of Scheffe, and present those results as well as the Welch F-statistic in table 5.3 for those variables.

All of the F-statistics, which show whether mean government trust scores differ significantly by category, are significant at the $p < .001$ level, with the exceptions of church attendance and biblical literalism, which are significant at $p < .01$, and political interest, which is significant at the $p < .05$ level.

Moving on to substantive interpretations, we see that Hispanics score about six points higher than both whites and blacks on trust, both of which are statistically significant at the $p <.001$ level. Those with bachelor's or advanced degrees score significantly higher than both high school graduates and those with some college experience (both significant at $p <. 001$), but not those with less than a high school education. Trust scores also significantly differ by class, with those in the middle class trusting least, scoring an average of thirty-one points, and those in

the upper and lower class respectively averaging a score nearly two ($p < .05$) and three ($p < .01$) points higher.

Next, political independents have the lowest mean of government trust, whereas Democrats and Republicans report similar levels of trust at three points higher ($p < .001$). There is also significant variation in trust scores based on political interest, with those with not much political interest averaging around a 31 and those with some interest averaging a 33, which is significant at $p < .05$.

In terms of religious variables, those who attend church sometimes have a mean score of 33.3 on the trust index, which is significantly higher than those who never attend church, with their mean score of 31.3 ($p < .01$). Catholics, on average, score highest with a mean score of about 35, which is statistically significantly higher than moderate Protestant, sectarian Protestants, and the religiously unaffiliated (all at $p < .001$). Liberal Protestants, with their score of 34.6, are also statistically significantly higher than the mean score of 29.7 held by sectarian Protestants ($p < .01$). Finally, those who believe the bible is inspired by God trust more than those that believe the Bible is a book of fables ($p < .01$).

Moving on, those with political ties to neither their current president nor state governor score an average of thirty on the government trust index, whereas those with some sort of affiliation score an average of 34.5, both of which are statistically significantly different from those without ties at $p < .001$. Finally, we see that those who believe that they are better off this year than the year previous have significantly higher political trust scores (at about 35.3) than those who think they're worse off (27.5), as well as those who think they are doing relatively the same as the previous year (32.5). For this variable, all of the categories' mean scores are significantly different at $p < .001$.

Table 5.3. One-way ANOVAs of Multi-Categorical Variables on Political Trust, N = 6,719

	Mean	*SD*	Source	SS	Df	MF	F
White	31.79[a]	21.75	B. Gps.	15,872.6	3	5,290.8	8.98***
Black	31.32[b]	22.49	W. Gps.	3,292,812.5	6,715	490.3	
Hispanic	37.02[ab]	24.62	Total	3,308,685.2	6,718	492.5	
Other Race	33.95	22.71					
Less than High School	32.98	23.38	B. Gps.	15,094.9	3	5,031.6	10.26***
High School Graduate	31.39[c]	22.34	W. Gps.	3,293,590.2	6,715	492.5	
Some College	30.76[d]	21.80	Total	3,308,685.2	6,718	492.5	
Bachelor's or Higher	34.42[cd]	21.68					
Lower Class	33.84[e]	23.74	B. Gps.	7,274.8	2	3,637.4	7.40***
Middle Class	31.30[ef]	21.85	W. Gps.	3,301,410.3	6,716	491.5	
Upper Class	33.08[f]	21.91	Total	3,308,685.2	6,718	492.5	
Democrat	33.18[g]	22.26	B. Gps.	16,364.6	2	8,182.2	16.69***
Independent	30.24[gh]	22.14	W. Gps.	3,292320.6	6,716	490.2	
Republican	33.80[h]	27.97	Total	3,308,685.2	6,718	492.5	
Little Political Interest	31.20[i]	23.14	B. Gps.	3,536.7	2	1,768.3	3.49*
Some Interest	33.00[i]	22.04	W. Gps.	3,305,148.4	6,716	492.1	
Very Interested	31.88	21.79	Total	3,308,685.2	6,718	492.5	
Never Attend Church	31.29[j]	22.41	B. Gps.	5,418.4	2	2,709.1	5.51**
Attend Sometimes	33.30[j]	21.98	W. Gps.	3,303,266.8	6,716	491.8	
Attend Weekly	31.81	22.18	Total	3,308,685.2	6,718	492.5	
Lib. Prot.	34.60[k]	22.12	B. Gps.	28,360.5	5	5,672.1	11.61***
Mod. Prot.	31.42[l]	21.79	W. Gps.	3,280,324.6	6,713	488.6	
Sect. Prot.	29.71[kl]	21.67	Total	3,308,685.2	6,718	492.5	
Catholics	35.04[l]	22.66					
Min. Rel. Trad.	34.61	22.34					
Unaffiliated	30.73[l]	22.26					

Table 5.3. (continued)

	Mean	*SD*	Source	SS	Df	MF	F
"The Bible is the Word of God"	32.02	22.72	B. Gps.	6,204.1	2	3,102.0	6.73***
"Inspired by God"	33.05[m]	22.06	W. Gps.	3,302,481.1	6,716	491.7	
"Book of Fables"	30.22[m]	21.11	Total	3,308,685.2	6,718	492.5	
Pres. and Gov. are in Respondent's Party	34.52[n]	22.08	B. Gps.	35,067.2	2	17,533.6	35.97***
One is in Party	34.51[n]	22.23	W. Gps.	3,273,617.9	6,716	487.4	
Neither is in Party	29.94[n]	21.95	Total	3,308,685.2	6,718	492.5	
Worse Off than Last Year	27.47[opq]	21.12	B. Gps.	67,023.7	2	33,601.9	71.14***
Same As	32.55[opq]	21.67	W. Gps.	3,241,481.4	6,716	482.7	
Better Off	35.33[opq]	22.76	Total	3,308,685.2	6,718	492.5	

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$
Note: Any categories with the same superscript letter are significantly different from each other.

The final bivariate relationships are the correlations between first- and level-2 continuous or interval variables. For the sake of space, I present both level-1 continuous variables (age and social trust) and level-2 variables in the same table, instead of separating them into level-1 and level-2 relationships. These correlations are presented in Table 5.4.

While this matrix details many relationships, most importantly there is no association between corruption and government trust (.017, $p < .155$), which is contrary to much literature on the subject (e.g., Rothstein 2013; Uslaner 2008; Vogl 2012). Of course, bivariate correlations merely show relationships without controlling for other factors, which, when controlled for, might allow the effect of a state's corruption level to play out in the expected way.

Table 5.4. Correlation Matrix of Level-1 and Level-2 Continuous Variables, N = 6,719

	1	2	3	4	5	6	7	8
1. Government Trust	1.000	--	--	--	--	--	--	--
2. Corruption Rate	.017	1.000	--	--	--	--	--	--
3. Age	-.020	.023	1.000	--	--	--	--	--
4. Social Trust Index	.212***	-.039***	.137***	1.000	--	--	--	--
5. Modern Urbanity Index	.027*	.252***	-.002	.024**	1.000	--	--	--
6. Household Income	.083***	-.011	-.007	.192***	.348***	1.000	--	--
7. Liberal Government	-.033**	-.129***	-.005	-.017	.010	-.097***	1.000	--
8. Liberal Citizenry	-.030*	.070***	.022	-.006	.297***	.302***	.322***	1.000

In Table 5.4 we also see that liberal citizenries are positively correlated with corruption rates ($r = .070$, $p < .001$), although a liberal government is negatively correlated with corruption rates ($r = -.129$, $p < .001$). However, both of these correlations are rather weak.

Social trust, as expected, is moderately correlated with government trust at .213 ($p < .001$). Social trust is also correlated with other interesting variables. First, there is a small significant positive association of a state's overall level of modern urbanity with social trust ($r = .024$, $p < .01$). Social trust is also negatively correlated with the corruption rate ($r = -.039$, $p < .001$), but positively correlated with an individual's age ($r = .139$, $p < .001$), indicating that older people are more trusting of others, not controlling for other factors.

The modern urbanity index score is weakly positively correlated with trust in government ($b = .027$, $p < .05$). Also, the overall average household income of a state is weakly positively correlated with a citizen's government trust score as well ($b = .083$, $p < .001$). Furthermore, we see weak negative relationships between liberal government and trust in government (-.033, $p < .01$) and liberal citizenries and trust in government (-.030, $p < .05$). What this suggests is that, not controlling for any other factors, citizens score higher on trust in government when they live in more conservative states both governmentally and in terms of the population, though the relationship is a weak one.

In sum, while it is somewhat surprising that there is no relationship between corruption and government trust in the bivariate characteristics, the other measures seem to fall in line with their expected relationships.

SECTION III: MULTILEVEL LINEAR REGRESSION

With the bivariate relationships established, we can now examine the effect of independent variables on an individual's trust in the government, controlling for other factors. As in Chapter 4, I have separated these results in two Tables: 5.5 and 5.5a.

First, I present the unconditional means model predicting government trust in Table 5.5. This, sometimes referred to as the "null" model (Hayes 2006), serves as a baseline to understand how later models affect the explained variation in government trust. Model 2 gives us a sense of the impact of corruption on political trust scores without controlling for other variables.

Model 1 shows that the mean of the government trust score for this sample is 32.108, which means that citizens in each state, on average, held a score of about 32 on the trust index, which is sensible because it also was the average reported in descriptive statistics.

Comparability diagnostics for ML linear regression are similar to those for ML logistic regression. First, the log likelihood provides a baseline from which to compare model fit as well: if the chi square difference from the previous model reaches a certain level we can say the model is a better fit. The baseline score of -30,344.652 in Model 1 is what future results will be compared against.

Table 5.5. Multilevel Regression Empty Model and with Corruption Rate, N = 6,719

	(1) b (S.E.)	(2) b (S.E.)
Constant	32.108***	32.131***
	(.477)	(.491)
Corruption Rate	--	3.881*
		(1.679)
Reliability Diagnostics		
Log Likelihood	-30,344.652	-30,342.009
df difference		1
χ^2 score		2.643
State *SD*	2.207	2.312
	(.426)	(.439)
Individual *SD*	22.081	22.069
	(.191)	(.191)
State ICC	.010	.011
	(.004)	(.004)
Snijders/Bosker R^2 Level-1	.000	.001
Snijders/Bosker R^2 Level-2	.000	-.098

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$

Next, we see standard deviations of 1) the individual's scores from the grand mean and 2) the states from the grand mean. In this model, the average score of an individual on voter trust is 32.108. Individuals deviate from that grand mean with a standard deviation of 22.08. States' residuals deviate from that score with a standard deviation of 2.207. In other words, there is more variation in government trust scores within a particular state than between states, indicating individual-level factors as important to understand in terms of determinants of trust. In fact, the ICC here is .010, which suggests that 1% of the variability in government trust left unexplained is due to differences at the state level. This number is very small, so we should expect to see that the multi-level models to follow may not differ much from a standard OLS regression model.

Lastly, the ML Snijders/Bosker R^2 shows that none of the variation is explained in Model 1, which makes sense because there are no added explanatory variables. The best way to define

R^2 for ML models is the proportion of the variance explained in comparison to a baseline model (Snijders and Bosker 1999). This is in contrast to standard linear regression, where R^2 is commonly reported as the explained variation divided by the total variation (Recchia 2009).

Model 2 introduces the corruption rate into the baseline model, and gives us a sense of the influence of corruption, not controlling for other factors. The addition of the corruption rate does some interesting things. First, a one-unit increase in the corruption rate leads to an increase of 3.88 on a citizen's trust index score ($p < .05$). This is a strange finding, especially because there is nothing in the literature to suggest that *increasing* corruption will have a positive effect on citizen trust.

Furthermore, we see that both individual-level and state-level standard deviations are reduced, which makes sense: including a variable would potentially explain more of the variability of a given model. When we examine the comparability diagnostics, however, we see that the measure of corruption is not a very robust influencer of one's trust score. Specifically, the log likelihood does not change in a statistically significant way, which means that the inclusion of this variable does not add to our understanding of government trust.

Furthermore, the level-1 Snijders/Bosker R^2 is very low. The introduction of the corruption rate explains about .1 percent of the variation at the individual level. For the level-2, the introduction of the actually adds to the variance of the previous model (Snijders and Bosker 1999), which results in the negative coefficient. In other words, adding the corruption rate to the model increases the variance of the state's residual around the grand mean instead of reduces it (Snijders and Bosker 2012). This furthers the idea that the state corruption rate is not a powerful influence of overall government trust.

Next, we can turn to the models which test the effect of corruption on trust controlling for other variables in Table 5.5a.

Model 3 introduces the level-1 variables to the model. Notably, we see support for the idea that individuals trust the government because of some general social trust, with an increase of about five points on the government trust index for every one unit increase in social trust ($p <$ .001), controlling for other factors. However, we also see support for the idea that citizens trust the government based on, as described in the literature, institutional or structural measures that speak to an individual's interpretation of how well the government operates. People score higher on the trust index by about four and a half points if they think they're doing better the year of the interview than they were the year before ($p <$ .001), ceteris paribus. They're also more likely to trust the government by a little over two and a half points if they think that they're about the same off as compared to last year ($p <$ .001), net all. Thus, perhaps it is not whether the government is benefitting people's livelihoods, but whether people perceive the government to not be actively harming their lives. Finally, those who report approving of their current presidential administration versus not approving it score about seven and a half points higher on the government trust index ($p <$ .001), net all.

On the other hand, we see that there is no significant difference in government trust scores of people with political ties to government officials, compared to people with no such ties. Thus, people do not trust more or less based on whether their party is in power, but based on their previous levels of generalized social trust and their assessment of the efficacy of the presidential administration, as well as several other demographic factors.

Table 5.5a. Multilevel Linear Regression Coefficients Predicting Political Trust, N = 6,719

	(3) b (S.E.)	(4) b (S.E.)	(5) b (S.E.)	(6) b (S.E.)
Constant	26.536***	26.574***	26.353***	26.395***
	(1.911)	(1.833)	(1.830)	(1.832)
Female	.094	.085	.077	.083
	(.526)	(.525)	(.525)	(.525)
Married	.487	.566	.596	.597
	(.582)	(.581)	(.581)	(.581)
Race[a]				
Black	2.363**	1.933*	1.963*	1.939 *
	(.911)	(.912)	(.921)	(.911)
Hispanic	5.859***	5.309***	5.418***	5.419***
	(.995)	(.995)	(1.003)	(.992)
Other	2.753	2.240	2.240	2.250
	(1.429)	(1.492)	(1.493)	(1.493)
Age	-.054**	-.061***	-.062***	-.062***
	(.018)	(.018)	(.018)	(.018)
Own Home	-1.733**	-1.736**	-1.653**	-1.656**
	(.630)	(.629)	(.629)	(.629)
Education[b]				
High School	-2.062*	-2.425**	-2.412**	-2.408**
Graduate	(.890)	(.890)	(.889)	(.890)
Some College, no	-3.578***	-4.130***	-4.168***	-4.161***
Degree	(.945)	(.949)	(.949)	(.949)
BA or Advanced	-1.628	-2.185*	-2.204*	-2.195*
	(1.022)	(1.025)	(1.025)	(1.025)
Class[c]				
Lower Class	3.367***	3.307***	3.421***	3.418***
	(.808)	(.807)	(.808)	(.808)
Upper Class	.080	.313	.275	.270
	(.646)	(.646)	(.647)	(.647)
Union Membership	-.705	-.719	-.814	-.803
	(.716)	(.714)	(.715)	(.715)
Employed	-1.112	-1.033	-1.026	-1.023
	(.968)	(.967)	(.966)	(.966)
Political Party[d]				
Independent	-1.308	-1.248	-1.389	-1.406
	(.883)	(.882)	(.882)	(.883)
Republican	.877	.745	.733	.724
	(.706)	(.706)	(.705)	(.706)

Table 5.5a. (continued)

	(3) b (S.E.)	(4) b (S.E.)	(5) b (S.E.)	(6) b (S.E.)
Political Interest[e]				
Somewhat	.744	.750	.790	.788
Interested	(.699)	(.698)	(.699)	(.699)
Very Interested	.322	.321	.393	.389
	(.768)	(.766)	(.769)	(.769)
Rel. Attendance[f]				
Never	-.788	-.741	-.779	-.782
	(.720)	(.719)	(.719)	(.718)
Weekly	-1.424*	-1.281	-1.249	-1.249
	(.669)	(.668)	(.669)	(.669)
Rel. Affiliation[g]				
Liberal Protestant	4.103***	4.411***	4.431***	4.404***
	(1.152)	(1.151)	(1.150)	(1.152)
Moderate Protestant	1.797*	1.947*	2.085**	2.067**
	(.773)	(.771)	(.773)	(.773)
Catholics	4.201***	4.244***	4.179***	4.160***
	(.832)	(.830)	(.831)	(.832)
Min. Rel. Traditions	5.045**	5.195**	5.004**	5.971**
	(1.695)	(1.691)	(1.691)	(1.693)
Unaffiliated	2.560*	2.511*	2.535*	2.531*
	(1.197)	(1.093)	(1.092)	(1.092)
Biblical Literalism[h]				
God inspired Bible	-.996	-.968	-1.050	-1.045
	(.626)	(.624)	(.625)	(.625)
Bible is Fables	-3.737***	-3.784***	-3.939***	-3.931***
	(.952)	(.950)	(.953)	(.953)
Social Trust Index	4.855***	4.732***	4.667***	4.669***
	(.273)	(.274)	(.278)	(.278)
Political Allegiance[i]				
One Affiliate	1.416	1.674	1.715	1.691
	(.880)	(.878)	(.879)	(.888)
No Affiliates	-.074	-.040	.117	.116
	(1.069)	(1.067)	(1.071)	(1.070)
Better Off[j]				
Same	2.618***	2.811***	2.786***	2.778***
	(.681)	(.681)	(.684)	(.684)
Better Off	4.436***	4.426***	4.424***	4.411***
	(.671)	(.670)	(.671)	(.671)
Approve	7.513***	7.560***	7.553***	7.552***
	(.564)	(.563)	(.568)	(.568)
Year	--	.298***	.281***	.275***
		(.056)	(.062)	(.063)

Table 5.5a. (continued)

	(3) b (S.E.)	(4) b (S.E.)	(5) b (S.E.)	(6) b (S.E.)
Modern Urbanity Index	--	--	-.735 (1.169)	-.668 (1.178)
Average Household Income (in 10,000's)	--	--	1.129* (.456)	1.135* (.459)
Liberal Government	--	--	.013 (.015)	.014 (.015)
Liberal Citizenry	--	--	.003 (.029)	.002 (.030)
Corruption Rate	--	--	--	.687 (1.510)
Reliability Diagnostics				
Log Likelihood	-29,928.593	-29,914.434	-29,911.063	-29,910.962
df difference	32	1	4	1
χ^2 score	416.059***	14.159***	3.371	.101
State *SD*	1.544 (.389)	1.411 (.379)	1.326 (.413)	1.161 (.411)
Individual *SD*	20.773 (.180)	20.733 (.179)	20.732 (.179)	20.730 (.179)
Year *SD*		.000 (.000)	.000 (.000)	.000 (.000)
State ICC	.005	.005	.003	.003
Snijders/Bosker Level-1 R^2	.119	.123	.125	.125
Snijders/Bosker Level-2 R^2	.261	.293	.347	.342

Two Tailed: * $p < .05$, ** $p < .01$, *** $p < .001$
Reference Categories: [a] White, [b] Not a High School Graduate, [c] Middle Class, [d] Democrat, [e] Not interested in politics, [f] Attends Sometimes, [g] Sectarian Protestant, [h] Bible is the Word of God, [i] Both President and Governor are of Respondent's Political Party, [j] Worse off

Demographically, we see that there is no significant effect of sex, marriage, union membership, employment, political party, nor political interest on trust in the government, controlling for other factors. However, some demographic level-1 characteristics do seem to influence trust scores. For instance, for every one year of age, citizens report trusting the government less by about half a point, controlling for other factors. Homeowners compared to

non-homeowners trust the government an average of one and three-quarters points less, net all. These findings are interesting in that they suggest some of the very factors that influence one's level of social stability in society (such as being an older citizen or owning one's home) reduce one's level of political trust. Recalling that age and homeownership both increased an individual's odds of voting, it is indeed curious as to why factors that increase the odds of voting also decrease the level of trust in the government.

In terms of racial categories, Hispanics and blacks are more likely to report trusting the government than whites, controlling for other factors. This is somewhat surprising, considering the literature suggests these groups are more likely to have less trust for the government. While this could be a ramification of this sample (i.e., these are respondents who elected to take a survey on the government, and thus may not be representative of all citizens – especially those less trusting of any official sort of survey), this might also suggest that minorities who achieve equitable levels of success in society as whites trust the government even more than whites. On the surface, this idea makes sense: because it is so hard for the average black or Hispanic citizen to achieve the same levels of success as the average white citizen in our modern society (Omi and Winant 1994), those who have achieved such success may have found resources within the government to help them toward that goal, and thus have a more appreciative stance towards the government. Of course, the flip side of this argument is that whites are less trusting than these two groups: this might be a ramification of white citizens interpreting the government as not working in their best interest.

We also see some interesting relationships between education and class categories with reference to political trust. Controlling for other factors, high school graduates ($p < .05$), and those with some college but no degree score about have lower trust than high school dropouts (p

< .001). In other words, it seems that having at least some education reduces trust, compared to not achieving even a high school degree, at least when only level-1 demographic characteristics are taken into account.

Similarly, we see that individuals categorized as living in the lower class (in the bottom sixteen percent of the income distribution as reported by NES) score about three points higher on the government trust index than those who are middle class ($p < .001$), controlling for other factors. Middle class individuals are statistically indistinguishable from those who are categorized as upper class. As with racial categories, this could reflect that those with a lower social status see the government as a provider of resources, or it might reflect a lack of trust on behalf of those with more resources.

Lastly, we see that each of the religious characteristics influence government trust. First, those who attend religious services weekly compared to those who only show up to church sometimes score about 1.4 points lower on the government trust index ($p < .05$), controlling for other level-1 individual factors. We also see that all religious affiliation groups score higher on the government trust index statistically significantly, compared to sectarian Protestants and controlling for other factors. The biggest increase is five points for members of minority religious traditions ($p < .01$), and the smallest is the increase for moderate Protestants, at about two ($p < .05$). Finally, we see that people who believe the Bible is a book of Fables are also less trusting of the government, by about four points, than people who believe the Bible is the word of God, controlling for other factors.

For comparability purposes, the chi square score indicates that the introduction of these variables significantly improves the fit of the model. The Snijders/Bosker R^2 for the first level indicates that these demographic variables explain about 11.9% of the variance left unexplained

in the null or empty model for individuals. The Snijders/Bosker R^2 for level two indicates that about a quarter of the variance in level-2, or aggregate government trust index scores, is explained compared to the null model. Notably, we are primarily concerned with how much the characteristics explain the level-1 variation, although ML Pseudo-R^2s are only somewhat useful for meaningful interpretation (Long and Freese 2014).

Model 4 introduces the variable of time, which has a small positive effect on government trust scores, controlling for other factors ($b = .298$, $p < .001$). This means that for each year the government trust score rises by three-tenths of a point, if one were to take into account all else.

Importantly, time is allowed to affect government trust differently based on state, because I allow time to have random slopes (Huber 2013). What that means is that the effect of time in Minnesota, say, could be different than the effect of time in Washington, and we'd be able to assess how much it varies by looking at the standard deviation of time from its mean score of .298 in much the same way we can see the standard deviation of the constant and the state's residual. However, because the standard deviation is the equivalent of 0 ($SD = .0000013$), we can interpret that as saying that the effect of time is the same for each state. The small positive effect herein is reflective of a general positive trend, and not explained by a particular state influencing individuals to be more trusting, somehow, later in the sample, for instance.

The only variables that changed in significance with the introduction of time had to do with education and religiosity. First, people with bachelor's degrees or higher score statistically significantly lower on their government trust index than those without a high school education ($b = -2.185$, $p < .05$), when controlling for the positive effect of time as well as other factors. Next, the significant negative effect of attending church weekly compared to sometimes on government trust scores in Model 3 failed to achieve significance.

Finally, we see that the addition of the time variable increases the fit of the model as shown by the statistically significant chi square test statistic. Both Snijders/Bosker R^2s are increased as well with the addition of time, signifying that time helps explain more of the variation from the null model at both level-1 and level-2.

Model 5 introduces the four level-2 characteristics: the modern urbanity index, average household income, liberal government, and liberal citizenry. Of the four, the only level-2 characteristic that reached statistical significance was average household income. Specifically, for every ten thousand dollars increase in household income for a state, citizens who live in that state score an average of 1.2 points higher on the government trust index, net all.

On the other hand, neither a liberal nor conservative citizenry nor a liberal or conservative government influence citizens' trust scores, controlling for other factors. A state's modern urbanity score is also not statistically significant, suggesting that the underlying factors in that index might not have much to do with a citizen's overall level of trust in the government. The only change from this model for earlier variables was for moderate Protestants, scoring two points higher than sectarian Protestants.

Finally, we can also see that adding these four state-level variables did very little if anything at all to improve the model over including only individual-level characteristics. The chi square score does not reach statistical significance, although Model 5 does a little to improve the level-1 R^2. This suggests that government trust is a personal decision, not influenced by state-level characteristics.

The final model, Model 6, is the full model. Here we see that the positive relationship between the corruption rate and citizen's government trust is still positive (as it was in the bivariate relationship), but the size is much smaller and the effect is not statistically significant.

In fact, the introduction of the corruption rate failed to improve on the chi square score from the previous model, and added nothing to the level-1 R^2.

Therefore, I find no support for H2: Individuals who live in states with higher rates of corruption convictions will trust the government less than citizens living in less corrupt states. In fact, there is no relationship between the corruption rate and individual trust in the government. As shown by the relative lack of explanatory power of state-level characteristics compared to individual-level characteristics, it seems that citizen government trust is only loosely related to the general features of a state, and much more closely related to individual characteristics and perceptions of government efficacy.

SECTION IV: INTERACTION EFECTS

Unsurprisingly, given the nonsignificant relationship between political trust and the corruption rate, no interaction effects emerged to suggest that the level of political corruption in a state influenced the effect of key demographic variables on political trust.

Explicitly, I fail to find support for H2a (The level of state political corruption will influence the effect of education on political trust in that the positive effect of education will be reduced in more corrupt states); H2b (The level of state political corruption will influence the effect of income on political trust in that the negative effect of having low income will increase in more corrupt states); H2c (The level of state political corruption will influence the effect of race on political trust in that the negative effect of being black compared to being white will increase in more corrupt states); and H2d (The level of state political corruption will influence the effect of religious affiliation on political trust in that in more corrupt states, the negative effect of being sectarian Protestant compared to liberal Protestant on political trust will increase). The lack of

significant product terms and the failure to improve model fit indicate that the effects of these particular social markers will influence trust the same in the most corrupt and least corrupt states.

SECTION V: ALTERNATIVE SPECIFICATIONS

Finally, because the NES trust index had a borderline acceptable Cronbach's alpha of .680, it is worth considering the different elements of trust separately because it is certain possible that corruption could affect one element of the index, but not the others. Thus, it is necessary to test whether the null findings presented above are due to the lack of internal consistency in NES' measure of government trust.

Therefore, I tested each of the four variables in the index independently, as dependent variables, keeping all of the other variables in the models. In other words, instead of testing the effect of the corruption rate on an individual's level of trust in the government using the government trust index, I test the effect of the corruption rate on an individual's answer to each of the four questions that were used to construct the index.

These specific questions ask how often respondents trust the government in Washington to do what is right, analyzed with ML ordinal logistic regression; whether respondents believe the government is run by a few big interests, or for the benefit of all, analyzed with ML logistic regression; how much respondents think the government wastes tax dollars, analyzed with ML ordinal logistic regression; and how many government employees respondents think are "crooked," analyzed with ML ordinal logistic regression. For each response, a higher value indicates a more trusting answer, and so an OR over 1 would indicate a positive effect on trust, and an OR under 1 would indicate a negative effect on trust (Long 1997).

Table 5.6 presents the results of this analysis. While I only present the coefficients between the corruption rate and each particular dependent variable, all of these models control for factors previously controlled for in Table 5.5a, Model 6. As this table makes clear, the effect of corruption on each operationalization of trust fails to reach statistical significance. Thus, this lack of support for Hypothesis 2 is robust to other specifications of political trust as well.

CONCLUDING REMARKS

Overall, this research suggests that a state's level of political corruption has no impact on political trust for citizens in general. Instead, citizens trust the government based on their preconceived notions as well as their concrete assessments of the government and their place in the world. The fact that corruption does not factor into that assessment of the government, as well as the fact that adding level-2 characteristics to the model with no increase in model fit, suggests that individual-level factors are more influential than state-level factors in people's political trust.

This chapter also indicates that the state corruption context did not change the effect of socio-demographic variables on trust. The effect of state-level political corruption had no influence on the effect of income, education, race, or religious affiliation. I discuss these findings in more detail in the next, final chapter of this dissertation.

Table 5.6. The Effect of Corruption Rate on Alternative Specifications of Trust in ML Regressions

Question	Response Choices	Method	O.R.	95% C.I. LL	95% C.I. UL	N
How much of the time do you think you can trust the government in Washington to do what is right?	1. Never 2. Some of the time 3. Most of the time 4. Just about always	ML OLR	1.289	.918	1.810	6,505
Would you say the government is pretty much run by a few big interests looking out for themselves or that it is run for the benefit of all the people?	0. Few big interests 1. Benefit of all	ML LR	1.191	.881	1.609	6,523
Do you think that people in the government waste a lot of money we pay in taxes, waste some of it, or don't waste very much of it?	1. A lot 2. Some 3. Not very much	ML OLR	.941	.697	1.272	6,712
Do you think that quite a few of the people running the government are crooked, not very many are, or do you think hardly any of them are crooked?	1. Quite a few 2. Not many 3. Hardly any	ML OLR	.938	.696	1.263	6,665

Note: Controlling for all variables present in Table 5.5a, Model 6
ML = Multilevel; OLR = Ordinal Logistic Regression; LR = Logistic Regression

CHAPTER 6

CONCLUSION

In this dissertation, I explored the ramifications of state political corruption on two factors of interest to sociologists and political scientists alike: voting and citizen trust in government. I examined whether a state's level of corruption as measured by PIN statistics had a significant negative effect on self-reported voting, in a direct test of what I call disengagement theory. I then tested whether that same corruption had a negative effect on political trust. Additionally, I explored and discussed both individual- and state-level independent variables as other potential factors in explaining these outcomes. I also explored the possibility that the amount of corruption in a given state had any influence on the effects of income, education, race and religious affiliation on self-reported voting and political trust.

In this chapter, I briefly recap the major findings from chapters four and five, and detail where these findings confirm, or differ from, previous research. Then, I present the limitations of the research. Finally, I discuss policy implications that come from this research as well as suggestions for future scholarly work. But first, I revisit the work that has come before in order to better contextualize what I find.

SECTION I: SITUATING THE RESEARCH

To begin, this dissertation fits in with the great deal of work that has already been done on political corruption in the United States and internationally, as well as the substantial bodies of research that discusses the effect of corruption on political participation. Teachout (2014) and Glaeser and Goldin (2004) document the unique history of corruption in the U.S. from the inception of the Republic to where we stand now. Political corruption, furthermore, is an issue that captivates the minds of not only the citizenry, but also scholars, as evidenced by the vast

amount of work done on it (e.g., Amundsen 1999; Glaeser and Goldin 2004; Rothstein and Uslaner 2005; Teachout 2014; Treisman 2007; Vogl 2012). Throughout this work, scholars have discovered that corruption often occurs in places with histories of corruption (Heywood and Rose 2014; Rothstein and Uslaner 2014); where there is systemic opportunity for corruption to occur in the structure of the government (Alt and Lassen 2003; Johnston 2005; Perrow 1984) when political players have close, personal ties to outside corporate and criminal influences (De Graaf and Huberts 2008; della Porta and Vanucci 2012); and where citizens pay little attention to those who lead them (Treisman 2007). Impacts of this corruption range from the benign or beneficial, like when citizens merely 'grease the wheels' of the political process (Lessoff and Connolly 2013; Merton 1968), to the much more malignant and severe, like when buildings crumble during earthquakes because corrupt officials allowed corporations to not follow proper building codes (Escaleras, Anabarci and Register 2007; Green and Ward 2004).

The effect of corruption on political participation in the U.S. has been to-date left less explored. Therefore, this dissertation bridges the work done on corruption with others' work on political participation (e.g., Alatas 1990; Leighley and Nagler 2014; Mishler and Rose 2001). First, I present three theories as to how political corruption might affect voting. The first, disengagement theory, argues that citizens will vote less in more corrupt places due to feelings of not being supported by the government and that their voice does not matter (Birch 2010; Chang and Chu 2006; Miles 2015). The second, mobilization theory, argues that citizens will vote *more* in more corrupt places, in order to oust corrupt elected officials (Kostadinova 2009; Praino, Stockemer and Moscardelli 2013). The third, voter acquisition theory, states that citizens will vote more in corrupt places, because politicians will try harder to encourage voter turnout so they

can reap the benefits of political corruption (Karahan, Coats and Shughart 2006, 2009; Stockemer, LaMontagne, and Scruggs 2013).

This dissertation also examines the effect of certain demographic characteristics on voting, paying particular attention to the effects of income, education, race and religious affiliation. This once again builds off of previous scholars' work. For instance, Leighley and Nagler (2014) present statistics that show that those with higher education and higher incomes are more likely to vote; Manza and Wright (2003) and Sherkat (2014) suggest religious affiliations can influence political participation at the voting booth; and Musgrove (2012) and Wilson (2012) document that blacks vote more than whites, controlling for other factors. What this dissertation adds to the conversation is a potential answer to the question of whether the effect that these variables have on voting is the same depending on the amount of corruption in the state.

In this dissertation, I also build off of the work done by scholars on the factors that affect citizen political trust. While there are two schools of thought on the determinants of political trust, the first that people are socialized into a trusting orientation early in their lives (Hetherington 1998; Mishler and Rose 2001; Uslaner 2002), and the second that political trust is a factor of a citizen's favorable or unfavorable view of the government (Catterburg and Moreno; Mishler and Rose 2001, 2005), I adopt the latter perspective in my hypothesis that political corruption will negatively affect trust in the government. Furthermore, this dissertation adds to the work of those who have found that one's education positively affects one's political trust and that the rich are more trusting than the poor (Anderson and Tverdova 2003; Rothstein and Uslaner 2005), that minorities are less trusting of the government than whites (Musgrove 2012; Omi and Winant 1994), and that religious affiliations influence political trust (Bean 2010;

Sherkat 2014). Once again, I ask whether the impact of state-level corruption influences the effects of income, education, race and religion.

SECTION II: DISCUSSION OF FINDINGS

Building off of this research and while conducting my analyses, I am led by the research questions which guide this paper. In the introduction, I asked: how does corruption affect voting? How does it affect political trust? Finally, how might the effect of state-level corruption itself influence how income, education, race, and religious affiliation influence voting and political trust? Now, I now turn to the discussion of each of my dependent variables, where I remind the reader of the central findings, but also provide rationales for why this is the case.

Voting

As shown in Chapter 4, I found a statistically significant negative effect of the political corruption rate on the odds of turning out to vote, though that effect is small: in fact, it has the smallest statistically significant effect on the odds of voting of all of the first- and level-2 characteristics that affect self-reported voting according to the predicted probability analysis. Nevertheless, I conclude that corruption does have a negative effect on voting, which supports disengagement theory and supports my first hypothesis. Simply put, people in states where the corruption rate is higher are less likely to vote than people in states where the rate is lower, controlling for many other factors.

Disengagement theory would suggest the reason for this is because citizens in more corrupt states feel like their voices are not being heard by politicians who may very well be conducting illicit operations while in office. Furthermore, since the measure of corruption here is not just the number of politicians prosecuted but overall corruption of local and state officials, citizens might believe that the entire political system is not worth participating in. Therefore,

they choose to stay home on Election Day. This dissertation makes the argument that, at least in the United States, disengagement theory is supported in that more corruption leads to lower turnout (Miles 2015). U.S. citizens who live in more corrupt states will turn out to vote less than those who live in less corrupt states, likely for the theoretically-established rationales of increased apathy and a decreased sense of efficacy in bringing about change.

This means that the two theories that predicted increased voter turnout, mobilization theory and voter acquisition theory, are not supported. For mobilization theory, which suggests that citizens vote more in order to "throw the bums out," it is important to understand the geographic area that developed that theory: newly minted democracies in Eastern Europe (Kostadinova 2009). The incipient nature of the democratic process there might explain why people felt like turning to the voting booths to oust corrupt officials would really work. Perhaps, countries without as long of a storied past of corrupt elected officials, or countries promising fresh changes in the relationship between the government and those governed might expect to see people really believe in those values and vote accordingly. This is not the case in the U.S.

On the other hand, this analysis also fails to support voter acquisition theory, which suggests political players will value corrupt seats and encourage voters to come out to vote (Karahan, Coats and Shughart 2006, 2009). This is somewhat surprising, because that theory was established using U.S. based research (Karahan, Coats and Shughart 2006, 2009). However, this theory might be better suited to understand the effect of corruption on much smaller elections, where citizens and their governors might know each other personally, and where those personal relationships might supersede the negative implications of a political scandal in an official's past. While, assuredly, corrupt state governors have allies, the majority of people who are eligible to

vote in midterm or presidential elections will not know corrupt elected officials personally, and those ties will be less salient.

Thus, disengagement theory best explains the effect of corruption on voting in large elections in a long-established democracy. Instead of corruption serving as a mobilizing agent, citizens might view it as so embedded in the fabric of their country that they see their votes are relatively meaningless. Because certain states are known for corruption more than others, and a state's corrupt legacy is relatively sticky in the minds of citizens, this does not bode well for the future of voter turnout and democracy in states like Illinois and Louisiana.

I also find significant effects of other variables on voter turnout, although that is not the main purpose of this analysis. Specifically, I find statistical significance for certain level-1 variables. Married people are more likely to vote than unmarried people. Age is positively associated with the likelihood of voting as well: for every year of age, an individual is slightly more likely to vote. Higher levels of religious service attendance and increased political interest also led to higher odds of reporting to have voted. Homeownership, union membership, and employment all increase the odds of voting. What many of these significant associations suggest is that individuals more connected to the social fabric of society are more likely to vote, perhaps because their civil attachments draw them towards this other element of civic engagement (Putnam 2000). Of course, it might also be that people vote not because of some intrinsic motivation, but because their social peer groups externally motivate them to show up on Election Day (Rolfe 2012).

My key demographic variables also affect self-reported voting, and they do so with the same effect size in more corrupt and less corrupt states. Anybody with less than a bachelor's degree is less likely to vote than an individual with an advanced degree. The higher one's

income, the more likely one is to vote. Blacks are more likely to vote than whites, whereas Hispanics and members of other races are all less likely to vote than whites. Finally, moderate Protestants, sectarian Protestants, Catholics, and the religiously unaffiliated are all less likely to vote than liberal Protestants. Once again, we can see that the factors that positively influence voting are connected to being engaged with the "status quo" of society more generally (Putnam 2000; Rolfe 2012). Whatever the case, this suggests what Leighley and Nagler (2014) also say: those more connected with society as it is (or those with potential power to change society in the way they see fit, like the wealthy or educational elite), are more likely to vote than those who are less supported by the status quo.

Only two variables that were not level-1, that of year and that of it being an election for the president, reached significance. First, as has been well-documented, people vote more in presidential election years, and this dissertation supports that. However, a novel contribution of this dissertation is that, controlling for many factors, we can see that the odds of voting for people in general go down with each year. While this effect is small, it indicates that as time goes on, people as a whole are voting less. For those interested in encouraging voter turnout, this might be troubling.

Finally, it is important to reiterate that I find significant effects of the level-1 variables of education, age, racial category, income, marital status, political interest, homeownership, union affiliation, and being employed on self-reported voting. Thus, this research also shows the importance of using certain level-1 variables in further analyses of voting and voter turnout. Smets and Van Ham (2013) show that there is a lack of well-established variables used in most studies of voting, and I would argue that the variables presented above should be included in later analyses, as a step towards defining a list of "usual suspects."

Trust in Government

Next, Chapter 5 detailed the examination of corruption's effect on individual trust in the government. In short, I fail to find evidence that a state's level of corruption influences an individual citizen's level of trust in the government. Further, I fail to find support for the idea that the effect of income, education, race or religion differs based on the level of corruption in the state.

This first finding is indeed curious and counters much international research on citizen trust scores and government corruption (Chang and Chu 2006; della Porta 2000; Vogl 2012), as well as theorists predicting that corruption reduces trust (Alatas 1990; Rothstein 2013; Uslaner 2008). This poses the idea that there might be a particular way U.S. citizens interpret political corruption. For instance, citizens might think of the government as a whole as corrupt or not (Teachout 2014), and pay little mind to the goings-on of their own states. If citizens do not pay much attention to state politics, or if they think that the federal government affects their lives more than the state government does, then they may not consider the type of corruption accounted for here in their assessments of government trustworthiness. This seems a more plausible explanation than the idea that Americans do not care about corruption in general in their political trust, given results from other researchers that suggest citizens of other countries hold less trust in corrupt governments.

What is supported, however, is the idea that both level-1 cultural and rational factors play a part in people's views of the trustworthiness of the government. Specifically, I found statistically significant positive relationships between measures of approving of the president and political trust as well as thinking one is better off than the year before and political trust. This reflects the idea that citizens rationally assess the government around them, and then decide to

trust the government based on those assessments (Mishler and Rose 2001). These relationships suggest that one's level of political trust is not a factor that exists independent of actions by the government.

However, I also find support for the socialized/individual factors approach as well, which suggests people have their views on the trustworthiness of the government based on their socialization and upbringing, and their own deep seeded beliefs about people in general (Mishler and Rose 2001). Specifically, trusting others in general is positively associated with trusting the government. Thus, it seems like these two views on what fosters government trust are not mutually exclusive: citizens can rationally assess the government in their decisions to trust, but that trust is also influenced by their own and ideas about others' trustworthiness more generally.

My key level-1 demographic variables also affect political trust, with the same effect size in more corrupt and less corrupt states, in somewhat surprising ways. High school graduates, those with some college, and those with a bachelor's degree or higher trust the government less than those with just an eighth grade education. Similarly, those who are in the bottom third of the income distribution trust the government *more* than those in the middle third. This suggests that citizens who are more embedded in the social system or status quo feel like the government is less trustworthy than those without such ties in terms of class and education. Also, blacks have higher trust scores than whites, as do Hispanics, controlling for other factors. Thus, unlike in the analysis of voting, we can see that the factors that positively influence trust are not connected to being engaged with the status quo. The only one of my key demographic variables that bucks this trend is that all Christian religious affiliations, members of minority religious traditions, and the religiously unaffiliated trust the government more than sectarian Protestants, which is the religious affiliation least connected to the status quo and "this-world" ideologies. The question,

which I turn to in the next section is, why is there this discrepancy wherein social connectivity in terms of race, income, and education leads to voting but not trust?

Before embarking on that analysis, however, it is important to note the two remaining variables that influence political trust, both in positive directions: year and average household income, a level-2 variable. For the latter, this means that individuals who live in states with higher average household income have higher trust scores. Average household income might reflect the level of economic productivity in a given state, and thus, this might indicate another area in which citizens could see a general level of state-government efficacy (in terms of raising the average wage) and adjust their views of state politics accordingly.

The positive effect of year is another interesting finding in this research. This analysis suggests that over time people have become *more* trusting of the government, controlling for other factors. Then why are we experiencing such high levels of government distrust, especially in more recent years (PEW Research Center 2015)? This research suggests that over time, political trust might be increasing if all other factors were held equal, but, importantly, not everything is. For instance, waning presidential approval ratings (especially those for Bush II in the lattermost years of his Presidency) and growing income equality might make it so that this small, positive effect is overtaken in aggregate levels of citizen trust. In other words, the reason we see trust scores decline is not inherent in some attribute of time – in fact, over time, trust scores increase – but instead that those factors that negatively influence trust exert a stronger influence on citizens, especially in more recent years.

In sum, there seems to be support for both cultural/individual and rational measures to affect citizen political trust. One of those measures, however, is not political corruption.

What Makes a Voter? What Makes a Truster? The Different Impacts of Demographic Characteristics on Voting and Government Trust

Having found support for one hypothesis, and failing to find support for others, this dissertation has done what it set out to do. But, it also introduces a new wrinkle in our understanding of the correlates of both voting and political trust. While findings from chapters four and five are not comparable in a direct sense, because of different sample sizes and different analytic strategies, it is useful to examine the characteristics that are of statistical significance in each analysis, in order to understand some larger social realities about demographic characteristics, trust, and voting. I do this by discussing which characteristics maintain their significance and direction in each ML analysis, which characteristics are significant in one analysis but not the other, and which relationships signify either greater trust and lower turnout or higher turnout and lower trust.

First, some demographics seem to not play a part in individual political trust, nor self-reported voting. For example, being a woman compared to being a man or belonging to Republican versus a Democrat does not affect one's odds of voting or one's trust score, controlling for other factors. While it might be of interest to see how members of different political parties or how men and women determine whether or not they will vote or how they go about deciding whether to trust the government, it seems as if neither factor would serve much purpose as control variables in future analyses, because their effects are nonsignificant here.

On the other hand, two relationships are statistically significant in both models and in the same, expected direction: all religious affiliation groups score higher on government trust than sectarian Protestants, net all, and liberal Protestants are more likely to vote than any other Christian religious affiliation and the religiously unaffiliated. In both cases, sectarian Protestants

trust and vote less than liberal Protestants. This might indicate that there is something unique about these two groups that affects their relative levels of political participation and political trust. Similarly, being black increases the odds of voting, compared to whites, as well as increases one's score of government trust, controlling for other factors. The latter point means that if black Americans are no different than whites in terms of their structural and social resources (that is, all of the variables controlled for in Chapter 5), they trust the government more than whites. This finding is indeed surprising, given the rich literature on reasons for black Americans to be *less* trusting of the government as a whole (Musgrove 2012; Wilkes 2015; Wilson 2012).

The most common finding in comparing these two analyses is that certain characteristics indicate higher or lower odds of voting *or* higher or lower government trust scores, but they only reach statistical significance in one of the analyses. For instance, being married has a positive effect on the odds of voting, ceteris paribus, but does nothing to increase or decrease the level of trust in the government. Controlling for other factors, being another race (that is not Hispanic or black) compared to white decreases the odds of voting, but does nothing to affect political trust. Both having a family member in a union and being employed increase the odds of voting but do nothing to alter political trust, net all. There are many more such relationships in this analysis, but the key point is that factors that affect self-reported voting and citizen trust are not always the same. What seems to occur is that the demographic characteristics that signify rootedness in the social fabric of society (like weekly church attendance) also contribute to one's likelihood of engaging in other actions that signify making a change in society in a socially acceptable way (i.e., voting), but do not necessarily change one's feelings of trustworthiness of the government.

This point is made particularly clear when seeing where such significant results actually differ (i.e., statistically significantly predicting higher probability in one analysis and lower probability in another). For instance, being an older citizen or homeowner decreases one's trust in the government, but actually increases the odds of turning out to vote, net all. Similarly, those with the least education (high school dropouts) trust the government more than those with higher levels of education, although education increases the likelihood of voting, all else being equal. Members of the lowest class category report trusting the government more than middle class citizens (who are statistically indistinguishable from upper class citizens in their political trust scores), although higher income increases the odds of voting. Controlling for other factors, being Hispanic compared to white increases an individual's trust score by almost six points, but reduces the odds of turning out to vote by a small amount. Thus, it is abundantly clear that political trust should not be considered synonymous with the idea that citizens will turn out to vote. What's more, in results not shown here for the sake of space and small sample size, with government trust included as a factor in the analysis of the correlates of self-reported voting, the effect of political trust had no effect on the odds of voting, controlling for other factors.

Instead, these conflicting results are certainly suggestive of a phenomenon that might be occurring in American politics: voter turnout is not indicative of a more trusting population. In fact, in some instances, a lack of trust might fuel one's interest in impacting the government through voting (Kostadinova 2009), as might specifically be the case for homeowners and older Americans. These results instead lead me to interpret voting as an embedded practice for citizens – they do or do not vote, for various reasons, but not based on feelings that the government is trustworthy. Scholars who suggest that countries that experience higher levels of voter turnout

are also made up of a more trusting citizenry should take heed of this U.S. example, where this does not seem to be the case.

SECTION III: LIMITATIONS

Like all studies, this dissertation is not without its limitations. I first point out the limitations that come from the data sources itself, and then discuss the limitations with the methodology I used.

First, both NES data and PIN official statistics should not be used without a critical understanding of what they do and do not tell us. For NES data, more for voting measures, and less so for trust, it is prudent to be concerned with how much people are telling the truth. There is a long established literature on over-reporting one's voting habits, and this NES data is no different in terms of citizens over-reporting, as shown by the discrepancies between semi-official voter turnout scores and the ones in this dataset in Figure 3.1. Thus, it bears repeating that this study examines *self-reported* voting, and cannot explicitly speak to *actual* voting.

In addition to problems with this measure of voting, there are also issues with the PIN measure of government corruption. Once again, I examine the effects of political corruption *prosecutions* on self-reported voting and trust in the government. As previously mentioned, there are definitely some gaps in what the PIN measures, specifically with reference to corruption that goes on behind closed doors. Furthermore, with recent developments in how much money can go into political campaigns, and the amount of money in politics in general (Teachout 2014), many citizens may think the system itself is corrupt, even if the things politicians are doing are technically aboveboard. Moreover, this trouble might occur for PIN reports heading into the future: while certainly PIN statistics missed certain behind-closed-doors corruption in the past, it was not until the late 2000s that we saw the rise in SuperPACs, money-as-free-speech, and

Supreme Court rulings that made what many suggest should be illegal (and that the founders of the constitution definitely tried to outlaw) legal indeed (Teachout 2014) in terms of the amount of influence lobbyists and corporations can exert over politics.

These are major impediments to using official PIN statistics today, and will only continue to be problematic in the future. If voters are concerned with corruption that is not assessed through this measure, then the null finding in Chapter 5 which suggests that citizens do not consider corruption when trusting the government might be wrong. Citizens might really care about corruption, just not the way that it is measured using PIN statistics. Furthermore, State Attorneys and other prosecutors may be more attuned and prepared to enforce federal laws in states with higher corruption rates, even if this type of corruption is not the most devastating as interpreted by citizens, which might serve to inflate the rates for states that have been perceived as corrupt in the past: in a way, a self-fulfilling prophecy.

Nevertheless, the most scandalous corruption cases have still been documented in these PIN reports, which are probably the ones with which citizens are most familiar. However, the fact is that these rates also include small-scale and mid-level corruption prosecutions, and these cases are given as much weight as a high profile case in the scoring itself. Thus, it might just be that the measure does not tap into citizen perceptions of corruption. Unfortunately, there is no way to gauge a citizen's perception of corruption in this data: NES does not include a measure of perceived corruption in one's own state, but merely asks questions about one's general level of trust in the government as a whole; questions that, as Chapter 5 shows, seem to have little to do with state-level corruption.

This leads to the acknowledgment that another limitation of this research project is that I cannot adequately measure one's knowledge of corruption. In fact, NES does ask a question

related to knowledge of newspapers and political campaigns, but this question is only asked in certain presidential election years, and then to only certain respondents, and thus would drastically limit my sample size. Nevertheless, it is important to note that a limitation of this work is that while I can only measure the prevalence of corruption prosecutions in my analysis, the influence of corruption would only plausibly occur when citizens know about it. While this is somewhat of a logical leap – in that we cannot really know how well-informed people are just because of their citizenship in a state, as mentioned before, there is reason to believe that citizens are indeed well-aware of the amount of corruption in their states (Boylan and Long 2003; Goel and Nelson 2011; Peters and Welch 1978).

Another limitation of this dissertation is that I cannot tap into the underlying mechanisms behind why citizens of different groups report different voting and trust levels. Thus, for example, while this research shows that members of all religious affiliations (including the religiously unaffiliated) are more trusting than sectarian Protestants, this analysis cannot measure *why* that is the case in terms of the actual influence that sectarian Protestantism has on its adherents.

Finally, as mentioned in the methods section, there is still work to be done to grapple with some of the statistical intricacies of MLM. There might be further statistical developments, especially concerning weighting, that will be better implemented in later analyses. What can be said at this point is that these results are robust to both MLM and standard logistic and linear regression. And, even if future techniques of modeling ML time-series cross-sectional data like this one become available, as shown by the quite small state- and year-level variance for both analyses, there is reason to suspect that these findings will hold.

SECTION IV: POLICY IMPLICATIONS

Next, I offer suggestions for policy-makers to encourage voter turnout and citizen trust in the government, in line with the findings presented above. In addition to offering tangible and actionable policy implications, I also discuss potential policy implications that would affect voting and trust in a more distal manner, though they would still be supported in some way or another by the findings above.

Voting

First, and in line with Hypothesis 1, these results suggest that reducing state-level corruption will have a small but significant effect on encouraging (reported) voting. Here, we can see a definite policy implication for politicians: to increase voting, reduce corrupt practices, or put checks and balances into play that we know make corruption harder to engage in. Other scholars have offered a wide array of suggestions to this regard. For example, state governments could institute more transparent government practices, and citizens without strong relationships to fellow politicians and corporate leaders should be encouraged to run for political office (Ghiloni 1987). Third-party watchdog groups could oversee state governments and other political operators, and there should be independent audits of such organizations to encourage rule-abiding in all government employees (Vogl 2012). In short, we should take steps that encourage a less corrupt government, not only because of the multitude of reasons discussed in Chapter 2 of this dissertation, but also because we do see it negatively affects self-reported voting.

While this was not the major focus of this dissertation, I also found that certain demographic characteristics encourage citizens' reported voting. More distally, policy-makers might encourage certain behaviors or actions to increase turnout based on these characteristics as well. For instance, more tax breaks might make marriage a more realistic possibility for citizens,

or the government could make homeownership more easily accessible for first-time buyers with the knowledge that both of these factors increase one's odds of reporting to have voted. Other factors that indicated a well-functioning economy also are linked to higher odds of turning out to vote (e.g., an individual's income), and a latent consequence of governmental policies that encourage economic success might just be seeing more people at the voting booth.

Government Trust

Unlike in the analysis of self-reported voting, I did not find support for the primary hypothesis, and therefore do not show that reducing overall corruption will positively impact citizen trust in the government. This is somewhat disquieting, because we are left with the idea that corruption is not the biggest issue on citizens' minds when they make their decisions about the trustworthiness of the government. Even in the bivariate case, the correlation between corruption rate and government trust does not reach statistical significance, and no effect emerges when controlling for other potential correlates. Therefore, this dissertation cannot suggest reducing corruption would increase political trust.

Instead, this research suggests that the factors most salient to encourage citizen trust seem to be beyond the scope of government intervention. Particularly, one's level of social trust in others significantly affects one's level of trust in the government. On the outset, there does not seem to be much the government can do to increase such generalized social trust. Governments may, however, encourage opportunities for citizens to form such bonds by making it easier for citizens to join civic groups, perhaps by offering tax breaks for volunteerism in time in much same way they allow tax breaks for charitable monetary donations. Once again, this would be a very roundabout way to encourage political trust, although it may have other positive latent consequences for community life in general (Putnam 2000).

Another significant factor in encouraging citizen trust is whether a respondent approves of the president's policies and administration. In a two-party system, however, there seems to be increasingly less approval of a president if one is not part of the President's political party. Thus, administrations attempting to sway citizens by increasing their approval ratings through new policies and by meaningfully changing laws might only increase approval in a portion of the population, and might decrease their approval in those of the other party.

However, citizens that think they are at least as well off as they were the year before are more trusting than those who believe that they are worse off. Thus, if policies were implemented that encourage more citizen wealth accumulation, perhaps by lowering taxes or encouraging the type of economic growth that leads to more well-paying jobs, middle-class citizens might respond by placing more trust in the government. For instance, while this is somewhat speculative, one step could be by reducing property taxes, which might 1) encourage more wealth accumulation and thus increase the likelihood of thinking one is better off than the year before and 2) also reduce the negative impact of being a homeowner on citizen trust.

In short, because citizens seem to base their trust in the government on economic issues, the government would do well in taxing citizens less, or encouraging economic growth for the middle-class. There are certainly other attributes connected to trust in the government that policy-makers cannot nor should influence (e.g., one's religious affiliation and interpretation of the Bible), but actively maintaining or increasing citizen quality of life might be the best tactic for the government to take in encouraging citizen political trust.

SECTION V: FUTURE RESEARCH

To conclude, I now discuss some ideas for future research based off of the work laid out above. Specifically, I advocate for the following: 1) an aggregate-level quantitative model

utilizing a better-articulated model of MLM interpretation and theoriziation, 2) a qualitative research design to investigate the themes established above, 3) running a similar analysis, but with the effects of corruption lagged, 4) understanding the effect of big scandals on citizen voting and trust, and, finally, 5) developing more robust and valid measures of corruption.

First, investigators should employ a macro-level, quantitative analysis to see if political corruption affects voter turnout rates in the aggregate. The results herein suggest that the answer should be yes, because corruption reduces likelihood of voting. However, whether this actually translates to aggregate levels of corruption remains to be explored. For example, this research suggests that individual household income increases the likelihood of reporting to have voted, while average household income of a state has no effect on the likelihood of voting. This suggests that there are some unique relationships that occur at the level-1 that are different from similar measures occurring at level-2. This raises the question: how would aggregate measures of level-1 factors, (e.g., percentage of a state with union membership or Christian identification) influence those individuals individually and in the aggregate? There might be something unique about characteristics that affect aggregate levels of turnout, but not individual likelihood of voting, or vice versa.

To that end, quantitative scholars would do well to establish more theories and methodologies to discuss multilevel modeling. Currently, there is little articulation in the literature about how and when aggregate and state-level factors affect individuals beyond the effect of their individual-level characteristics. As mentioned, results from this dissertation suggest individual- and aggregate-levels of similar variables do not always affect the same independent variable in the same way (e.g., individual household income led to higher odds of

self-reported voting while there was no effect of state-level average household income), and scholars should theorize over the best way to think about and interpret these multilevel problems.

Second, researchers might do well to employ in-depth interviews to assess why citizens vote and trust, and what cultural or structural factors influence their voting habits and their perceptions of government trustworthiness. Perhaps even more useful, future researchers should examine why those who do not vote fail to engage in the political process, building off of the work of Piven and Cloward (1988) and Teixeira (2011). This might be a particularly rich area of analysis: self-reported non-voters buck the trend of over-reporting one's voting, and they may be able to point out elements of the political process that discouraged their voting that researchers have left unquestioned, because of our focus on understanding the factors that *lead* to more voting. Also through in-depth interviews, future researchers should conduct qualitative work that explicitly asks members of different religious backgrounds their reasons for voting or not voting. Only through that method might we tap into the given reasons and the principles these individuals adopt when deciding their particular political activities.

The third area of future research would be to test whether the impact of corruption changes over time. Recall, a state's political corruption rate is somewhat "sticky" – the most and least corrupt states seldom move much in their overall corruption rates over time. While corruption rates are relatively stable, it is not beyond the realm of possibility that a particularly scandalous case of corruption might involve enough political players in a certain year to leave a bad taste in the mouth of potential voters: in that case, we should see fewer citizens at the next election. Future researchers could determine whether the effect of corruption might change with time, or whether there were certain years where the effect of corruption was especially salient.

More specifically, it would be worthwhile to run interaction effects to see if the effect of

corruption changes over time to answer certain questions. For instance, might it be the case that political corruption had more influence in the 1980s than it does now? Using this alternative specification, future researchers could determine the lagged effects of corruption, and how those effects might influence turnout and trust. Running lagged effect models would help us see exactly how much time it takes for the effect of corruption to be at its most salient for citizens and voters: the next election? Ten years down the line? While this dissertation provides a starting point, future scholars could use this as a baseline for even more sophisticated models.

Fourth, and relatedly, researchers could conduct a historical analysis of newspapers for each state to determine how large political scandals affected voter turnout in the immediately following years. This would advance our knowledge beyond assessing corruption with PIN reports, yet it would also be a massive data collection effort. Nevertheless, by examining front-page headlines from the two or three best-selling newspapers per state, researchers could develop a rough indicator of the number of "big scandals" per year, per state. For instance, the imprisonment of half of Illinois' governors (four out of the last eight) most likely affects citizen perceptions of the government more than ten county clerks skimming money from the Department of Licensing in Washington. Future researchers could examine front-page newspaper coverage to determine the amount of attention paid by the mainstream media to a particular case, and examine whether these well-covered cases influence citizen trust and voting, and perhaps compare the effects to the corruption rate used in this dissertation.

Finally, a major concern for scholars who use PIN reports is the fact that what the PIN investigates might not even be what the public or even academics consider to be the forms of corruption that are most systematically damaging to voting and trust. In fact, future PIN reports will only move farther away from discussing the types of corruption with which most people are

concerned, because the data investigated here represent a time that will not exist in the future: the time period between when the PIN started reporting their statistics to the general public and the Supreme Court's ruling in the *Citizens United* case (in 2010) that legally redefined corruption solely and explicitly as *quid pro quo* deals. This ruling, and several other recent rulings like it, essentially made backroom dealings legal, as long as they are not explicitly discussed by politicians and influencers in terms of mutual beneficence. With this definition, the utility of the PIN's corruption reports will be even more limited in the future.

In fact, I speculate that the reason that corruption seems to have no effect on government trust is that citizens are relatively unconcerned with many of the cases of political corruption that are reported by the PIN, and are more troubled about the legal ways in which the system is manipulated by special interests. Future research is needed to test this hypothesis. What are the most important influences on citizens' own perceptions of what impacts their level of trust? As this dissertation suggest, it does not seem to be due to objective levels of state corruption prosecutions. Constructing a dataset that asks citizens what they care most about when they think of what makes a government trustworthy would be a suitable next step in this research agenda.

Thus, future researchers might attempt to develop a new measure of political corruption that taps into what people care about the most, which would plausibly be more reflective of citizens' trust scores. To that end, future researchers could survey citizens nation-wide on their perceptions of corruption in their states, and test whether these assessments correspond with PIN corruption rates. Furthermore, NES might include a question directly asking citizens how much political corruption they believe occurs in their state, relative to other states, in future waves of the survey. While this will not allow for comparability to the past fifty-or-so years of the survey, it will allow future scholars a place to begin their assessments. Finally, scholars could fall in line

with Boylan and Long (2003) and attempt to construct a measure of political corruption using expert opinion from statehouse reporters. In sum, the time of using PIN reports as suitable measures of legally-defined corruption may have passed, and this dissertation presents results found right at the tail end of that period.

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APPENDICES

APPENDIX A

RELIGIOUS AFFILIATIONS CODING SCHEME

[Sherkat's (2014) Coding Schemes in Brackets]
Code as given by NES as follows

1. LIBERAL PROTESTANTS
    a. [Liberal Protestants]
    Congregational
        United Church of Christ
        Congregational Christian
    Presbyterian
        Presbyterian Church in the USA
        Cumberland Presbyterian Church
        Presbyterian Church in American
        Evangelical Presbyterian
        Reformed Presbyterian
        Presbyterian
    b. [Episcopalians]
    Anglican/Episcopalian
    Independent Anglican/Episcopalian
    c. [Unitarians]
    Unitarian

2. MODERATE PROTESTANTS
    a. [Moderate Protestants]
    Methodist
        United Methodist Church, Evangelical United Brethren
        African Methodist Episcopal Church
        African Methodist Episcopal Zion Church
        Christian Methodist Episcopal Church
        Primitive Methodist
    Reformed
        Christian Reformed Church
        Reformed Church in America
        Free Hungarian Reformed Church
    Restorationist
        Christian Church (Disciples of Christ)
        Christian Churches and Churches of Christ
        Christian Congregation
    b. [Lutherans]
    Lutheran
    Evangelical Lutheran Church in America
    c. [Baptists]
    American Baptist Association

American Baptist Churches U.S.A. (inaccurately known as "Northern Baptist")
Baptist Bible Fellowship
Baptist General Conference
Baptist Missionary Association of America
Conservative Baptist Association of America
General Association of Regular Baptist Churches (G.A.R.B.)
National Association of Free Will Baptists (United Free Will Baptist Church)
Primitive Baptists
National Baptist Convention in the U.S.A.*
National Baptist Convention of America*
National Primitive Baptist Convention of the U.S.A.*
Progressive National Baptist Convention*
United Free-Will Baptist Church [1990 only]
Reformed Baptist (Calvinist)
Southern Baptist Convention
Fundamental Baptist (no denom. ties)
Local (independent) Baptist churches with no denominational ties or links to a national fellowship

3. SECTARIAN PROTESTANTS
   a. [Sectarian Protestants]
      Adventist
         7th Day Adventist
         Fundamentalist Adventists (Worldwide Church of God)
         Adventist
      United Missionary; Protestant Missionary
      Non-Traditional Protestants
         Christian Scientist
         Spiritualist
         Jehovah's Witnesses
         Unity, Unity Church, Christ Church Unity
         Fundamentalist Adventist (Worldwide Church of God)
         Religious Science, Science of Mind (Not Scientology or Christian Science)
         Non-Traditional Protestant
      Pentecostal
         Assemblies of God
         Church of God (Cleveland, TN)
         Church of God (Huntsville, AL)
         International Church of the Four Square Gospel
         Pentecostal Church of God
         Pentecostal Holiness Church
         United Pentecostal Church International
         Church of God in Christ
         Church of God in Christ International
         Church of God of the Apostolic Faith

Church of God of the Prophecy
Vineyard Fellowship
Open Bible Standard Churches
Full Gospel
Apostolic Pentecostal
Spanish Pentecostal
Pentecostal

Independent-Fundamentalist
Plymouth Brethren
Independent Fundamentalist Churches of America
Independent Fundamentalist

Holiness
Christian and Missionary Alliance
Church of God (Anderson, IN)
Church of the Nazarene
Free Methodist Church
Salvation Army
Wesleyan Church
Church of God of Findlay, OH
Holiness

European Free Church (Anabaptists)
Church of the Brethren
Brethren
Mennonite
Moravian
Old Order Amish
Quakers
Evangelical Covenant Church
Evangelical Free Church
Brethren in Christ
Mennonite Brethren

Fundamentalist Lutheran
Lutheran Missouri Synod
Lutheran Wisconsin Evangelical Lutheran Synod
Other Conservative Lutheran
Lutheran Free Church

Other Fundamentalist

b. [Mormons]
Mormons, Latter Day Saints

c. [Other Protestants]
Protestant, no denomination given
Non-denominational Protestant
Community Church
Inter-denominational Protestant
Christian – "Just Christian"

4. CATHOLICS
    a. [Catholics]
    Roman Catholic

5. MINORITY RELIGIOUS TRADITIONS
    a. [Jews]
    Jewish, no preference
    Orthodox
    Conservative
    Reform
    b. [Other Religions]
    Muslim, Islam
    Buddhist
    Hindu
    Bahai
    American Indian Religions
    New Age
    Wicca
    Pagan
    Other non-Christian/non-Jewish
    Scientology
    Religious/ethical cults
    More than one major religion (Christian, Jewish, Muslim)
    Eastern Orthodox
        Greek Rite Catholic
        Greek Orthodox
        Russian Orthodox
        Rumanian Orthodox
        Serbian Orthodox
        Armenian Orthodox
        Georgian Orthodox
        Ukrainian Orthodox
        Eastern Orthodox

6. UNAFFILIATED
    a. [Unaffiliated]
    Atheists
    Agnostics
    Other

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Major Professor: Rachel Whaley